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EXPERIMENTS ON ALTERNATE CURRENT ARCS BY AID OF OSCILLOGRAPHS.[1]

By W. DUDDELL, Wh. Sc., Associate, and E. W. MARCHANT, B. Sc., Associate.

A SHORT time after Mr. Frith's paper was read before the Physical Society in 1896 on the "Effect of Wave Form on the Alternate Current Arc," he suggested to the late Mr. Ray and one of the authors that they should investigate whether the reactions of the arc on the wave form of an alternator, to which he had called attention, would still occur if a transformer were used between the alternator and the arc. Mr. Ray and one of the authors started experiments to settle this point; but owing to the death of Mr. Ray they were never completed, and the present authors took up the subject.

We, however, no longer confined ourselves to this one point, but extended our investigations to an examination of the reactions of the arc on the alternate current wave form in general. The results of some of these experiments are embodied in this paper.

It is well known that, if a non-inductive resistance between two points in any alternate current circuit be replaced by an arc, the waves of the current and of the P.D. between these points will be altered even though the arc transmits the same root-mean-square current at the same P.D., and that this alteration depends on the kind of carbons employed, their hardness, &c., and the nature of the rest of the circuit.

[1] This paper will be discussed at a meeting, the date of which will be announced hereafter.

Historical.

The first person to notice the reaction of an arc on the alternate current wave form was probably M. Joubert:[1] who, writing in 1880, says: "This experiment gave me for the current a curve almost rigorously sinusoidal except for a slight displacement of the maximum in the direction of motion"; and in the same article he says: "At the moment when the current is zero in the circuit, the difference of potential between the two carbons is also zero; but in an inappreciable time this difference of potential attains a value of 40 to 45 volts, which value it keeps almost without variation until the current again attains a very small value. The final drop in the curve is very sudden, but I have nevertheless been able to follow it in its details. I was not able, however, to do as much for the rise at the commencement, which seems to occur almost in an instantaneous manner."

Two years later MM. Jamin and Maneuvrier,[2] whilst experimenting on the alternating arc between dissimilar electrodes, noticed that, when the electrodes were of the same material and of different sizes, the current was larger when it flowed from the larger electrode to the smaller, than when it flowed in the opposite direction. They also noticed when using one electrode of metal and one of carbon that the current was larger when it flowed from the former to the latter, and they suggested that these methods might be used for rectifying alternate currents.

In 1884, the late Dr. Hopkinson,[3] in his admirable paper on alternate currents before this Institution, worked out a mathematical equation for the flow of current in the alternate arc on the assumption that the arc possessed a constant E.M.F. which opposed the current, and which vanished with it.

The results of the investigations of Messrs. Tobey and Walbridge[4] on the wave form of a Stanley alternate current arc dynamo when supplying from 1 to 40 arcs were published in 1890. This paper is of interest as it gives what we believe to be the first published wave form of a single hand-regulated arc, which shows the characteristic reaction of high voltage carbons.

[1] *Journal de Physique*, 1880, vol. ix. p. 297.
[2] *Comptes Rendus*, 1882, vol. xciv. p. 1,615.
[3] *Proceedings of the Institution of Electrical Engineers*, 1884.
[4] *American Institute of Electrical Engineers*, vol. vii. p. 367.

In 1891 two important papers appeared on the periodic variation of the luminous intensity of the alternate current arc; the first described the experiments of Mr. McMynn [1] and the second was from the pen of M. Blondel.[2] A paper by Professor E. L. Nichols, describing the experiments of Messrs. Archbold and Teeple[3] on the alternate current arc between a point and a plate is also of considerable interest, as it gives the wave forms which show very clearly the curious unilateral nature of this arc. The low power factor of the arc was pointed out in this year by Professor Ayrton and Dr. Sumpner,[4] who compared the arc to a self induction; this power factor was also the subject of investigation by Mr. Steinmetz[5] in the autumn of 1892.

In April, 1892, M. Blondel[6] presented to the Société Française de Physique the first of the series of papers which he has written on the subject of the reaction of an arc on the wave form and which contain almost all the results that have been published on the subject up to the present time. We shall make frequent reference to these papers in the course of our communication.

The second and main paper[7] of the series appeared in 1893, and the third[8] and last in 1895.

In 1894 Messrs. Rössler and Wedding[9] investigated experimentally the effect of wave form on the efficiency of the arc and came to the conclusion that a flat-topped current wave form was more efficient than a pointed one; and this was subsequently confirmed by Mr. Görges,[10] who also investigated the periodic curves of light emitted by the arc.

Dr. Sahulka[11] pointed out that when an alternate arc burns between a metal and carbon there seems to exist a difference of potential in a constant direction between the

[1] *American Institute of Electrical Engineers*, 1891, vol. viii. p. 214.
[2] *La Lumière Électrique*, 1891, vols. xlii. pp. 551, 618, and xliii. p. 51.
[3] *American Journal of Science*, 1891, vol. xli. p. 1.
[4] *Proceedings of the Royal Society*, vol. xlix. p. 424.
[5] *Electrotechnische Zeitschrift*, 1892, pp. 460 and 567.
[6] Abstracted in *La Lumière Électrique*, 1892, vol. xliv. p. 135.
[7] *La Lumière Électrique*, 1893, vol. xlix. pp. 501, 557, 608. Abstracted in the *Electrician*, vol. xxxii. p. 161.
[8] *L'Industrie Électrique*, August 10, 1895.
[9] *Electrotechnische Zeitschrift*, 1894, vol. xv. p. 315. Also in the *Electrician*, vol. xxxiii. pp. 523, 539.
[10] *Electrotechnische Zeitschrift*, 1895, vol. xvi. p. 548, and in the *Electrician*, vol. xxxvi. p. 230.
[11] *L'Eclairage Électrique*, 1894, vol. i. p. 474; and a later paper, *Zeitschrift für Electrotechnik*, 1898, p. 213.

electrodes, and also that if a search carbon is introduced into an alternate arc between carbon electrodes, and a high resistance galvanometer be connected between it and either carbon, there appears to be a P.D. between the search carbon and the main carbon such that both main carbons are positive with respect to the search carbon.

In 1896 Professor Fleming and Mr. Petavel[1] in their paper on "An Analytical Study of the Alternate Current Arc" give some very interesting sets of periodic curves comprising P.D., current, power and light, which show how very little time lag there is between the light given out by the arc and the current through it; they seemed to have used cored carbons, so no great reaction on the wave form took place. Arcs between metal and carbons were again examined in 1896 by Mr. Arons,[2] who found, as had been previously shown by MM. Jamin and Maneuvrier, that the current is greatest when the metal is positive, and gave some reasons in explanation. The reaction of the arc on the wave form was next described by Mr. Frith,[3] who pointed out that it probably accounted for the different efficiencies of arcs as tested in the laboratory and as used in practice; and it was due to some suggestions made by Mr. Frith after the reading of this paper that the present experiments were undertaken.

Mr. Burnie[4] in his paper on "The Factors which Determine the Efficiency of the Alternating Current Arc" gives periodic curves of both light and heat radiated by the arc with the corresponding P.D., current, and power wave forms, and shows clearly how the current wave form affects the efficiency. He concludes by showing that a square current wave with a front peak should give high efficiency.

In concluding our historical reference, we wish to draw the attention of those interested in the alternate current arc to the very important paper by MM. Blondel and Jigouzo[5] on the "Luminous Efficiency of the Alternate Current Arc."

That a large amount of work has been done on the alternate current arc and its reaction will be gathered from the references which we have given to some of the principal

[1] *Proceedings of the Physical Society*, 1896, vol. xiv. p. 115.
[2] *Annalen der Physik und Chemie* (*Wiedemann*), 1896, vol. lvii. p. 185.
[3] *Proceedings of the Physical Society*, 1896, vol. xiv. p. 245.
[4] *The Electrician*, 1897, vol. xxxix. p. 849.
[5] *The Electrical World* of New York, 1897, vol. xxix. pp. 232, 258.

papers on the subject, nevertheless there still remained a vast amount more to be done before we could fully understand the intricacies of its reactions on the wave forms.

Wave Forms and Observations Required.

In an investigation like the present into the reactions of the alternate current arc on the wave form, we in general require three wave forms, viz. :—

1. The form of the wave of potential difference between the terminals of the dynamo when the arc is burning (which we will call for short the "P.D. dynamo" curve).

2. The form of the wave of potential difference between the terminals of the arc (which we will call the "P.D. arc" curve).

3. The form of the wave of current (which we will call the "current" curve).

We also require the root-mean-square values of the above waves, the power supplied to the arc, the arc length, and the data of the circuit in which the arc is burning.

As the wave forms are the most important part of our experiments, and as the value of the results obtained depends in a large measure on the suitability of the method chosen to delineate the wave forms, we will mention some of the considerations which affect the choice of the method.

Considerations affecting the Choice of a Method of Obtaining the Wave Forms.

Owing to the sensibility of the arc to draughts, impurities in the carbons, movement of the crater, and even to noise,[1] its reaction on the wave form does not remain constant, so that generally one wave differs slightly from the next. We thus have in many cases a series of dissimilar waves for each electrical quantity, and this dissimilarity is most marked in the wave of P.D. between the arc terminals.

Accurately speaking, therefore, in such cases we have no right to call the shape of any "*one* wave" "*the* wave form"; but in most steady states of the arc the similarity between

[1] H. T. Simon, *Annalen der Physik und Chemie*, 1898, vol. lxiv. p. 233.

successive waves is amply sufficient to justify us in taking one wave as typical of the rest.

This want of similarity between the successive waves is liable to introduce errors in the results obtained by those methods in which the wave form is built up point by point, each point being obtained by taking a mean value of a large number of ordinates having the same angular position in the period of a series of perhaps slightly different waves; because in these methods the successive points usually belong to separate series of waves and not to the same series.

If the time which elapses between obtaining the successive points is long, the final wave form may be inaccurate although the position of each point has been found with great precision. There is also a practical difficulty introduced by this want of identity between successive waves, for it sometimes produces, with the instruments used in these methods, considerable instability in their deflections, especially near the zero points of the curves. For the above reasons it is preferable, and in many cases absolutely necessary, that the corresponding wave forms of the different electrical quantities, *i.e.*, P.D., current, &c., should be obtained simultaneously.

Many of the most interesting phenomena of the arc and those on which we require additional information are of a more or less variable or transient nature, such as the hissing arc, and arcs between electrodes other than carbons, consequently the waves only repeat themselves a very limited number of times, if at all.

These considerations led us to choose a method in which the time taken in recording each wave is as small as possible, and by which the several P.D. and current wave forms can be obtained simultaneously.

Methods used by Previous Investigators and by Ourselves.

Many previous observers have used some form of Joubert contact method which required a considerable time to delineate each wave; and many of them have obtained the waves of P.D. and current one after the other. M. Blondel, however, employed a most ingenious modification of the contact method by which he was able to delineate his wave forms in as short a time as one or two minutes, and he

obtained both P.D. and current waves simultaneously; also for some of his curves he used an oscillograph.

We commenced our investigations by the point method; but after working with it for nearly three months we came to the conclusion that our investigations would never be completed unless some quicker and more accurate method was devised for obtaining the curves. From the considerations already given we concluded that we required a method by which we could not only examine each individual wave, but also record simultaneously the P.D. dynamo, P.D. arc, and current wave forms.

One of us therefore considered the subject of curve tracers, and oscillographs, and more especially the methods which had been suggested by M. Blondel and the methods on which experiments had been carried out by Professor Ayrton and Mr. Mather; the result was that two oscillographs[1] were devised and constructed which were used in all our subsequent experiments.

An oscillograph is essentially a galvanometer whose deflection at any moment is sensibly proportional to the instantaneous value of the current flowing through it at that moment; so that if an alternate current flows through it, the deflection is continuously proportional to the ordinates of the wave form of that alternate current, and by forming a continuous record of the deflection we obtain the wave form.

In order that such an instrument may be accurate and that it may be conveniently used either as an ammeter or a voltmeter, it ought to fulfil the following conditions:—

1. Very short free periodic time compared to the period of the wave forms being recorded.

2. Critical damping, that is the motion just ceases to be oscillatory.

3. Negligible self-induction.

4. Sufficient sensibility.

Many attempts have been made to fulfil these conditions; but we venture to think that no preceding instruments have been so well adapted for our purpose as the oscillograph described below.

[1] A description of the principle of these instruments was given in a paper on "Oscillographs" at the British Association, Toronto meeting; see the *Electrician*, 1897, vol. xxxix. p. 637.

DESCRIPTION OF OSCILLOGRAPHS USED.

Fig. 1 is a diagrammatic view of the instrument to show the principle on which it works. In the narrow gap between the poles, N. S., of an electromagnet are stretched two parallel conductors, *s. s.*, formed by bending a metal strip back on itself over the pulley, P. At the bottom ends the strips are clamped on a block, K, and at the top press against a bridge piece, L. The current to be measured flows up

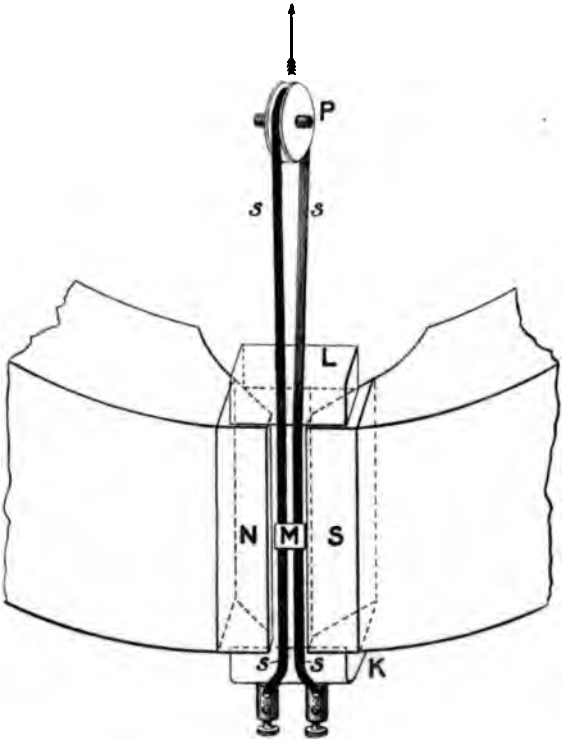


FIG. 1.

one strip and down the other, thus causing one to move forward and the other back, and turning the mirror, M, fixed to their centres through an angle which, if small, is proportional to the current. The necessary damping is provided by immersing the strips in oil contained in a chamber of which the sides are formed by the pole pieces, the back by a brass plate and the front by a lens.

To observe the motion of the mirror, M, the spot of light reflected from it on a screen was examined in a rota-

FIG. 2.

ting mirror, and to obtain a permanent record, the motion of the spot was photographed on rapidly moving photo-

graphic plates, from which the illustrations to this paper are reproduced half-size.

In our experiments two oscillographs were generally used, one having a single vibrating system as just described; and the other having two independent vibrating systems, each in its own gap, in the magnetic circuit. Fig. 2 is a general view of the latter, and Fig. 3 shows the same instrument, with the front containing the lens removed; this front is standing on the base of the instrument towards the left-hand side; the small glass tube connected to the front

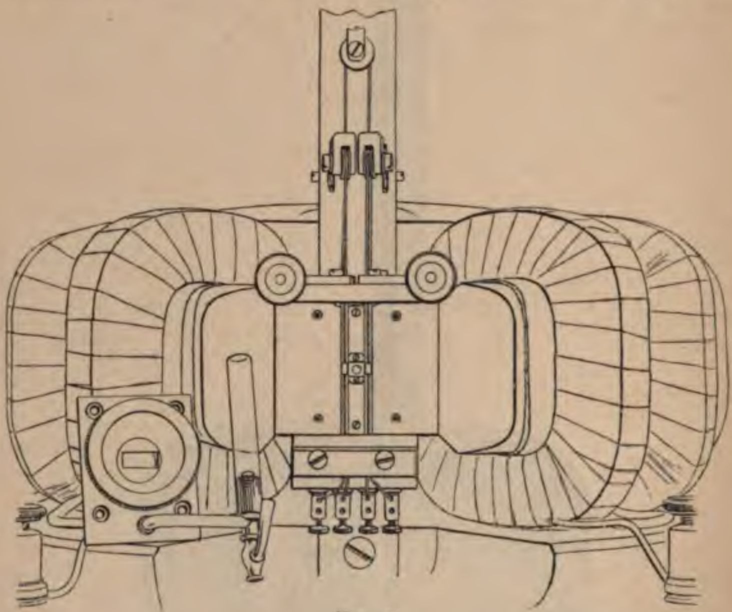


FIG. 3.

is for the purpose of introducing the damping liquid, and acts as a gauge glass. This double instrument is also provided with a fixed mirror seen between the moving mirrors, the spot from which traces the zero line.

The optical and general arrangement of these oscillographs is shown in Fig. 4, in which O_1 is the oscillograph having one vibrating system and whose mirror is marked m_1, and lens l_1, and O_2 is the oscillograph having two vibrating systems whose mirrors are m_2 and m_3, and lens is l_2; the mirror marked m_4 is the fixed mirror which draws the zero line.

The source of light used was a direct current arc enclosed in an optical lantern, L, fitted with a parallel beam system and vertical slit, *d*, about 1·5 mm. wide. The distance from the slit *d* to the lens l_2 was 270 cms.

The photographic plate was dropped down a long vertical slide which is shown in section at S. This slide had a catch at the top from which the plate was dropped, and a horizontal slit about 6 mm. wide to let the light from the mirrors fall on to the plate; the vertical distance from the catch to the horizontal slit was such that the mean velocity of the plate when passing the slit was 640 cms. per second; a brake which pressed on the back of the plate served to stop it at the bottom of the slide. The plates were introduced into and removed from the slide by means of light-tight bags

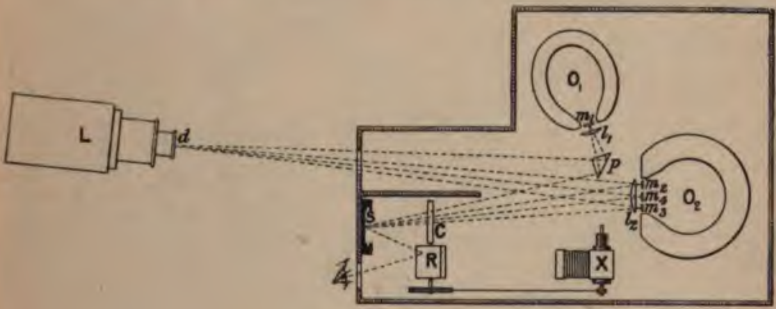


FIG. 4.

and wooden clips. The photographic plates used were Ilford red-label of stereoscopic size, that is $6\frac{3}{4}$ ins. × $3\frac{1}{4}$ ins., and were developed by hydroquinone and caustic soda. In front of the slit in the vertical slide is a cylindrical lens, C, with its axis horizontal, the use of which will be explained later. R is the rotating mirror and X the small direct current motor which drives it; *p* is the right-angled total reflection prism.

The distance of these instruments from the plane of the falling plate in S is such that the images of the vertical slit, *d*, formed by the mirrors, m_2 m_3 m_4, and lens, l_2, are in focus on the plane of the falling plate when the cylindrical lens, C, is removed, and this also applies to the image of the slit formed by m_1 and l_1 after total reflection in *p*. The cylindrical lens which has a focal length of 15 cms. is

placed in such a position that the planes of the falling plate and the mirrors of the instruments are at its conjugate

FIG. 5.

foci; the result of this is to condense vertically the images of the vertical slit, *d*, and turn them into small intense

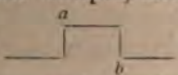
spots of light without reducing the deflections obtainable. When the mirrors m_1 m_2 m_3 m_4 are all on the same horizontal plane, the resulting intense spots will all be in the same horizontal line even if the vertical axes of the mirrors are not all parallel.

This cylindrical lens, which was suggested to us by Professor Boys, besides giving very intense spots removed all necessity for a narrow slit in the vertical slide or a delicate vertical adjustment of the spots. The spots can now be made to coincide with that given by the fixed mirror, m_4, or zero spot, by means of the tangent heads fixed to each vibrating system.

In order to observe the wave forms at the same time that they are photographed, a white screen is placed in S just behind the plane of the falling plate, and the spots fall on this screen except when intercepted by the plate. By observing the reflection of the spots on this screen in the rotating mirror, R, we see the wave forms even whilst the plate is actually photographing them.

The whole of the apparatus except the lantern is enclosed in rough wooden cases, in which there are holes to let in the light from the lantern, and through which the rotating mirror may be seen. The wooden case part of the apparatus is very prominent in the photograph, Fig. 5.

As it is most important that the damping of the instruments should be such that the motion of the mirror just ceases to be oscillatory, a fine adjustment of the damping is necessary, and this is provided by using a liquid, thus obtaining a viscous damping which depends on the temperature. We have therefore only to find, once for all, over what range of temperature the damping fulfils the above condition, and afterwards to use the instrument within this range. For this purpose the oscillographs are provided with thermometers, and the temperatures of the instruments are varied by means of their exciting currents.

The method of finding the correct damping temperature is quite simple, and consists in using a square wave form thus 

obtained from cells and a contact maker, and gradually raising the temperature of the instrument until the spot, as seen in the rotating mirror, is just on the point of overshooting at a and b. The oscillo-

graph will then be at the correct temperature; for smooth wave forms slightly higher temperature, *i.e.* less damping, may be used with advantage.

The electrical connections of the oscillographs are shown in Fig. 6, in which O_1 and O_2 are the two oscillographs, B a set of accumulators which supply the exciting current for their electromagnets, A an ammeter, and R a carbon resistance used to adjust the exciting current. L is a small lamp used to illuminate the thermometers on the oscillographs, and X a motor which drives the rotating mirror. The three vibrating systems, v_1 v_2 v_3, are each in series with a fuse, a switch, and a resistance box, shown respectively at

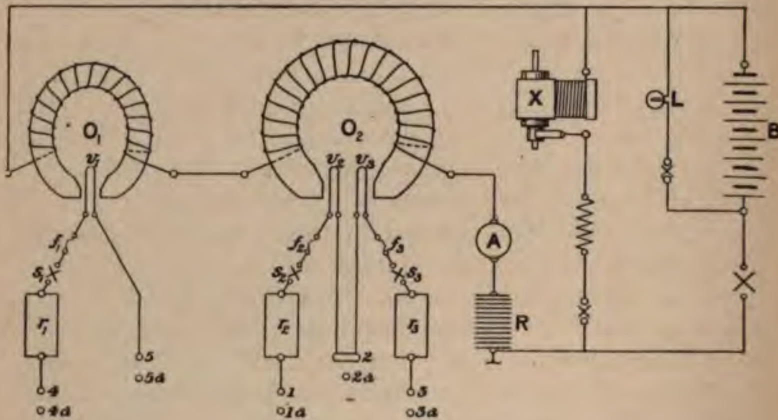


FIG. 6.

f_1 f_2 f_3, s_1 s_2 s_3, and r_1 r_2 r_3, and are connected to mercury cups on wax blocks 1, 2, 3, 4, 5. On these wax blocks connections are made either to the points between which the P.D. is required, or to the terminals of the low resistance shunts used to obtain the current wave forms.

The instruments were calibrated by passing a known current, generally $\frac{1}{10}$ amp., through them when they were in their normal working condition of excitation and temperature, and measuring the deflections produced, from which the resistance necessary in the boxes, r_1 r_2 r_3, was calculated so that one millimetre on the plate might represent a round number of volts or amperes.

To give an idea of the vibrating systems used, the data

of one system of oscillograph, O_2, are given below, the other system being almost identical, and that of O_1 a little more sensitive.

Strips rolled from 6 mil. phosphor bronze wire.
Free vibrating length, 4·5 cms.
Tension on the pair, 2,040 grams.
Weight per cm. 1·5 milligrams.
Distance apart, centre to centre, 1·38 mm.
Mirror, about 3 × 1·5 mm. by 0·2 thick.
Weight of mirror, about 2 milligrams.
Resistance, 0·53 ohm.
Damping correct at 32° to 34° C.
Undamped free periodic time, $\frac{1}{2700}$ sec.
Distance from instrument to plane of plate, 135 cms.
Sensibility at this scale, distance 1 mm. = $\frac{1}{200}$ amp.

ADVANTAGES OF THE OSCILLOGRAPH METHOD.

The principal advantages of the oscillograph method as employed by us may be classed under five heads.

1. We obtain the form of each individual wave, and we can see when and in what way it differs from the next.

2. We obtain our three wave forms simultaneously, and can thus see, when there is a change in one wave, what is the corresponding change in the other two.

3. We are able to investigate reactions which do not recur periodically, or which are of a transient nature.

4. The great saving in time and labour over all point methods, even over methods so perfect as those of Professor Rosa and M. Blondel. The actual time taken in putting a plate in the slide, photographing the wave, and removing the plate is well under a minute, and the average time taken in developing and fixing the plates in batches of a dozen or more is less than three minutes.

The greatest saving in time and labour, however, lies in the use of the rotating mirror in which the wave form may be observed while the experiment is in progress, so that only those wave forms need be recorded which show some interesting feature. This reduces the number of wave forms recorded to a minimum.

5. Accuracy. Up to the present we have not considered the important question of the accuracy of the wave forms

obtained by our method; we shall, therefore, proceed to answer it now.

The accuracy of the results obtained depends on the shape of the wave form it is required to record, and on its frequency; the more peaked and distorted the shape, that is, the more important the higher harmonics of the wave form are, the less is the degree of attainable accuracy. Also the higher the frequency the less the accuracy.

To show the degree of accuracy under our ordinary

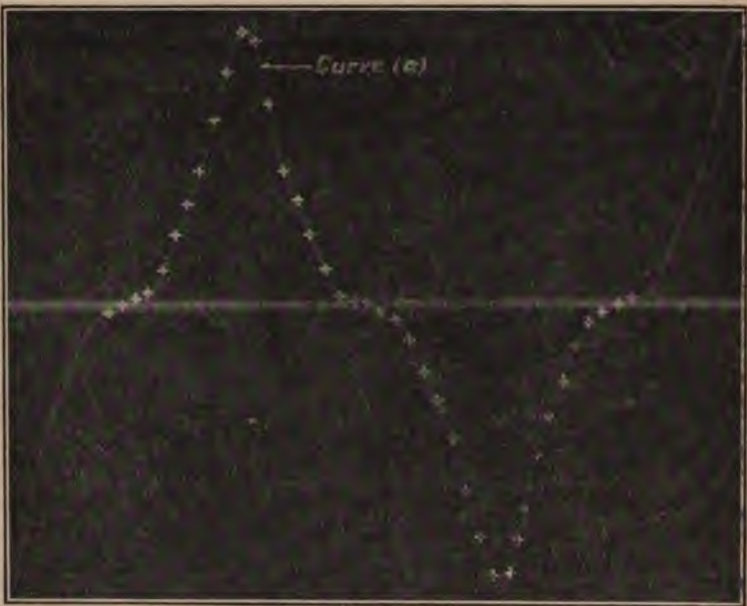


FIG. 7.

working conditions, we have compared some wave forms obtained by the oscillograph with their shapes as given by the Joubert contact method.

For this purpose we have chosen three wave forms, the circuit conditions of which we could easily maintain constant.

(*a*) The open circuit curve of an inductor alternator (Pyke and Harris machine mentioned later) frequency of 97 ∿ per sec.; this was chosen for its very peaked form and the presence of a large third harmonic.

(*b*) and (*c*) The P.D. and current curves respectively of the same machine at the same frequency when sending current through a practically non-inductive wire resistance; these were chosen for their distorted shapes. In Fig. 7 is given curve (*a*), reproduced full size, as obtained by the double oscillograph, and on it are points marked thus + in the figure as given by the Joubert contact method. In Fig. 8 are given curves (*b*) (*c*) obtained in the same way, and in which the crosses have the same meaning.

The practical identity of the points on the oscillograph

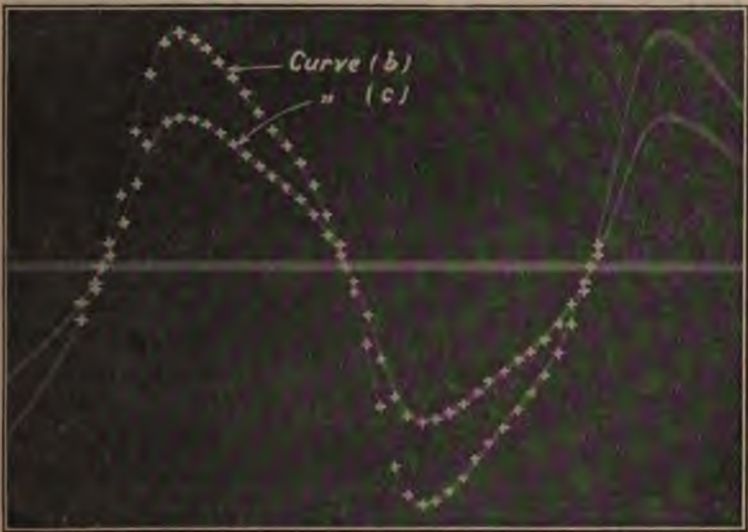


FIG. 8.

curves with those obtained by the Joubert method is very striking, and where small differences can be detected we are unable to say which is the more accurate; for the speed of the dynamo was not, of course, absolutely constant; and it must be remembered that the oscillograph points have reference to time, while the Joubert points have reference to the angular position of the moving part of the dynamo.

This close agreement of the shapes of the curves obtained with the oscillographs and the shapes of the waves might be expected from the following considerations.

According to the theoretical investigations of M. Blondel,[1]

[1] *Comptes Rendus*, 1893, vol. cxvi. p. 751.

when we are working under normal conditions—namely, with critical damping and a frequency of 100 ∿ per sec., the amplitude of the fundamental harmonic of the wave form should not be reduced by more than 0·14%; but the amplitude of higher harmonics would be reduced by a greater amount. Thus the third harmonic should have its amplitude reduced by something under 1·2%. There is also another possible source of error—namely, the higher harmonics of the wave form may be displaced relatively to the fundamental; the maximum value of this angular lag between the third harmonic and the fundamental is theoretically in our case $2\frac{1}{4}°$, and increases for the higher harmonics.

In most practical cases, however, the amplitudes of the higher harmonics are comparatively small, so that the errors due to their reduction in amplitude and lag in phase are inappreciable. If, however, the oscillograph is required to record a wave form of which the fundamental, or any of the higher harmonics of large amplitude, has a periodic time comparable with the free periodic time of the instrument, errors will be introduced; but the results obtained can always be corrected for them as pointed out by M. Blondel.

NOTE.—Since the results contained in this paper were obtained, an oscillograph of the type we used has been constructed by the Cambridge Scientific Instrument Company, which, while it has a sensibility the same as the above instruments and fulfils the other conditions, *has a free periodic time of less than $\frac{1}{10,000}$ second*, and is consequently very much more accurate, especially for work at high frequencies.

DETAILS OF CIRCUIT.

Fig. 9 is a diagram of the connections generally used, in which D is the alternator, R is the non-inductive resistance or impedance coil in series with the arc, as required. S is the 0·1 ohm resistance used for the current wave forms. A is an Evershed alternate current ammeter, which could be read to $\frac{2}{3}$% at our ordinary working current of 15 amps. W is a Ganz wattmeter of the suspended coil type, used to measure the power supplied to the arc, and B is the box containing the resistances and switches belonging to it. We could read this instrument to within $\frac{1}{2}$% from 300 watts upwards; the current taken by its fine wire-suspended coil was less than the minimum change in current which we

could read on A, and was therefore neglected in nearly all cases. V_1 is an Ayrton Mather direct-reading electrostatic voltmeter, used to measure the P.D. between the dynamo terminals and also the P.D. between the terminals of the impedance coil at R when used, the change being made from one to the other by the two-way switch, *s*. As the range of pressure through which we worked was rather long, three voltmeters were actually used, giving us readings from 30 to 250 volts and with maximum error of $\frac{1}{10}$th volt for a steady P.D. When using the arc, however, we found we could only read to the nearest $\frac{1}{2}$ volt.

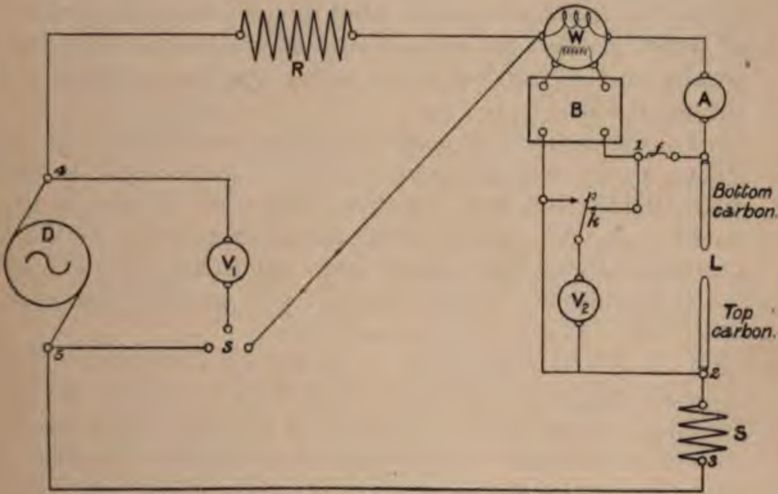


FIG. 9.

V_2 is a reflecting electrostatic voltmeter of the same type as the above, used to measure the P.D. between the terminals of the lamp, and this we could read when steady to $\pm$ 0·1 volt. All the instruments were periodically calibrated by means of the standards of the Central Technical College, and were found to keep very fairly constant, especially the voltmeters and Evershed ammeter.

We must call attention here to the fact that we measured the P.D. between the terminals of the lamp and not between the tips of the carbons, therefore what we call "P.D. arc" includes the drop of pressure in the terminals, the metal work of lamp, the carbon holders, and two

10 cms. lengths of carbon, 10 cm. being the length we usually allowed to project from the holders. We determined experimentally, by short circuiting the carbon holders with a 20 cms. length of carbon, what this drop amounted to, and found that, with our ordinary working current of 14·8 amps. and all the metal work hot as in use, the drop was 1·15 volt, and thus the resistance of these contacts, &c., is 0·078 ohm. All the figures which we give later are the actual readings and are *not* corrected for this drop.

The reason we did not measure the pressure between the carbon tips was that we wished to measure our r.m.s. volts between the same points as our instantaneous values given by the wave forms, and that, with our method of working, the resistance of contacts at the carbon tips would have introduced errors of an uncertain nature into the wave forms.

To obtain the three wave forms the points numbered 1a, 2a, 3a, 4a, 5a, in Fig 9 were connected respectively to the points having the same numbers on the wax blocks in the diagram of oscillograph connections, Fig. 6, and were there connected to the instruments when required.

Two alternators which differed very greatly from each other were used for our experiments :—

1. A Ferranti alternator of 9 kw. capacity which had a small armature reaction and which gave on open circuit, or on a non-inductive resistance, a very close approximation to a sine wave ; this machine had a range of frequency from 90 to 240 ∿ per sec. and gave at 100 ∿ per sec. on open circuit with maximum excitation about 100 volts.

The resistance of the armature is 0·24 ohm.
The self induction „ „ 0·0009 h.

2. A Pyke and Harris inductor alternator [1] of 6 kw. capacity which had a very large armature reaction and which gave on open circuit a peaked wave form (see Fig. 7), and on a non-inductive load a kind of hump (see Fig. 8). This machine had a range of frequency from 30 to 125 ∿ per sec. and gave at 100 ∿ per sec. on open circuit with maximum excitation about 330 volts.

The resistance of the armature is 0·4 ohm. The self induction of the armature, which is very large, varies with

[1] A description of this machine is given in the *Electrician*, 1892, vol. xxix. p. 37.

the exciting current and position of the inductor from a maximum with no excitation and the inductor opposite the pole of 0·012 h. to a minimum with the maximum exciting current we have used and the inductor half-way between the poles of 0·009 h.

The arc lamp used was a hand-regulated one having adjustments for centreing the carbons, and fitted with holders for two search carbons. For the purpose of measuring the length of the arc its image was projected on a screen by means of a lens so placed that the image was six times as large as the arc; the screen was ruled with lines 6 mm. apart, so that the length of the arc could be read off direct in millimeters. And it is the length as measured on this screen which we mean when we speak of the length of the arc: thus, for example, length 0· mm. does *not* mean that the *carbons* were actually *touching*, but that the *images* of the carbons were *touching*. This image of the arc was also very convenient for making observations on the appearance of the arc. The arc was shielded from draughts by means of a sheet zinc screen, which also served to protect the eyes from the light, a most important precaution, as a bare arc is very injurious to the sight.

CHOICE OF VARIABLES.

In experiments on the reaction of the arc on the wave form we have three main variables, viz.:—

1. The arc, which may be varied by changing its length, the current through it, and the nature of its electrodes.

2. The circuit, which may be varied by introducing resistance, self induction, and capacity, in series or parallel with the arc, or by the use of transformers, &c.

3. The impressed E.M.F. wave and its frequency, which may be varied by using different machines and different speeds.

We have thus a very large choice of variables, and their combination gives so many possible cases that a lifetime would not exhaust them. We have therefore confined ourselves to those cases which gave most promise of useful or interesting results.

In selecting our cases, we have been obliged to include a certain number which have already been partly investi-

gated, owing to the fact, that some of the previous workers have not confined themselves to changing one variable at a time, and therefore their results are not strictly comparable, one with the other.

Our investigations may be divided under the following eleven heads, thus :—

	In Diagram and Table. No.	Change Made.
1.	I.	Preliminary experiments. A resistance replaced by an arc.
2.	II. and III.	The arc length varied.
3.	IV.	Search carbons introduced. Distribution of fall of potential in arc.
4.	V., VI., VII., and VIII.	Resistance in series with arc varied.
5.	IX. and X.	Frequency changed.
6.	XI. and XII.	Transformers to supply current to arc.
7.	XIII. and XIV.	Different makes of carbons compared.
8.	XV. and XVI.	Foreign substances introduced as cores.
9.	XVII.	Arcs between carbon and different metals.
10.	XVIII.	The arc enclosed.
11.	XIX.	Miscellaneous.

In connection with the above we have examined over 400 different cases and have recorded over 1,000 wave forms from which we have selected typical cases to illustrate the paper.

The large number of typical cases contained in our diagrams were rendered necessary by the importance of determining how the reaction of the arc depends on each separate variable; and for this purpose it is absolutely necessary that the variables should be changed systematically one at a time, and that the results should be so arranged and classified, that the effect of the systematic change of each variable alone is at once evident.

Thus, probably now for the first time, it is possible to study how the change of each variable alone affects the alternate current arc.

Plan of Experiments.

In every experiment, a certain length of arc and current having been selected, the speed and excitation of the alternator were kept constant, and *the distance between the carbons was regulated so as to keep the P.D. between them constant during the whole course of the experiment.*

When once the arc had burnt into shape—that is, had attained the steady state corresponding with the length, current, and other conditions under which it is burning—we found, as long as the conditions of the dynamo and circuit remained the same, that regulating the length of the arc so as to keep the P.D. between its terminals constant, maintained the arc length, the current, and the power supplied to the arc constant. Thus it is possible to compare our results with those of M. Blondel who kept the current constant, and those of Prof. Fleming who kept the power supplied constant.

It seems to us that regulation by means of the power supplied can hardly be as sensitive, at any rate in the case of the arc between solid carbons, as regulation by the current or the P.D., for when the current rises the P.D. falls and the power factor for small changes remains constant; hence the power may be kept practically constant without the P.D. and current remaining so.

Mrs. Ayrton has drawn attention to the fact, that with the direct current arc, a considerable time is required for it to attain a steady state, and we find that this, as might be expected, applies equally to the alternate current arc. Thus a 15 amp. 3 mm. arc between a new pair of 13 mm. solid carbons requires generally about ½ hour to settle down, very long or short arcs generally requiring more time. When, however, one (or both) of the carbons is cored, things are very much changed, the time required being very much longer, and in the case of both new cored carbons as much as one hour to 1¼ hours is required; another noticeable fact with cored carbons is the very low P.D. at which the arc will burn when first struck, being often 14 or 15 volts for a 3 mm. 15 amp. arc whose normal voltage is about 31 volts. Even after the carbons have burnt into shape, if the arc be extinguished and re-lit, it will generally burn at too low a P.D., but the latter will gradually rise to its former value. We will refer to this subject again later, and attempt to give some explanations of these results.

We have had considerable difficulty in obtaining even approximately comparable results when using an arc with one or both of its carbons cored; this difficulty, which we believe was also first pointed out by Mrs. Ayrton, seems to be due to a want of uniformity in the composition of the

core, and in the binding material which holds the core together. We have also noticed another difficulty with cored carbons, and that is that any vibration or jar given to the arc causes some of the dust from the core to fall into the arc and completely alters its conditions of burning, the arc taking a considerable time to recover its normal state again, and this effect may greatly influence the adjustment of the regulating mechanism of an ordinary arc lamp which is liable to be shaken by the workman when setting it.

There are two kinds of carbons in general use, namely, solid and cored, and these give three combinations between which we can burn an arc, and they are—

1. The arc between solid carbons which we will call for the sake of shortness the "Solid arc";

2. The arc between one solid and one cored carbon which we will call the "Solid-Cored arc," always putting the top electrode first; and

3. The arc between two cored carbons which we call the "Cored arc."

In most of our comparative experiments we have used one make of carbons throughout, namely, the "Apostle," and we have tried in turn the effects with the "Solid," the "Solid-Cored," and the "Cored arc" with carbons of this make. We have also used in all the experiments, except where stated to the contrary, carbons 13 mm. in diameter, and we have taken, whenever convenient, about 15 amps. as a suitable working current for these carbons and 3 mm. as our standard length, although, as will appear later, we have often repeated the experiments with different lengths.

EXPLANATION OF TABLES AND DIAGRAMS.

In the diagrams and tables corresponding numbers in each refer to the same experiment, the number of the experiments in the diagrams being given in the right-hand bottom corner of each set of wave forms.

The tables contain, in all cases, the root-mean-square values of the wave forms and other data as obtained from the instrument readings, while the diagrams contain the actual wave forms.

In the wave forms given in the diagrams, time is always measured from left to right as usual, and the scales of the

respective curves are given on each diagram as so many volts or amperes per millimetre. Also in all cases the top half of the wave form corresponds to the top electrode, being positive.

DESCRIPTION OF RESULTS.

SECTION I.—PRELIMINARY EXPERIMENTS.

(Diagram and Table I.)

To give a clear idea of the main features of the reaction of the arc on the alternate current wave forms, we replaced a non-inductive resistance, between two points in a circuit, by a solid arc taking the same r.m.s. (root-mean-square) current, while the dynamo excitation and the other conditions of the circuit remained unchanged. The results of this substitution, for three different circuits, are given in the first two rows of diagram I. In each case the arc substituted had the same length, carbons, and current.

In the first case the whole circuit, including the alternator, was comparatively non-inductive, and with resistances alone all the waves were sinusoidal (expt. 1). The substitution of the arc for part of the non-inductive resistance caused a flattening of the P.D. wave between the terminals of the resistance, the production of a large front peak *p*, and the reduction of the current to zero for a considerable portion of the period (expt. 2).

In the second case, in which some of the resistance was replaced by inductance (expt. 5), the P.D. arc curve is almost rectangular, while the current rises nearly as rapidly as before the arc was introduced (expt. 6).

In the third case, the total inductance in the circuit had approximately the same value as in the last; but instead of having inductance outside the alternator an inductive machine was used. Although before the introduction of the arc the P.D. and current curves (expt. 9) are quite different from those previously obtained, yet after the substitution the P.D. arc curve is nearly rectangular, as before; but the current wave is little affected by the change (expt. 10).

DIAGRAM

PRELIMINARY

Carbons, 13 mm. "Apostle." Current,

ON FERRANTI ALTERNATOR.

Frequency, 100 ∿ per sec.

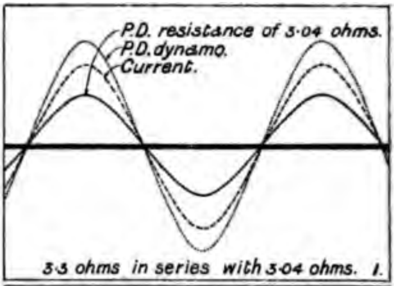
Resistance in series.

Same Dynamo Excitation.

NON-INDUCTIVE RESISTANCE IN PLACE OF ARC.

Scales.

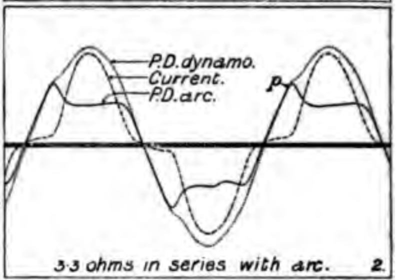
For P.D. Dynamo - 1 mm. = 10 volts.
P.D. Resistance, 1 mm. = 10 volts.
Current - - - 1 mm. = 2 amps.



SOLID ARC.

Scales.

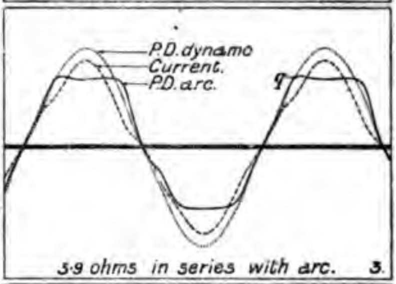
For P.D. Dynamo - 1 mm. = 10 volts.
P.D. Arc. - - 1 mm. = 10 volts.
Current - - - 1 mm. = 2 amps.



SOLID-CORED ARC.

Scales.

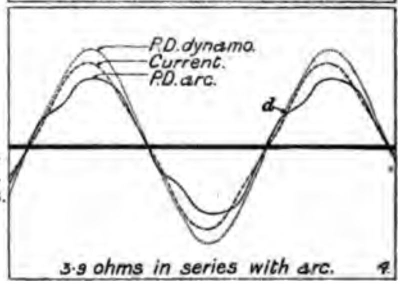
For P.D. Dynamo - 1 mm. = 10 volts.
P.D. Arc - - 1 mm. = 5 volts.
Current - - - 1 mm. = 2 amps.



CORED ARC.

Scales.

For P.D. Dynamo - 1 mm. = 10 volts.
P.D. Arc - - 1 mm. = 5 volts.
Current - - - 1 mm. = 2 amps.

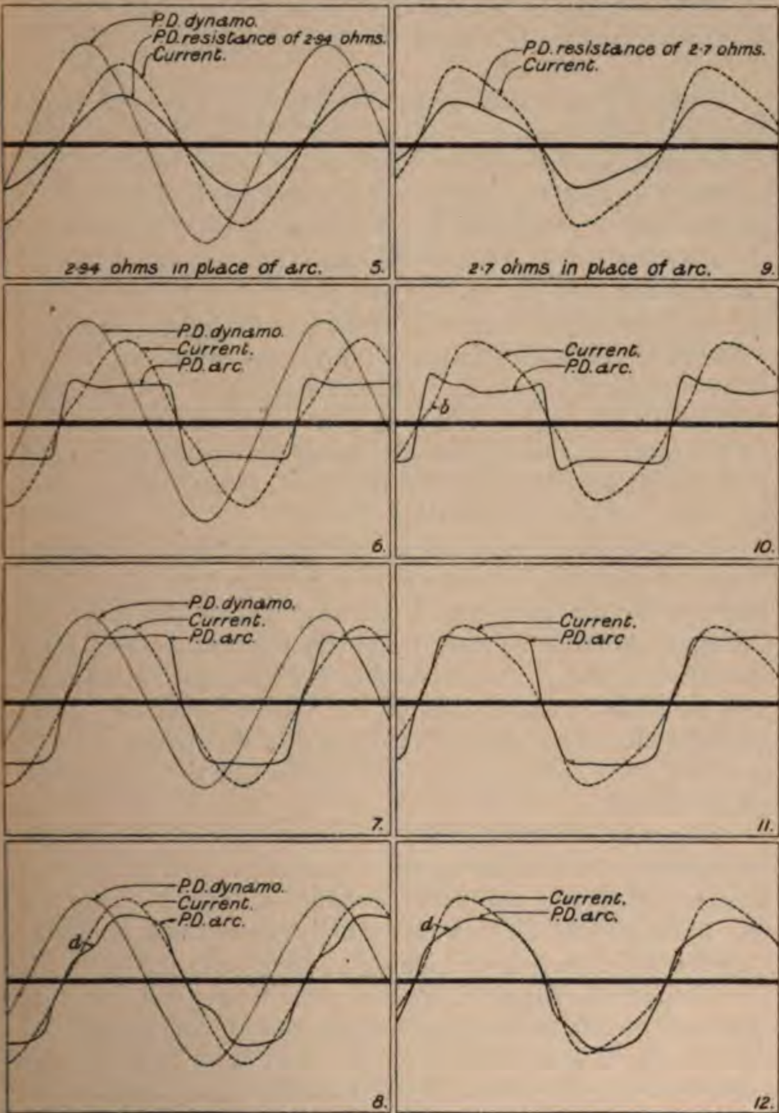


NOTE.—*These diagrams are*

14·8 amps. Arc length, 3 mm.

ON FERRANTI ALTERNATOR.
Frequency, 100 ∿ per sec.
Self-induction in series = $7{\cdot}6.10^{-3}$ h.
Resistance in series = 0·3 ohm.

ON PYKE AND HARRIS ALTERNATOR.
Frequency, 97 ∿ per sec.
Resistance in series = 0·3 ohm.



here reproduced half-size.

In row 4, two cored carbons are employed; and although, to keep the arc conditions constant, it was necessary to change the dynamo excitation, the curves are comparable in *form*, respectively, with those in the first row obtained with resistance in place of the arc. It will be noticed that the current curves in row 4, are practically the same as those in row 1, but that there is a characteristic dent, *d*, on the front side of the P.D. arc waves (expts. 4, 8, 12).

It might be expected that the solid-cored arc would have a reaction intermediate between those of the solid and the cored arcs, and this is the case, the P.D. arc waves, row 3, being squarer in shape than those in row 2. But, further, the two halves of the P.D. wave, above and below the zero line, are not exactly the same; the top half, which corresponds to solid carbon +, being slightly convex to the axis of time as in row 2, while, on the other hand, the lower half, which corresponds to the cored carbon being +, shows, especially in expt. 3, the characteristic dent of the cored P.D. arc curve. *Hence it follows that the nature of the carbon that is positive, at any instant largely governs the reaction at that time*, and this conclusion is amply borne out by experiments to be described later.

There is an observation of M. Blondel which is illustrated by diagram I., and which impresses itself on us the more we work at the subject, and that is, that the shape of the P.D. arc wave depends on the total resistance and self-induction in the circuit (including the alternator armature) and is almost independent of the type of machine used, whereas the current wave form is largely dependent on the machine used and the nature and amount of the impedance of the circuit.

When for a resistance between two points in a circuit we substitute a solid arc taking the same r.m.s. current, and the dynamo excitation and other conditions remain unaltered, we find that the r.m.s. of the P.D. between these points is raised in all cases (expts. 1 and 2, 5 and 6, 9 and 10, Table I.),

The power supplied to the circuit between the two points is greatly reduced by the substitution of a solid arc for one of the two resistances in series with the Ferranti alternator (expts. 1 and 2); and as the r.m.s. current has not been changed, the power supplied to the circuit outside

the arc is constant, therefore the whole output of the alternator is reduced by replacing part of the outside resistance by a solid arc, the main current and the excitation, &c., being the same in both cases.

We arrive at a similar conclusion when we consider an inductance and a resistance in series with the Ferranti machine, the resistance being replaced by a solid-arc taking the same current (expts. 5 and 6) ; in this case, however, the effect is only slight.

With the third circuit, that is a large and small resistance in series with the inductive Pyke and Harris alternator, the replacing of the larger resistance by a solid-arc taking the same current (expts. 9 and 10), the other conditions remaining the same, leads to an increase of the output of the machine, which is the opposite to the effect in the last two cases.

An examination of the wave forms (expts. 1 and 2) shows that the reduction in output on the Ferranti alternator is due to the current remaining small during a considerable portion of the period while the P.D. is comparatively large, and in a less degree to the drop in the P.D. dynamo which accompanies the higher maximum value of the current. This also explains the slight reduction observed in expts. 5 and 6.

The observed increase of output of the Pyke and Harris alternator is not so easily explained, for the total self-induction of the circuit in expts. 9 and 10 is roughly the same as in expts. 5 and 6. The explanation, however, seems due to the fact that owing to the very large internal drop of pressure in this machine, a slight diminution in the current at any instant is accompanied by a large increase in the P.D. between the dynamo terminals.

With the solid-cored and cored arcs the effects are similar ; but all occur in a much less degree.

The r.m.s. value of the P.D. between the terminals of the alternate current arc, the power it absorbs, and its power factor, are not fixed functions of the nature of the carbons, the arc length, and the current ; but depend also on the nature of the circuit outside the arc. Thus with the solid arcs (expts. 2 and 6), which are between the same carbons, have the same length and r.m.s. current, and are supplied by the same alternator, the P.D. between the terminals of the arc is reduced, 3 volts, or 6%, if resistance

in series with the arc be replaced by self-induction, while at the same time the power absorbed by the arc is increased by 9%, with a consequent improvement of the power factor from 0·74 to 0·855. The same effect is also produced if instead of self-induction in series a highly inductive alternator supplies the arc (expt. 10).

With solid-cored and cored arcs the same changes occur; but to a less extent.

The power factors of solid-cored and cored arcs are always higher than those of solid arcs, and for the cored arc are generally very close to unity.

The percentage of the total power given out by the alternator that is absorbed by the arc may be taken as a measure of the efficiency of the method for supplying power to the arc. Of the three methods which we have used in this section for obtaining a steady arc, the first, viz., resistance in series with an alternator having small inductance, is very inefficient, the power lost in the outside resistance being over 50 per cent. of the whole output. The second, viz., self-induction in series with the same alternator, was much better, the power wasted in the outside circuit being only that lost in the impedance coil and measuring instruments, while at the same time the arcs were much more stable. In the third case, with the inductive (Pyke and Harris) alternator the whole power given out by the alternator could have been absorbed by the arc, for the small resistance of 0·3 ohm in series was that of the measuring instruments and leads, and was not in any way necessary for stability of the arc. In fact one of the most striking results we have obtained is the great stability and comparative silence of all the arcs we have tried on this inductive machine, coupled with their steadiness and high power factors. We think that *if a highly inductive alternator, having good efficiency and a steep characteristic, could be built, these machines would be eminently suitable for arc lighting. For then the necessity for choking coils would vanish, and harder carbons, giving therefore a much higher luminous efficiency, could be employed without loss of steadiness of the light.*

Having in a general manner indicated the main reactions of the arc on the wave forms, in the preceding paragraphs, we will consider them more in detail and show the effect produced on them by changing the variables one at a time.

SECTION 2.—EFFECT OF LENGTH.

(Diagrams and Tables II. and III.)

In order to test the effect of the arc length on the reaction on the wave form, the size and kind of carbons, the current through the arc, the resistance, &c. in series, and the frequency of the impressed E.M.F. were kept constant, while the arc length alone was varied, the current being kept at the same value by changing the dynamo excitation.

With the solid arc on the Ferranti alternator, length 0 mm., that is the image of the two carbons touching (expt. 13), the P.D. arc curve has two small peaks connected by a slightly wavy[1] horizontal portion which we find is characteristic of the sound which the arc produces, the current remaining small for about $\frac{1}{8}$th of the period. If we now lengthen the arc, keeping the r.m.s. current through it constant, we find the front peak *p* on the P.D. arc curve grows higher and higher, while the back peak and waviness in the horizontal portion do not change much, until we reach an arc length of 1 mm. (expt. 14), which is a steady hissing arc; in this curve the length of time during which the current remains small has increased to about $\frac{1}{4}$th of the period and has caused the power factor to drop to the low value of 0·63. On still further increasing the length of the arc the peak on the P.D. arc curve no longer increases rapidly, and the time during which the current is small remains about the same—see expt. 15, which was with an arc length of 3 mm.

Experiments 19, 20, 21, and 22 are with the cored arc. In this case the characteristic change in the P.D. arc curve is that the dent *d* occupies a relatively higher position up the side of the curve as the arc length is increased.

The results with the solid-cored arc (expts. 16, 17, and 18) are much more complicated.

On the inductive Pyke and Harris machine, owing to its higher E.M.F., much longer ranges of arc lengths were obtainable, being only limited by the instability of long arcs. Thus with the solid arcs on this machine we were able to obtain a range of lengths from 0 to 20 mm. (expts. 23 to 28).

In this experiment, as with the solid arc on the Ferranti dynamo, the front peak in the P.D. arc curve increases with

[1] This waviness has partly disappeared in the reproduction of the curves.

DIAGRAM

EFFECT OF

ON FERRANTI

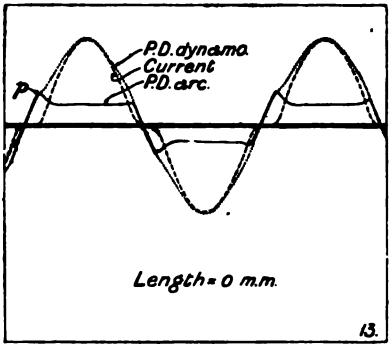
Carbons, 13 mm. "Apostle." Current,

Resistance in

Solid Arc.

Scales.

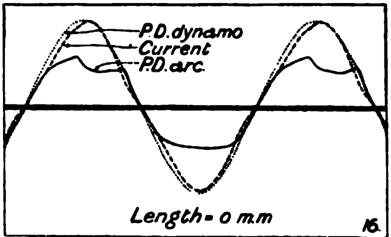
For P.D. Dynamo - 1 mm. = 10 volts.
P.D. Arc - - - 1 mm. = 10 volts.
Current - - - 1 mm. = 2 amps.



Solid-Cored Arc.

Scales.

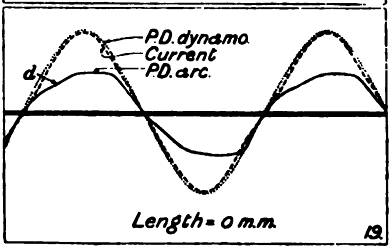
For P.D. Dynamo - 1 mm. = 10 volts.
P.D. Arc - - 1 mm. = 5 volts.
Current - - - 1 mm. = 2 amps.



Cored Arc.

Scales.

For P.D. Dynamo - 1 mm. = 10 volts.
P.D. Arc - - 1 mm. = 5 volts.
Current - - - 1 mm. = 2 amps.



Note.—*These diagrams are*

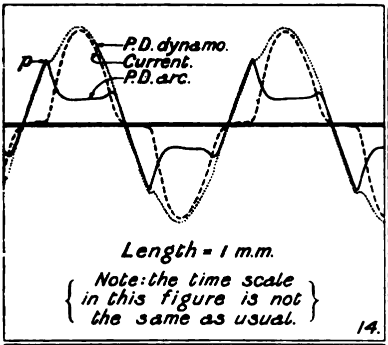
II.

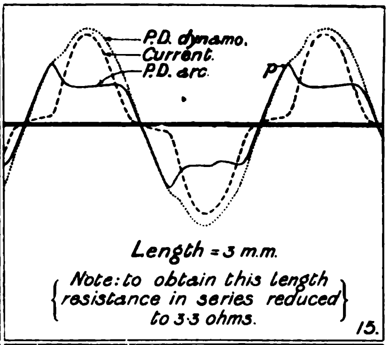
LENGTH OF ARC.

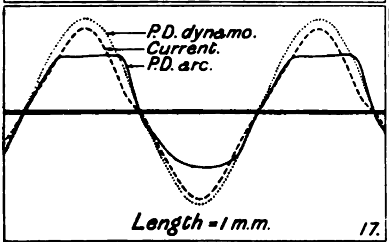
ALTERNATOR.

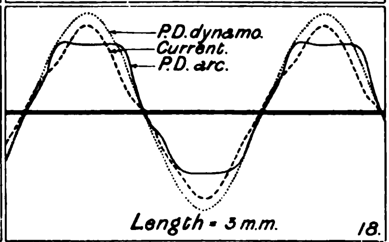
14·8 amps. Frequency, 100 ∿ per sec.

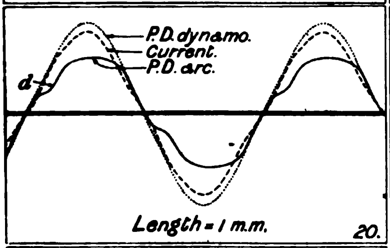
series, 3·9 ohms.

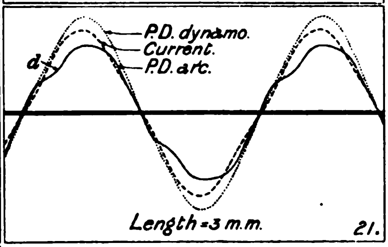












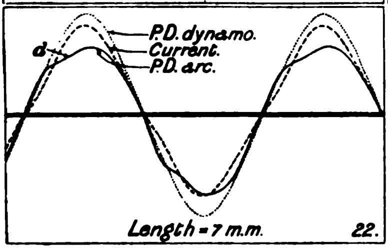


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DIAGRAM

EFFECT OF

ON PYKE AND

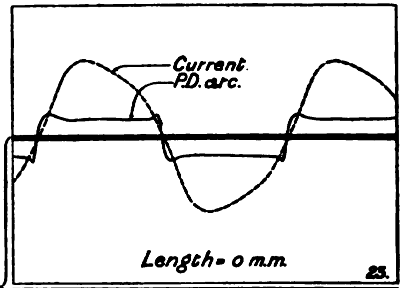
Carbons, 13 mm. "Apostle." Current,

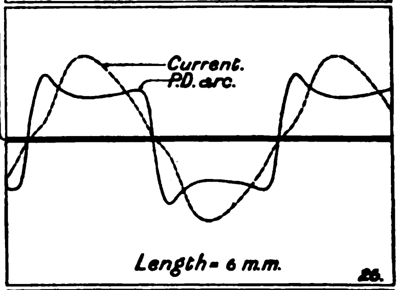
Resistance in

Solid Arc.

Scales.

For P.D. Arc. - - 1 mm. = 10 volts.
Current - - - 1 mm. = 2 amps.

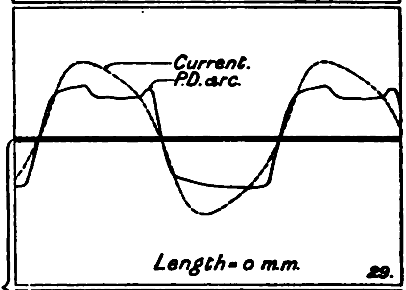


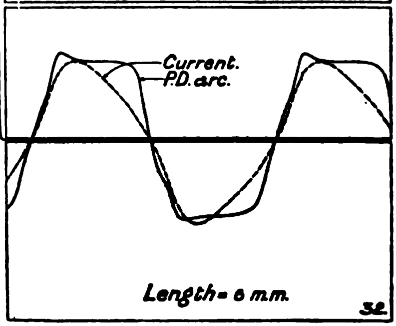


Solid-Cored Arc.

Scales.

For P.D. Arc. - - - 1 mm. = 5 volts.
Current - - - 1 mm. = 2 amps.





Note.—*These diagrams are*

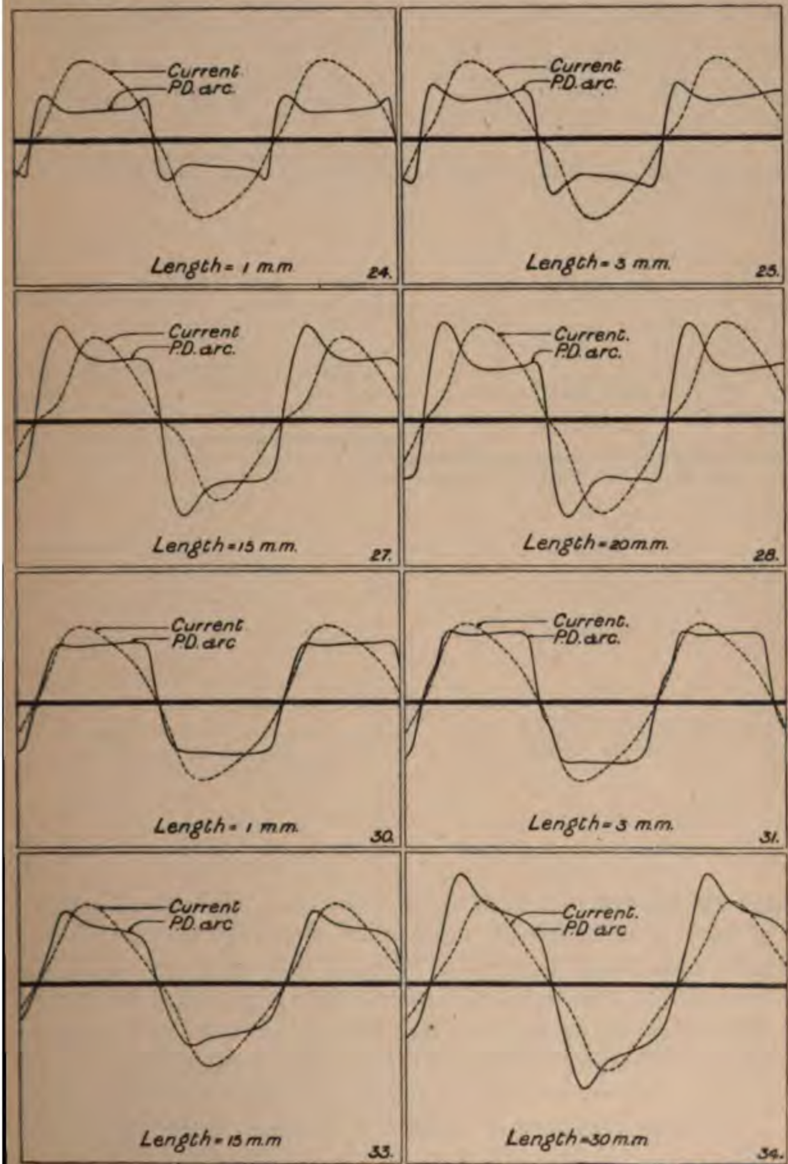
III.

LENGTH OF ARC.

HARRIS ALTERNATOR

14·8 amps. Frequency, 9·7 ∿ per sec.

series, 0·3 ohm.



Note:– P.D. arc scale changed to 1 m.m. = 10 V.

here reproduced half-size.

DIAGRAM

EFFECT OF

ON PYKE AND

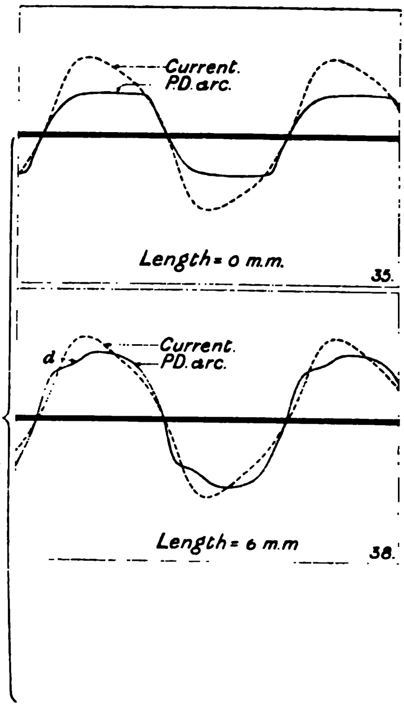
Carbons, 13 mm. "Apostle." Current

Resistance in

CORED ARC.

Scales.

For P.D. Arc. - - - 1 mm. = 5 volts.
Current - - - 1 mm. = 2 amps.



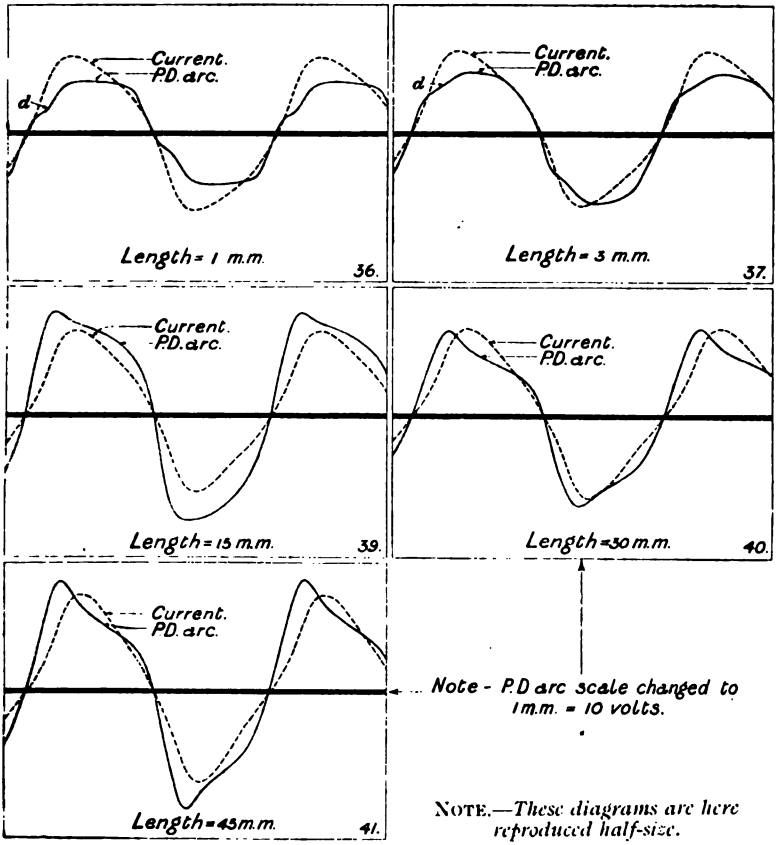
NOTE.—*These diagrams are here reproduced half-size.*

the length, but in a more regular manner, while the second or back peak is hardly noticeable when the arc length is more than about 10 mm. The current wave form tends to become more triangular in shape as the arc length increases; but this effect, we think, is due to the increase in excitation of the alternator, and not to the change in length of the arc itself, as this machine gives, when sending current through a non-inductive circuit, a more triangular wave when the exciting current is increased.

HARRIS ALTERNATOR.

14·8 amps. Frequency, 97 ∿ per sec.

series, 0·3 ohms.



NOTE.—*These diagrams are here reproduced half-size.*

The effect of change of length with the solid-cored and cored arcs on this machine is illustrated by expts. 29 to 41; the dent *d* moving up the side of the P.D. arc curve with cored carbons as before, and finally forming a front peak for long-cored arcs. The P.D. arc curve of long solid-cored arcs also has a high front peak.

A point of interest, as measuring in a way the stability of the arcs, is the length at which each arc could be burnt, the frequency, the resistance in series and the arc current, all

remaining the same. The corresponding lengths being 20 mm. for the solid arc, 30 mm. for the solid-cored, and 45 mm. for the cored arc.

The power factor, in all cases, on both alternators is lowest for hissing arcs and generally increases with increase of arc length. With solid carbons, however, we find that the power factor has a minimum in our experiments, when the arc length is about 1 mm., and the arc is hissing steadily.

In all the long arcs we have tried, whether with solid or cored carbons, we find that the P.D. between the terminals of the arc rises rapidly to a high value after passing through zero, and then falls again almost at once, forming a front peak on the curves. The increase in height of this peak with increase of arc-length led us to think that it was connected with the resistance of the gaseous column forming the arc proper, and we therefore tried the experiments described in the next section to settle this point.

SECTION 3.—SEARCH CARBONS IN ARC.

(Diagram and Table IV.)

In order to investigate the distribution of fall of potential in the alternate current arc, we inserted two search carbons (2 mm. dia.) into the arc, each search carbon being 0·5 mm. from one of the main carbons (see Fig. 10), and obtained the wave forms of P.D. between each search carbon and the main carbon close to it, as well as between the two search carbons, these three wave forms being recorded simultaneously. Directly after obtaining the above wave forms we, in each experiment, recorded the curves of P.D. between the two main carbons and the current through the arc, the search carbons remaining in position in the arc.

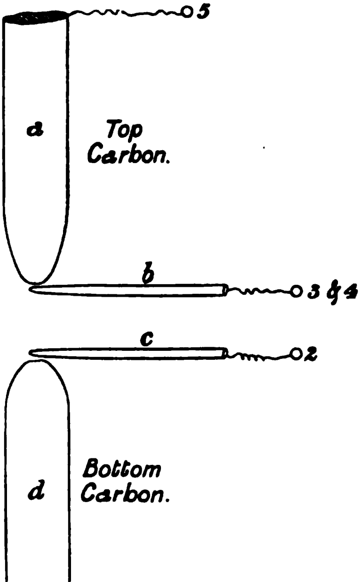


FIG. 10.

The experiments were carried out for two different arc

lengths, viz., 6 mm. and 15 mm., and for the same combinations of carbons as in our other experiments.

We find in all cases, when we consider the instantaneous values of the distribution of P.D., that they are very similar to those obtained for the direct current arc, and that we may divide the fall of the potential into three parts—a drop at the surface of the + crater, a drop in the arc proper, and a drop at the − crater, the drop at the + crater being always much larger than at the −.

As each crater changes its sign every half period, the difference of potential between it and the search carbon near it is alternately large when the crater is + and small when it is −, which makes the wave forms of this difference of potential very much higher on one side of the zero line than on the other, as is seen in Diagram IV. Thus if a search carbon is connected to the main carbon through an outside circuit, the quantity of electricity which will flow round this circuit will be larger in the direction from the main carbon to the search carbon than in the opposite direction. If a galvanometer be introduced into this circuit its deflection will seem to indicate that the main carbon is always at a higher potential than the search carbon in the arc; and this result is obtained whichever main carbon the search carbon is connected to. Our curves thus explain the above observation of Dr. Sahulka and others, and considerably alter its bearing on the question of the back E.M.F. of the arc.[1]

The magnitude of the fall of potential in the arc vapour is, we find, nearly independent of the direction of the current, so that the wave forms of potential difference between our two search carbons are almost the same on both sides of the zero line.

The instantaneous value of the P.D. between our search carbons is largest at the early part of the period giving a peak on the curve of P.D. between the search carbons, and this peak is very considerable with long arcs (expts. 45, 49, and 53—curve of P.D. between *b*, *c*.). *We thus trace the large front peak which we found on the P.D. arc curve for long arcs to the resistance of the gaseous column between the carbons, after cooling during the extinction of the arc. The small back*

[1] See leader in *The Electrician*, vol. xl., 1897, p. 326; and Dr. Sahulka, *Zeitschrift für Electrotechnik*, 1898, p. 213.

DIAGRAM

SEARCH CAR-

ON PYKE AND

Carbons, 13 mm. "Apostle." Current,

Resistance in

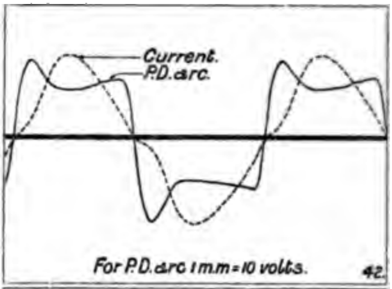
SOLID ARC.

Length of arc = 6 mm.

P.D. between terminals of arc and current curves.

Scales.

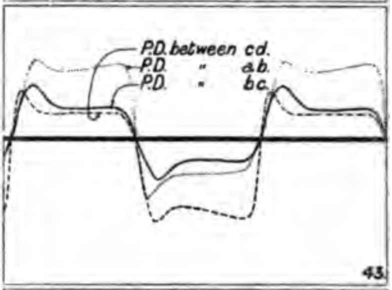
For Current - - 1 mm. = 2 amps.



P.D.'s between search carbons, curves.

Scales.

For all the curves - 1 mm. = 4 volts.

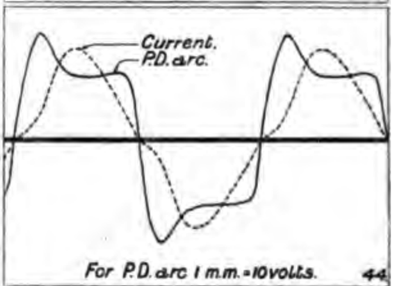


Length of arc = 15 mm.

P.D. between terminals of arc and current curves.

Scales.

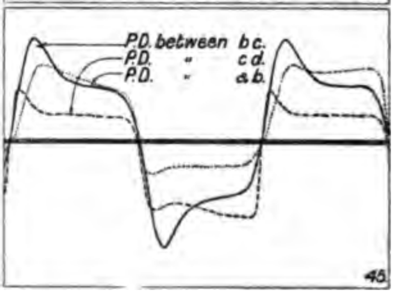
For Current - - 1 mm. = 2 amps.



P.D.'s between search carbons, curves.

Scales.

For all the curves - 1 mm. = 4 volts.



NOTE.—*These diagrams are*

IV.

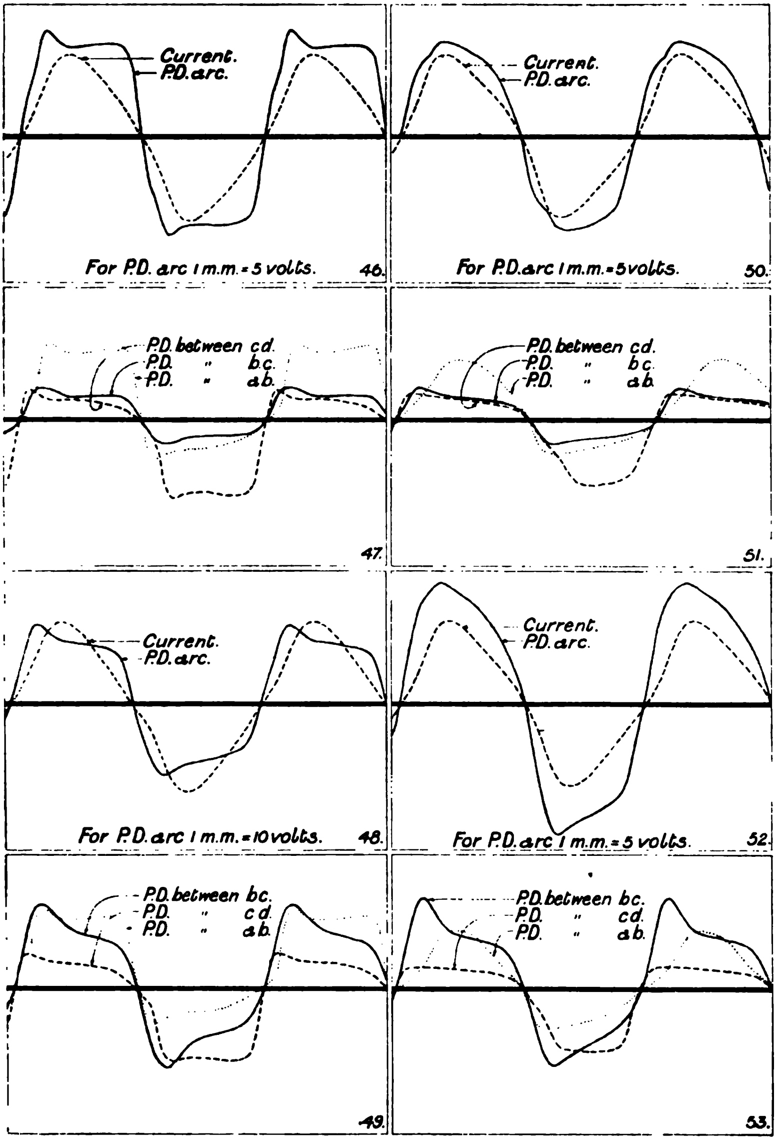
BONS IN ARC.

HARRIS ALTERNATOR.

14·8 amps. Frequency, 97 ∿ per sec.

series, 0·3 ohm.

SOLID-CORED ARC. | CORED ARC.



here reproduced half-size.

peak obtained with short solid arcs seems to arise at the + crater.

If we add up the corresponding ordinates of the three curves of fall of P.D. obtained with the search carbons, we ought to obtain the curve of the P.D. between the terminals of the arc which should be the same as that obtained experimentally, and as long as the current in the arc is not too small, this agreement is as close as might be expected in view of the fact that some minutes elapsed between recording the search carbons curves, and the corresponding P.D. arc curve.

When adding the ordinates of the curves for the cored arc, we find that *the chief cause of the characteristic dent in the front side of the P.D. arc curve is the peak in the curve of the drop of the P.D. along the vapour column.* And since this peak becomes higher with increase of arc length the dent is moved up the side of the P.D. arc curve, as noticed when considering the effect of length.

All results obtained by means of search carbons in the arc are liable to error, due to the disturbance of the arc by their presence. In our case we noticed that the introduction of the search carbons into the arc caused a sharp ridge to form across the craters, the direction of the ridge being parallel to the nearest search carbon, this latter appearing to protect the crater from wasting away. Another change that the search carbons produced was to increase the P.D. required to maintain an arc of given length and current, this increase being 17 per cent. in the case of a 6 mm. solid arc (compare expts. 26 and 42).

The power factor was also affected, and as far as our results go (compare expts. 26, 27, 32, 33, 38, 39 with 42, 44, 46, 48, 50, 52 respectively), the power factor is reduced with the solid arc and increased in the other cases by the introduction of the search carbons. One other difficulty which we noticed was that if by any chance one of the search carbons touched the crater of the main carbon the arc was extinguished, the crater touched appearing to travel along the search carbon until the arc became of considerable length and then went out.

In spite of these difficulties we think the results show conclusively :—

FIRST, *that at any instant the distribution of the difference*

of P.D. in the alternating arc is similar to that in the direct current arc.

SECOND, *that the growth of the front peak in the P.D. curve of the long solid arc and the movement of the dent in the P.D. curve of the cored arc are due to the increase of resistance of the gaseous column during each extinction as the arc is lengthened.*

SECTION 4.—EFFECT OF RESISTANCE IN SERIES.

(Diagrams and Tables V., VI., VII. and VIII.)

Mr. Frith, in his paper before the Physical Society, said that the effect of the arc on the wave form could be destroyed by placing a large resistance in series with the arc, and he pointed out the great importance of this observation as affecting efficiency tests of alternate current arcs. The curves with resistance in series obtained by Mr. Frith differ, however, very much from any of those given by M. Blondel; and the latter has not published any series of strictly comparable results to show the effect of change of resistance in series. We have therefore carried out a complete investigation of the subject for several different arc-lengths.

In our experiments we kept the size and kind of carbons, the arc-length, the current, and the frequency constant, the resistance in series being varied by small steps. The current in all cases was kept constant by changing the dynamo excitation. As the number of cases which we have examined is very large indeed, we have only included in the tables and diagrams the results for the highest and lowest resistances which we have tried and for one intermediate value. The experiments were carried out on both alternators and with the usual combinations of carbons.

The range of resistance used with the Ferranti machine, diagram V., was from 0·3 to 3·9 ohms; but it was found impossible to burn the solid and solid-cored arcs with 0·3 ohm in series; the cored arc, however, could be maintained, though it was not very stable. With about one ohm in series all three arcs could be obtained, the solid arc being none too steady, however.

Increasing the resistance in series has, with the solid arc, increased the peaks on the P.D. arc wave and reduce

DIAGRAM

EFFECT OF RE-

ON FERRANTI

Carbons, 13 mm. "Apostle." Current,

Resistance, 0·3 ohm.

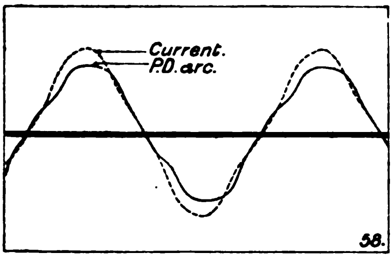
SOLID ARC.
Length, 1 mm.

With this resistance in series the arc was unstable.

SOLID-CORED ARC.
Length, 3 mm.

With this resistance in series the arc was unstable.

CORED ARC.
Length, 3 mm.



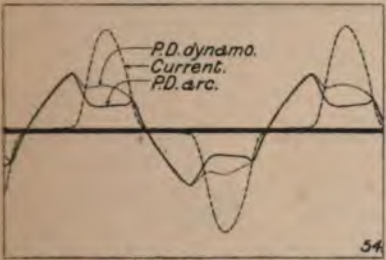
NOTE.—*These diagrams are*

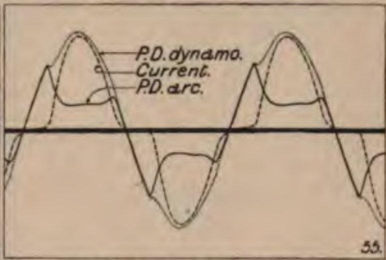
SISTANCE IN SERIES.

ALTERNATOR.

14·8 amps. Frequency, 100 ∿ per sec.

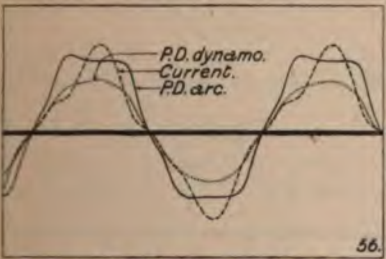
Resistance, 0·96 ohm. Resistance, 3·9 ohms.

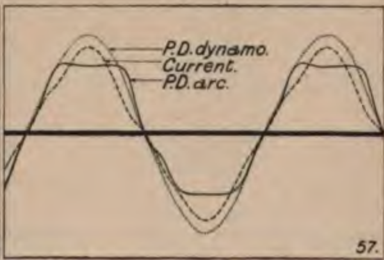




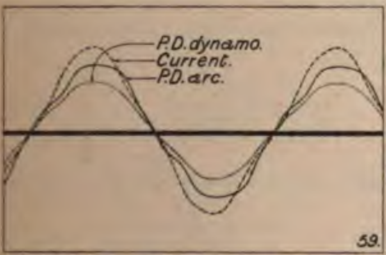
Scales — For P.D. dynamo 1 m.m. = 10 volts.
P.D. arc 1 m.m. = 10 volts.
Current 1 m.m. = 2 amps.

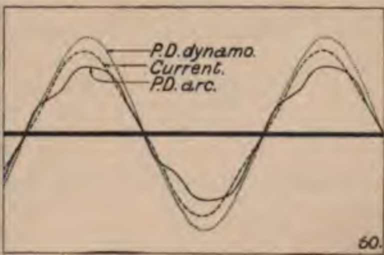
{Note - scale of time not the same as usual.}





Scales — For P.D. dynamo 1 m.m. = 10 volts.
P.D. arc 1 m.m. = 5 "
Current 1 m.m. = 2 amps.





Scales — For P.D. dynamo 1 m.m. = 10 volts.
P.D. arc 1 m.m. = 5 "
Current 1 m.m. = 2 amps.

here reproduced half-size.

DIAGRAM

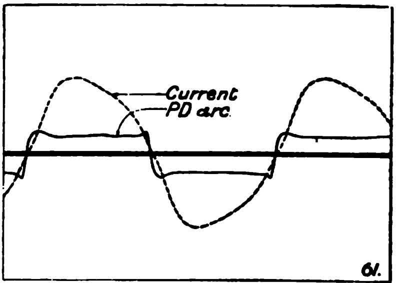
EFFECT OF RE-

ON PYKE AND

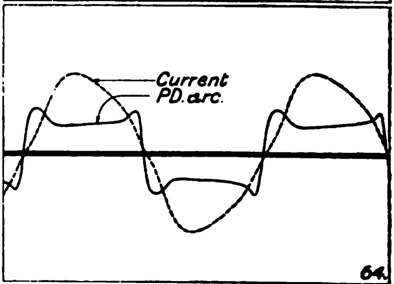
SOLID ARC. Carbons, 13 mm. "Apostle." Current,

Resistance, 0·3 ohm.

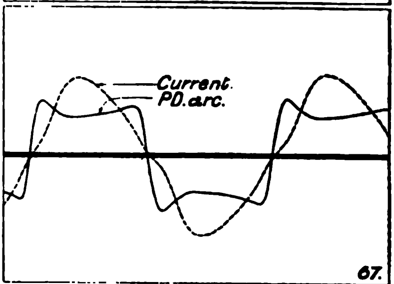
Arc length - - - - - - - 0 mm.



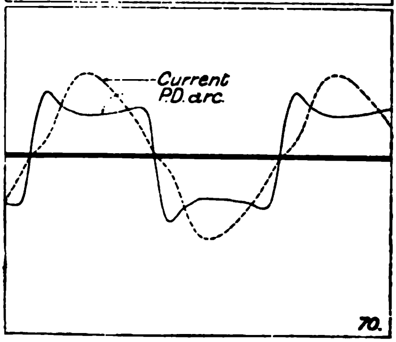
Arc length - - - - - - - - 1 mm.



Arc length - - - - - - - - 3 mm.



Arc length - - - - - - - - 6 mm.



Scales.

For P.D. Arc. - - - 1 mm. = 10 volts.
Current - - - 1 mm. = 2 amps.

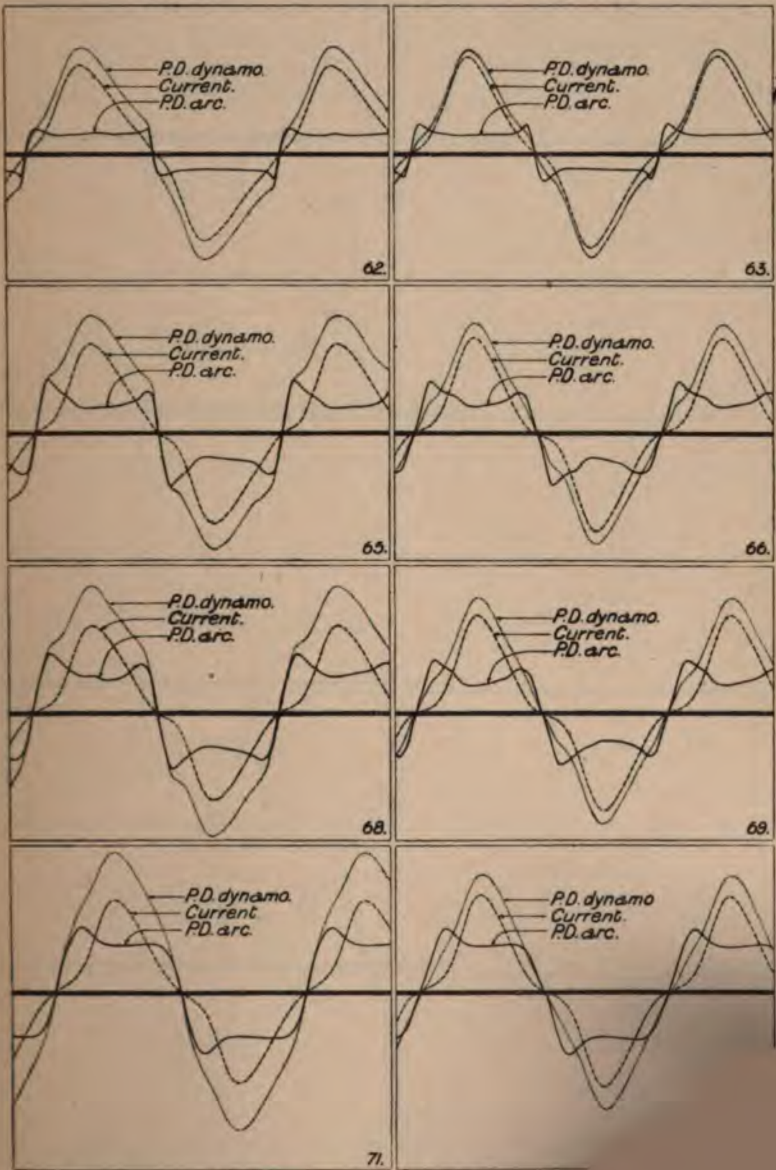
NOTE.—*These diagrams are*

SISTANCE IN SERIES.

HARRIS ALTERNATOR.

14·8 amps. Frequency, 97 ∿ per sec.

Resistance, 5·1 ohms. Resistance, 10·2 ohms.



For P.D. Dynamo - 1 mm. = 10 volts. For P.D. Dynamo

here reproduced half-size.

DIAGRAM

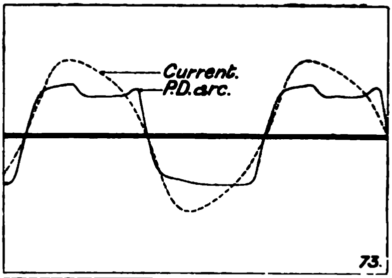
EFFECT OF RE-

ON PYKE & HARRIS

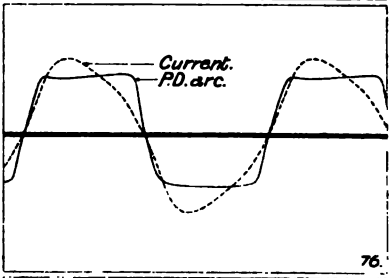
SOLID-CORED ARC. Carbons, 13 mm. "Apostle." Current,

Resistance, 0·3 ohm.

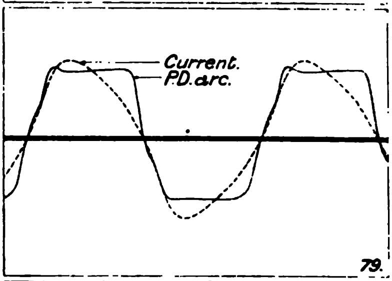
Arc length - - - - - - - - 0 mm.



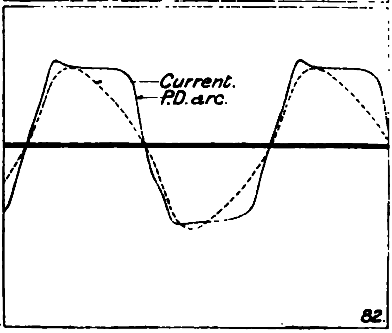
Arc length - - - - - - - - 1 mm.



Arc length - - - - - - 3 mm.



Arc length - - - - - - - - 6 mm.



Scales.

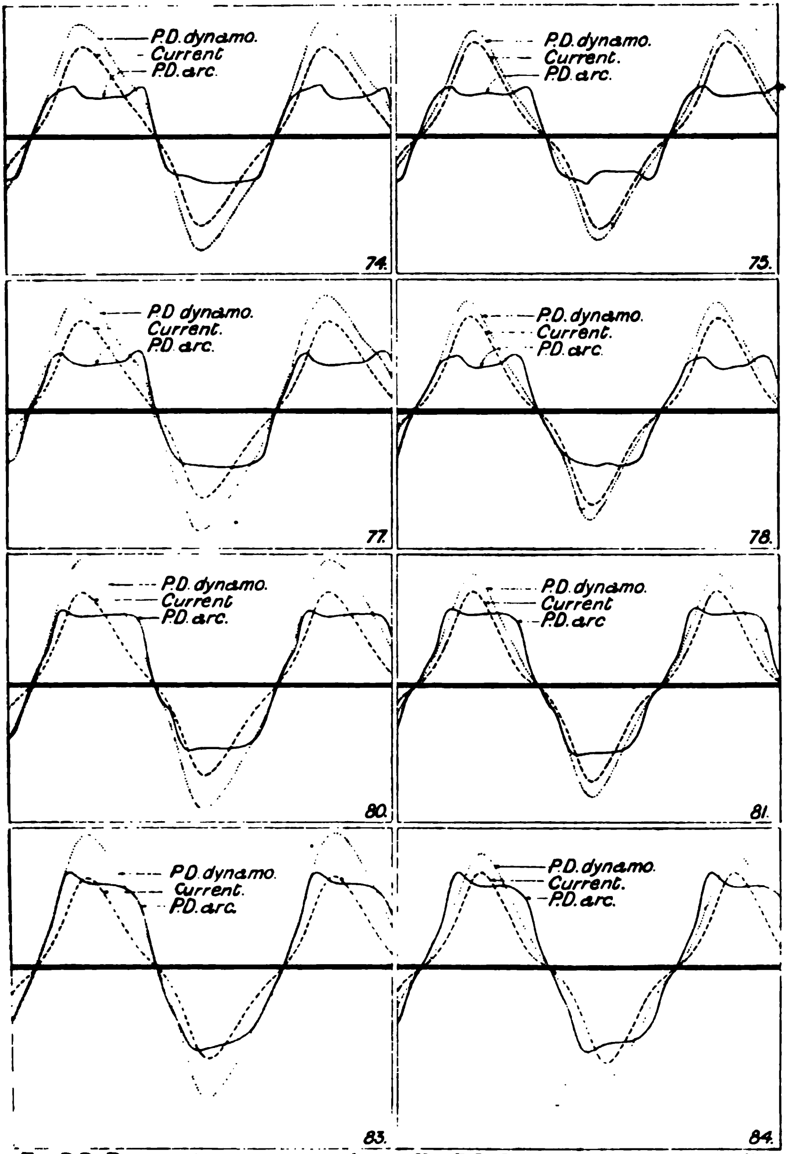
For P.D. Arc - - - 1 mm. = 5 volts.
Current - - - 1 mm. = 2 amps.

NOTE.—*These diagrams are*

ALTERNATOR.

14·8 amps. Frequency, 97 ∿ per sec.

Resistance, 5·1 ohms. Resistance, 10·2 ohms.



For P.D. Dynamo - 1 mm. = 10 volts. For P.D. Dynamo - 1 mm. = 20 volts.

here reproduced half-size.

DIAGRAM

EFFECT OF RE-

ON PYKE AND

Cored Arc

Carbons, 13 mm. "Apostle." Current.

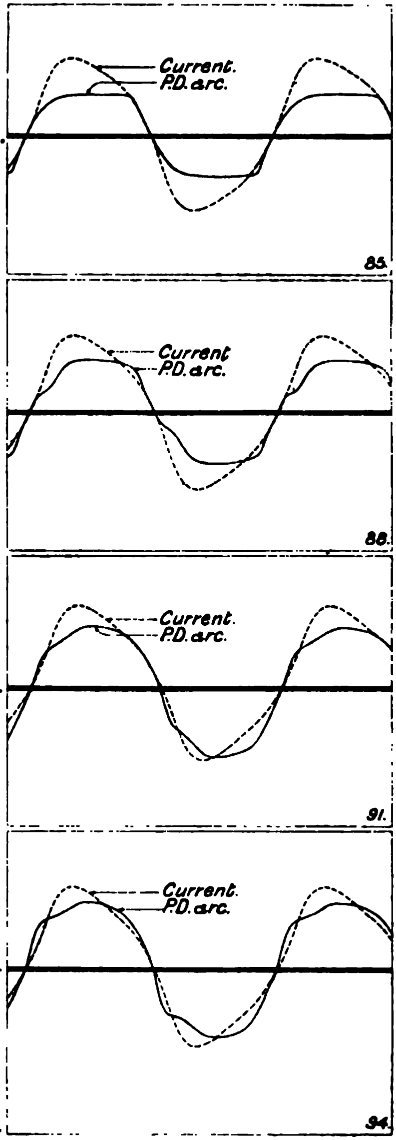
Resistance, 0·3 ohm.

Arc length - - - - - - mm.

Arc length - - - - - - - - 1 mm

Arc length - - - - - - - - 3 mm.

Arc length - - - - - - - - 6 mm.



Scales.

For P.D. Arc - - - 1 mm. = 5 volts.
Current - - - 1 mm. = 2 amps.

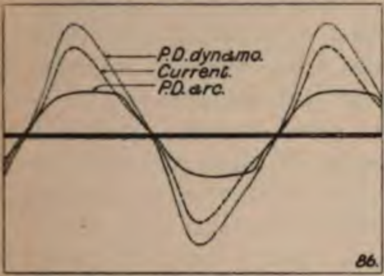
Note.—*These diagrams are*

SISTANCE IN SERIES.

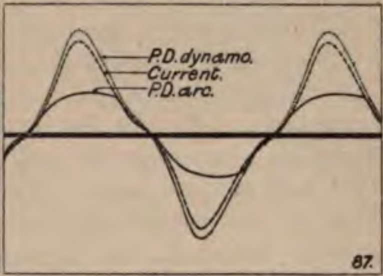
HARRIS ALTERNATOR.

14·8 amps. Frequency, 97 ∩∪ per sec.

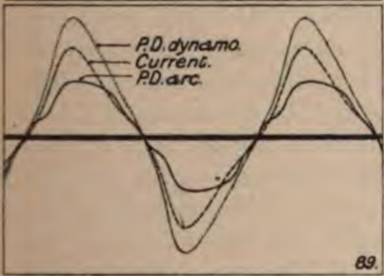
Resistance, 5·1 ohms. Resistance, 10·2 ohms.



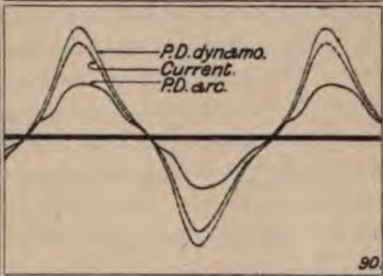
86.



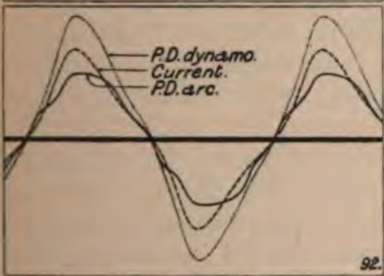
87.



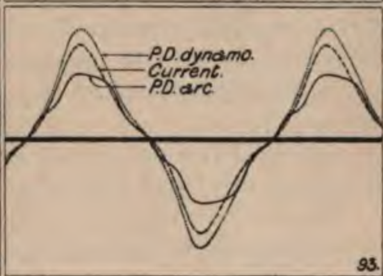
89.



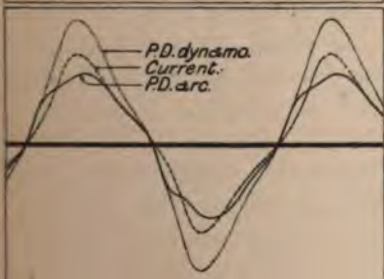
90.



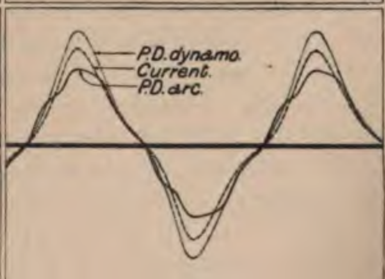
92.



93.



95.



96.

For P.D. Dynamo - 1 mm. = 10 volts. For P.D. Dynamo - 1 mm. = 20 volts.

here reproduced half-size.

DIAGRAM

EFFECT OF RE-

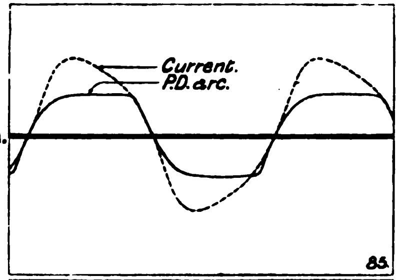
ON PYKE AND

Cored Arc

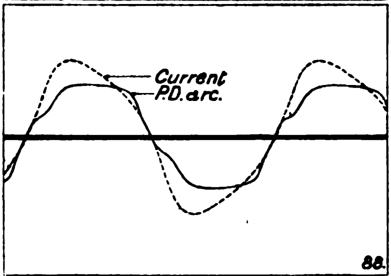
Carbons, 13 mm. "Apostle." Current,

Resistance, 0·3 ohm.

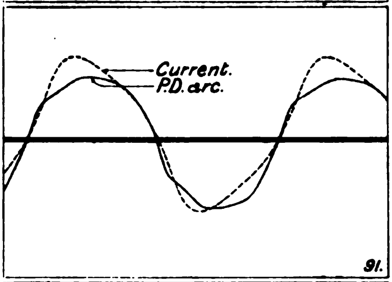
Arc length - - - - - - - mm.



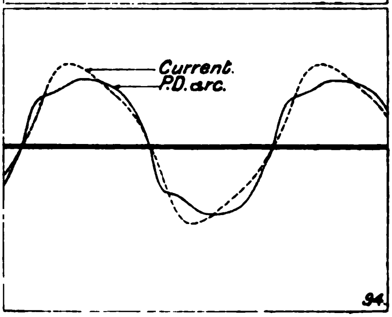
Arc length - - - - - - - 1 mm



Arc length - - - - - - - 3 mm.



Arc length - - - - - - - 6 mm.



Scales.

For P.D. Arc - - - 1 mm. = 5 volts.
Current - - - 1 mm. = 2 amps.

Note.—*These diagrams are*

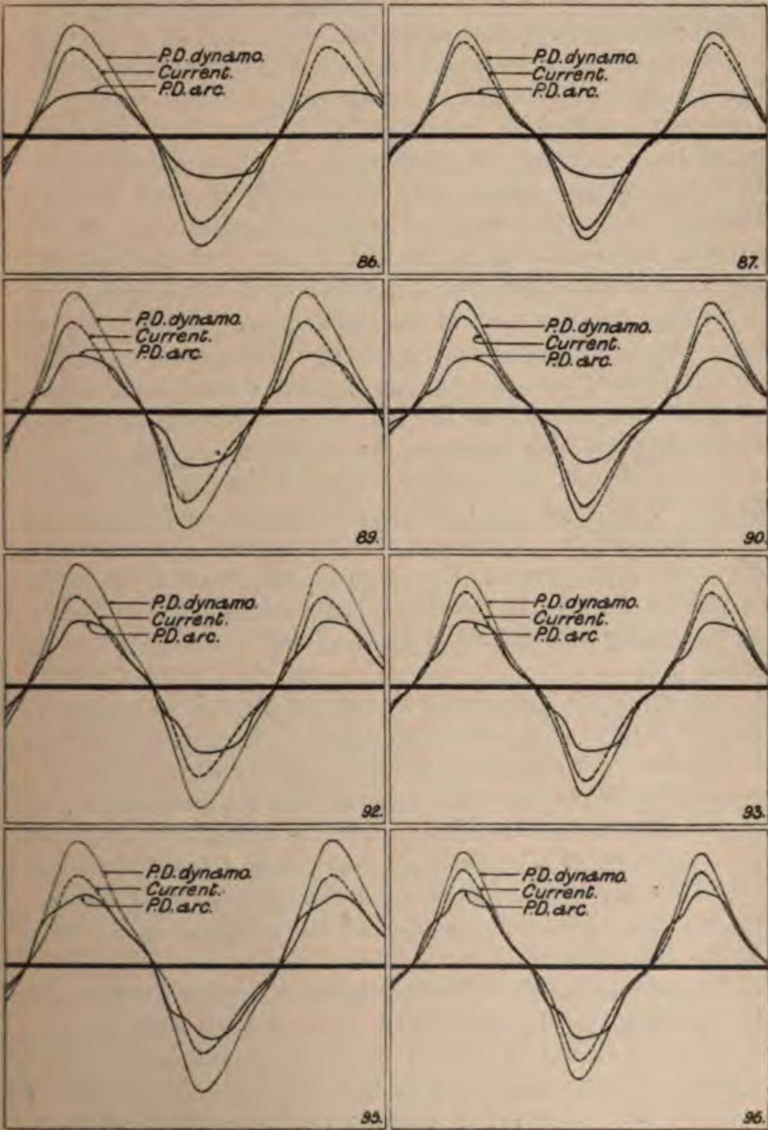
VIII.

SISTANCE IN SERIES.

HARRIS ALTERNATOR.

14·8 amps. Frequency, 97 ∿ per sec.

Resistance, 5·1 ohms. Resistance, 10·2 ohms.



For P.D. Dynamo - 1 mm. = 10 volts. For P.D. Dynamo - 1 mm. = 20 volts.

here reproduced half-size.

the time during which the current remained small (expts. 54 and 55); with the solid-cored and cored arcs, however, the effect was not very noticeable.

Experiment 54 is of interest as it was made with the arc having the lowest power factor for which we have recorded the periodic curves, the power factor being only 0·58; yet it will be noticed in the curves that there is no angle of lag, in the ordinary meaning of the term, as both the P.D. between the arc terminals and the current are zero at the same time. The idea that as the power factor is low there must be a considerable difference of phase between the P.D. and current seems still to exist in spite of all that M. Blondel has written on the subject, and in spite of the periodic curves he has published, none of which show any appreciable phase difference between P.D. and current. It is perhaps needless to add that, as yet, in all our results, we have not come across a case where the curves are sensibly out of phase; and we believe that in those cases, where previous experimenters thought they obtained a considerable angle of lag between their periodic curves of P.D. arc and current, this erroneous result was introduced by their not recording the two curves simultaneously.

Returning to experiment 54, it is to be noticed that the curve of P.D. between the dynamo terminals is also distorted by the arc to a considerable extent, the distortion being greatly reduced by increasing the resistance in series (expt. 55).

On the Pyke and Harris machine the range of resistance was from 0·3 to 10·2 ohms. (diagrams VI., VII. and VIII.). In this case, owing to the inductive nature of the machine, it was possible to burn all the arcs with 0·3 ohms. in series.

As before, increase of resistance in series with the solid arc tends slightly to increase the peaks on the P.D. arc wave, the effect being most marked in the 1 mm. arc (expts. 64, 65, 66). *The effect on the current wave was in all cases to make it more like the open-circuit wave form of the alternator used,* and this is well illustrated by the similarity of the current waves with 10 ohms. in series with the arc, diagrams VI., VII., VIII., to the open-circuit curve of the dynamo already given in Fig. 7.

We have observed a curious exception to the general rule that increase of resistance in series improves the steadiness of the arc, in the case of the 3 mm. solid-arc

(exps. 67, 68, and 69); this arc is quite steady but slightly hissing with 0·3 ohm. in series; increase the resistance however, and it hisses badly and becomes troublesome to work with. The explanation of this effect is not far to seek, for with the small resistance in series the current wave form is humpy and the arc is near the hissing point, and when the resistance is increased the current wave form becomes more like the open circuit wave of the machine, that is peaked, so that although the r.m.s. value of the current is the same, the maximum value is so much increased that the arc passes into the hissing state at the middle of each half period, thus making it hiss badly, and rendering it very troublesome to work with.

Turning next to the r.m.s. values of the P.D. between the terminals of the arc, in all cases they are reduced by increasing the resistance in series with the arc, except for the 1 mm. solid arc, this reduction in P.D. being as large as 9 per cent. for the 3 mm. solid arc (expts. 67 and 69). The slight increase of P.D. with the cored arc (expts. 58, 59, and 60) is probably due to want of uniformity of the cored carbons.

The power factors with the Ferranti alternator all tend to increase with increase of resistance in series, while those with the Pyke and Harris machine decrease for the solid and solid-cored arcs, and are almost unaffected for the cored arcs. These changes in the power factors are chiefly due to the current waves becoming more like the open-circuit curves of the alternators as the resistance in series is increased, while the P.D. waves for arcs of medium length do not alter very much.

Thus while we have not been able to produce the considerable changes in the shape of the P.D. arc wave observed by Mr. Frith, *our results confirm his conclusion that the efficiency of the alternate current arc is affected by the amount of resistance in series with it.* For change of resistance in series, we find, considerably modifies the current wave, and Messrs. Rössler and Wedding and others showed that the efficiency is dependent on the shape of the current wave form.

Mr. Frith's remark, that the arc has the power of automatically converting any wave form into the one best suited to its requirements, is not, however, borne out by our experiments (see expts. 54 and 55).

DIAGRAM

EFFECT OF

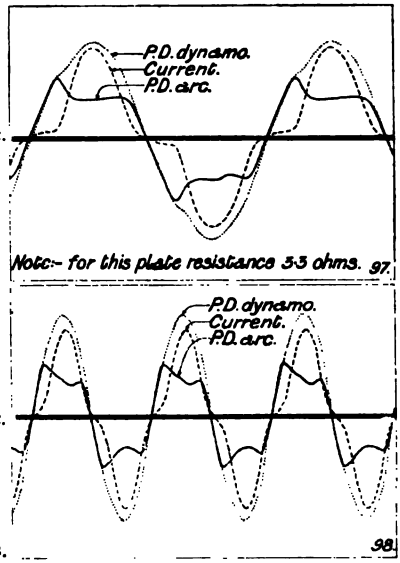
ON FERRANTI

Carbons 13 mm. "Apostle." Current,

Resistance in

SOLID ARC.

Frequency - - 100 ∿ per sec.



Frequency - - - - 200 ∿ per sec.

Scales.

For P.D. Dynamo - 1 mm. = 10 volts.
Current - - - 1 mm. = 2 amps.

For P.D. Arc - 1 mm. = 10 volts.

NOTE.—*These diagrams are*

SECTION 5.—EFFECT OF FREQUENCY.

(Diagrams and Tables IX. and X.)

A very large range of frequencies being in use in the alternating supply systems of this and other countries, and there being exceedingly little available data as to the effect of change of frequency alone on the behaviour of the arc, we carried out a series of experiments over a range of frequencies from 29·2 to 200 ∿ per sec., and this range embraces most of the frequencies that are in practical use. At the present time, in this country nearly all the public supply stations use frequencies between 50 and 100 ∿ per sec.; but there is a tendency towards lowering the frequency so as to be better able to operate alternate current motors.

To make our experiments as far as possible comparable one with the other, we brought the current back to a fixed

IX.

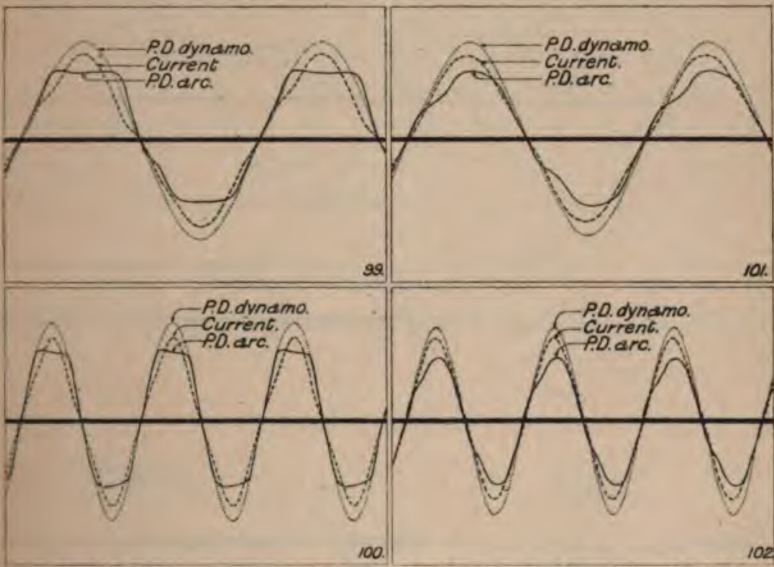
FREQUENCY.

ALTERNATOR.

14·8 amps. Length of arc, 3 mm.

series, 3·9 ohms.

SOLID-CORED ARC. CORED ARC.



For P.D. Arc - 1 mm. = 5 volts. For P.D. Arc - 1 mm. = 5 volts.

here reproduced half-size.

r.m.s. value by varying the dynamo excitation after each change of frequency, while the arc length and all the other conditions of the circuit remained unchanged.

To obtain the range of frequency from 29·2 to 200 ∿ per sec., we were obliged to use both the alternators, the results for the range 29·2 to 127 ∿ per sec. (diagram and table X.), were, however, obtained on one single machine and arc, thus strictly comparable one with the other, while the results for 100 and 200 ∿ per sec. (diagram and table IX.), though not obtained on the same alternator, lead us to the same general conclusions. It must be borne in mind when examining the wave forms, that the scale of time has been kept constant (except in expt. 115), so that at low frequencies the wave forms are very much drawn out.

The chief effect of reducing the frequency is, in all cases, *not only greatly to increase the actual time during which the*

DIAGRAM

EFFECT OF

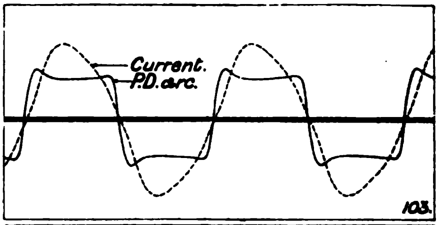
ON PYKE AND

Carbons, 13 mm. "Apostle." Current,

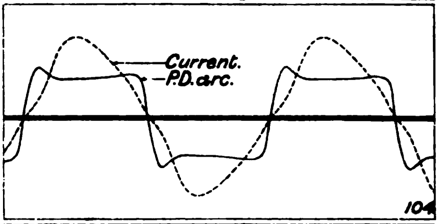
Resistance in

SOLID ARC.

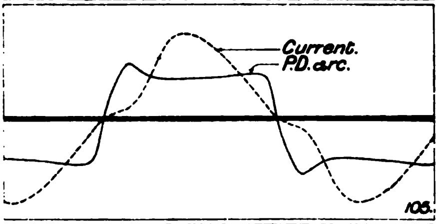
Frequency - - 127 ∿ per sec.

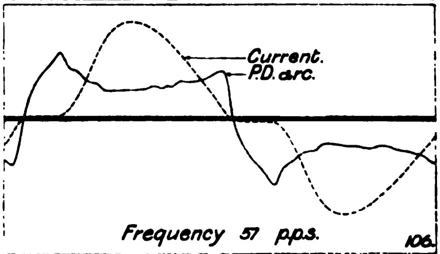


Frequency - 97 ∿ per sec.



Frequency - - 70 ∿ per sec.





NOTE.—*These diagrams are here reproduced half-size.*

Scales.

For P.D. Arc Solid Arc - - - - - - - - - - 1 mm. = 10 volts.
P.D. Arc Solid-Cored Arc and Cored Arc - 1 mm. = 5 volts.
Current - - - - - - - - - - - - - - 1 mm. = 2 amps.

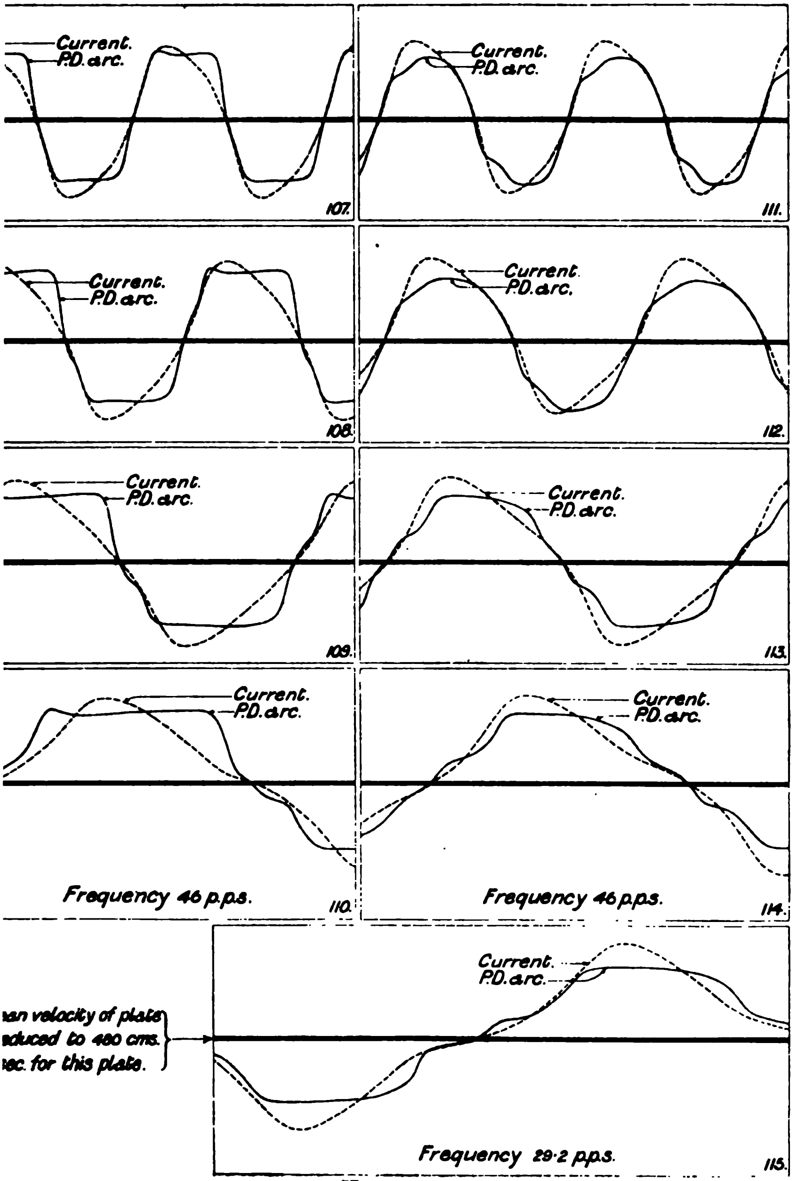
'ENCY.

ALTERNATOR.

:. Length of arc, 3 mm.

ohm.

SOLID-CORED ARC. CORED ARC.



current remains small in each period, but also to increase the fraction of the period during which it is so. Accompanying this there is, as we should expect, seeing that we have kept the r.m.s. value of the current constant, a considerable increase in the maximum value of the current with decrease of frequency, and this increase of the maximum value has caused the low frequency *solid* arc to hiss badly and become so unstable that we were unable to obtain results below a frequency of 57 ∿ *per sec.* The *solid-cored* arc was slightly unstable at 46 ∿ *per sec.* The *cored* arc, however, did not become unstable at 29·2 ∿ *per sec.*, the lowest frequency we tried; but the light of the arc was exceedingly unpleasant to the eyes at this low frequency.

When the r.m.s. value of the current, the length of the arc, &c., are kept constant, lowering the frequency increases the r.m.s. value of the P.D. for the solid arc, but reduces it for the cored arc, the effect with the solid-cored arc being apparently *nil*.

The power factor in all cases which we have tried was reduced by lowering the frequency.

In one of his first papers on the alternate current arc, M. Blondel stated that, as the current had a small value for a long time with low frequency arcs, he expected that the efficiency would be smaller with low frequencies than with high ones. Experiments,[1] however, made subsequently, convinced him that the opposite was the case. This conclusion was also arrived at by Professor Fleming and Mr. Petavel,[2] and is borne out by some experiments made by Messrs. Cameron and Hecht, at the Central Technical College, who measured the luminous efficiency of some of the arcs of which we give the wave forms. For the latter found that with the solid arc a reduction of frequency from 127 ∿ per sec. (expt. 103) to 57 ∿ per sec. (expt. 106) increased illumination per watt by 50 per cent., and that for the cored arc a reduction of frequency from 127 ∿ per sec. (expt. 111) to 46 ∿ per sec. (expt. 114) increased the illuminaiton per watt by 10 per cent. Thus, *as far as efficiency is concerned a reduction of frequency is very advantageous;* there is, however, a practical limit to this reduction due to the instability of the arc with low frequencies.

[1] *The Electrical World* of New York, 1897, vol. 29, pp. 232, 258.
[2] *Proceedings of the Physical Society*, 1895, vol. 14, p. 247.

SECTION 6.—ARCS ON TRANSFORMERS.

(Diagrams and Tables XI. and XII.)

One very important question was, whether, when the arc was supplied by the secondary of a transformer, the reactions on the wave forms would still take place, and if so, whether they would be transmitted to the primary circuit. To test this we used a small equal ratio *constant P.D.* (Westinghouse) transformer.

With the non-inductive Ferranti alternator a resistance of 2·4 ohms was necessary in series with the primary of the transformer as a steadying resistance, while with the high inductance Pyke and Harris machine no resistance at all was needed. In both cases the secondary of the transformer was connected to the arc, with only the usual measuring instruments in series (*see* Fig. 9 *ante*).

As more wave forms were required than we could record simultaneously, the curves were obtained in two sets, given one below the other in the diagram, the P.D. dynamo curve being included in both sets as a reference curve. As with the highly inductive machine, the only resistance in series with the transformer primary was that of the measuring instruments, the P.D. dynamo and P.D. between the primary terminals almost coincide, we have not therefore recorded this latter curve for the solid-cored and cored arcs in row four.

We find that the reaction of the arc on the wave forms of the circuit, when supplied with current by the secondary of a constant P.D. transformer (expts. 116, 118, 120, 122, 124, and 126), is similar to what we have found when the circuit is supplied by the alternator direct, and that the characteristic forms of the P.D. arc and current waves *are transmitted through this transformer to its primary circuit*, so that the P.D. and current curves for the two circuits are very similar (*see* diagram XI.).

One point of difference which we noticed with the solid arc (expt. 116) is that while in the secondary circuit the current curve keeps along the zero line for a time as usual, in the primary (expt. 117) the curve approaches the zero at a small angle, this being due to self-induction.

These experiments led us to think that any distortion of

DIAGRAM
ARC BURNING ON A

Ratio of trans-

Carbons, 13 mm. "Apostle." Current

Resistance in series

Solid Arc.

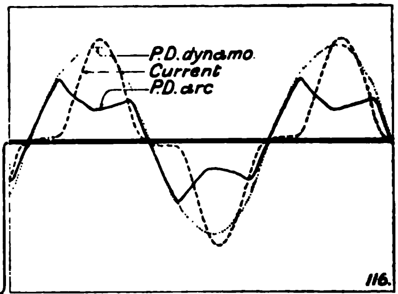
On Ferranti Alternator.

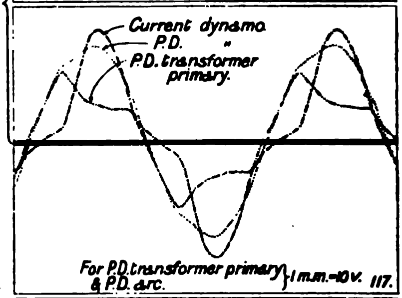
Frequency, 100 ∿ per sec.

Resistance in series with primary of transformer 2·4 ohms.

Scales.

For P.D. Dynamo - 1 mm. = 5 volts.
Current in primary of transformer and in arc - - - 1 mm. = 2 amps.





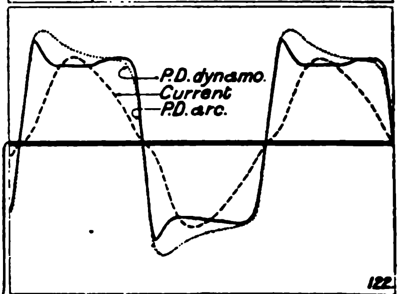
On Pyke & Harris Alternator.

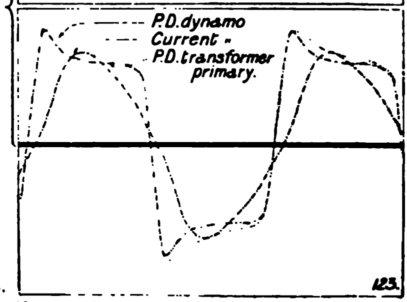
Frequency, 97 ∿ per sec.

Resistance in series with primary of transformer - 0·2 ohm. (leads, etc.).

Scales.

For P.D. Dynamo - } 1 mm. = 5 volts.
P.D. Transformer primary - - }
P.D. Arc - - - }
Current in primary of transformer and in arc - - - - 1 mm. = 2 amps.



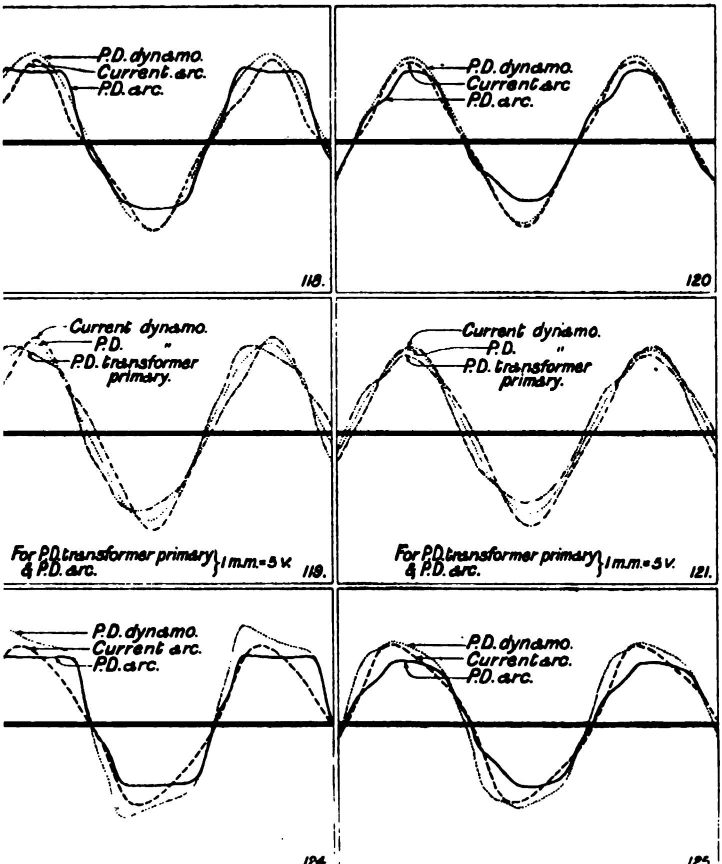


tion, 1 : 1.

, 14·8 amps. Length of arc, 3 mm.

ırc 0·2 ohm.

SOLID-CORED ARC. CORED ARC.



NOTE.—*These diagrams are here reproduced half-size.*

DIAGRAM XII.

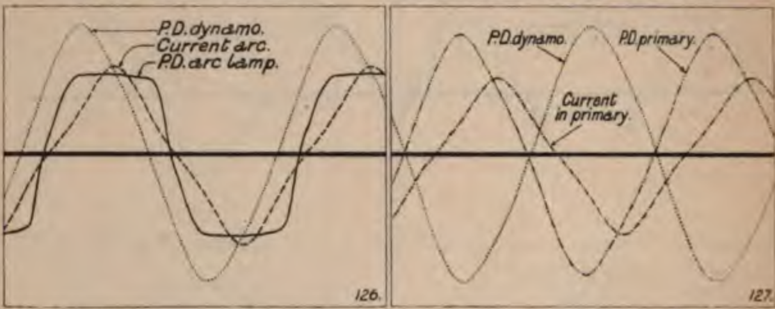
BRUSH ARC LAMP ON A MORDEY VICTORIA TRANSFORMER.

Supplied with high tension current by transforming up from Ferranti Alternator.

Frequency, 93 ∿ per sec.

Carbons, 15 mm. "Conradty," both Cored. Arc length, 2·2 mm.

Current in arc, 14·7 amps. Resistance in series, with lamp, 0·2 ohm.



NOTE. –*These diagrams are here reproduced half-size.*

Scales.

For P.D. Dynamo - - - -	1 mm. =	5 volts.
P.D. Arc Lamp - - -	1 mm. =	4 volts.
P.D. Primary - - - -	1 mm. =	200 volts.
Current in Arc - - - -	1 mm. =	2 amps.
Current in Primary - -	1 mm. =	$\frac{1}{15}$ amp.

the secondary wave forms would be approximately reproduced on the primary curves; but subsequent tests made with another transformer showed that this was not always the case.

In these experiments a differential (Brush) arc lamp and an approximately *constant current* (Mordey) transformer were used, the transformer being designed to supply a single arc from a 2,000 volt circuit.

The transformer and the arc lamp are those in general use for the street lighting of an important English town, and our experiments were arranged to produce the ordinary conditions of working. The results are given in diagram and table XII.

The reaction on the P.D. between the terminals of the arc lamp and the current wave forms, is what we should expect for the carbons used, the difference in phase between the curves being due to the self-induction of the regulating coils of the lamp. The distortion of the P.D. wave form is,

however, *not transmitted to the primary circuit* (expts. 126, 127).

This important difference in the transmission of the distortion from the secondary to the primary circuits arises from the fact that the constant-current transformer has necessarily a much greater magnetic leakage than that intended for constant P.D.

In both cases, however, *the arc reacts on the P.D. wave between its terminals and the current in the ordinary way.*

SECTION 7.—ARCS BETWEEN DIFFERENT MAKES OF COMMERCIAL CARBONS.

(Diagrams and Tables XIII. and XIV.)

To ascertain the part played by the nature of the carbons in the reaction of the arc on the wave form, we tried a large variety of those in general use, and we give here a few of the results.

All the experiments were made on the same alternator, and with the same excitation, those in XIII. having a constant resistance, and those in XIV. a constant self-induction in series with the arc, the same combinations of carbons being used in both cases.

The characteristic reactions of the arc on the wave forms are seen to occur in all cases, being modified, however, in amount by the nature of the carbon, and by the presence of impurities.

With the resistance in series the Conradty (Norris) carbons seem to distort the wave forms least, whilst with self-induction in series the Brush carbons have perhaps the smallest effect. On the other hand, the most rectangular P.D. arc wave which we have obtained for cored carbons is that with Carré carbons, and the power factor was lower than that obtained with any other cored arc of the same length and current. From this we conclude that the Carré cored carbons are probably very free from foreign bodies, since as shown later, foreign substances tend to reduce the reaction and raise the power factor of the arc.

We conclude from these experiments that although the amount of the reaction of the arc on the wave forms varies to a certain extent, from one make of carbons to another,

DIAGRAM

ARCS BETWEEN DIFFERENT

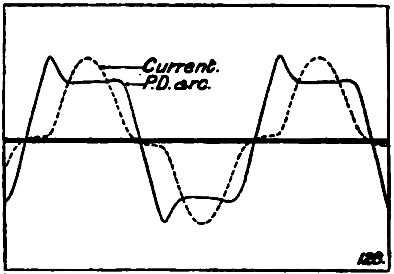
ON FERRANTI

Carbons, 13 mm. diameter. Resistance in

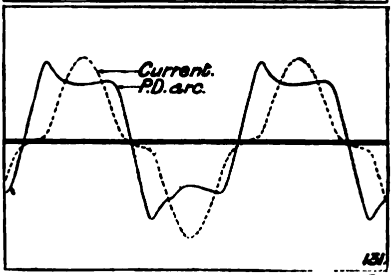
SOLID ARC.

Make of Carbons.

CONRADTY (NORIS).



BRUSH



CARRÉ.

Scales.

For P.D. Arc - - - 1 mm. = 6 volts.
Current - - - 1 mm. = 2 amps.

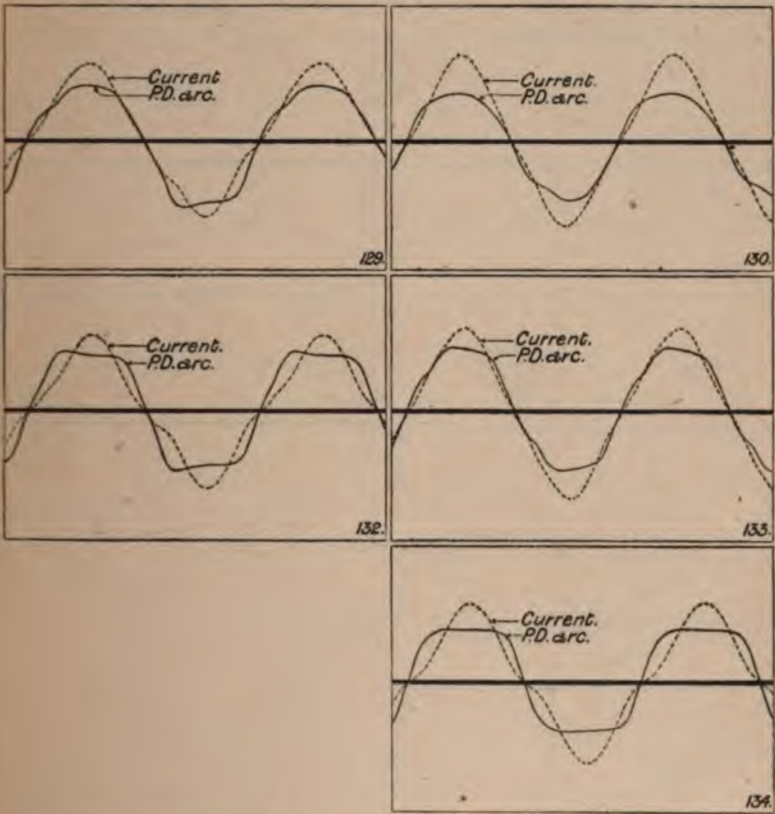
NOTE.—*These diagrams are*

yet the main features are in all cases so similar, that we can deduce what will be the effect of the change of any variable on the reaction of one make of carbons from the known effect on the reaction of another make. It is well-known that any change in wave form changes the impedance of an inductive coil, and as the shape of the wave depends on the

ALTERNATOR.

series, 3·64 ohms. Frequency, 100 ∿ per sec.

SOLID-CORED ARC. CORED ARC.



here reproduced half-size.

nature of the carbons, we might expect the impedance of an inductive coil in series with an arc to depend on the kind of carbons in the arc; and that this is the case is evident from the observed values of the impedance of the choking coil used in expts. 135 to 141 (see also expts. 5 to 8 *ante*). This change of impedance, though small, may affect the design of the regulating coils for arc lamps.

DIAGRAM

ARCS BETWEEN DIFFERENT

ON FERRANTI

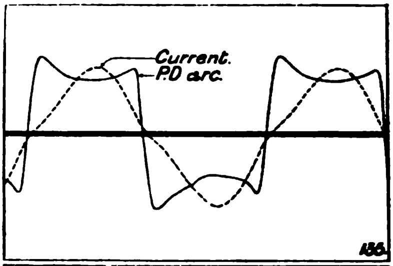
Carbons, 13 mm. diameter. Resistance in

Self-induction

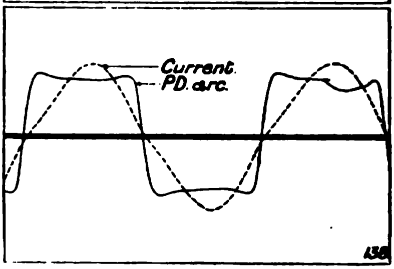
SOLID ARC.

Make of Carbons.

CONRADTY (NORIS).



BRUSH.



CARRE.

Scales.

For P.D. Arc. - - - 1 mm. = 6 volts.
Current - - - 1 mm. = 2 amps.

NOTE.—*These diagrams are*

SECTION 8.—ARCS BETWEEN ONE SOLID CARBON AND ONE CARBON CORED WITH A FOREIGN SUBSTANCE.

(Diagrams and Tables XV. and XVI.)

The great effect which the core produces on the reaction of the arc on the wave form, led us to hope that it would be possible to find some substance for the core which, while reducing the effect of the arc on the wave forms, would not impair the illuminating power of the arc.

XIV.

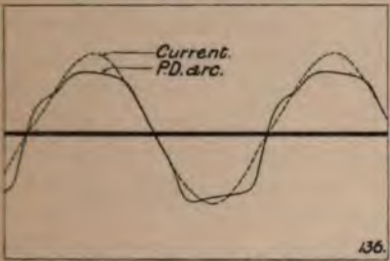
MAKES OF CARBONS.

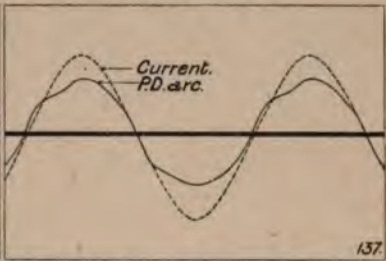
ALTERNATOR.

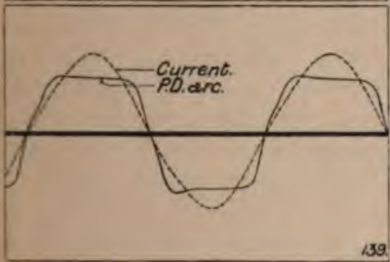
series, 0·25 ohm. Frequency, 100 ∿ per sec.

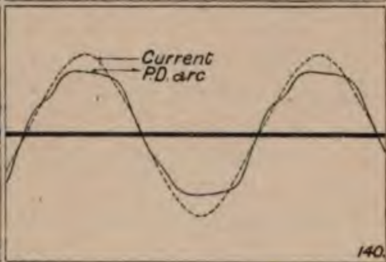
in series, 7·6 10^{-3} h.

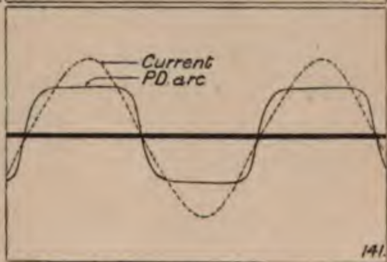
CORED-SOLID ARC. | CORED ARC.











here reproduced half-size.

We have experimented with a large number of refractory substances, chiefly oxides and silicates, these being put in the place of the core in a carbon whose core had been drilled out. These experiments, though so far unsuccessful in their main object, gave some interesting results.

The experiments in diagram and table XV. were made with constant dynamo excitation, and resistance in series, and with a current of approximately 15 amps., and the more interesting results were repeated with our usual

current of $14{\cdot}8$ amps. and arc length of 3 mm. for purposes of comparison—see diagram and table XVI. A description of the behaviour of each arc is given in the notes to the tables.

Experiment 159 was with an arc between a solid and hollow carbon; the latter being made by drilling out the core of a cored carbon; yet, although the core was completely removed, the P.D. arc wave differs from that usually obtained with both solid carbons, under the same conditions, in that there is only a slight trace of the front peak.

If now we introduce a foreign substance into the hollow carbon, the areas of the parts of the curves on the two sides of the zero line are generally no longer equal, and in most cases the two parts are quite dissimilar in shape.

With metallic copper as a core (expt. 150) the P.D. arc wave has a very high front peak, which is considerably larger when the metal is positive; there is also a considerable part of the period during which the current is inappreciable.

On filling the core with a piece of soda-glass rod (expt. 157), we were surprised to find that the glass, instead of hindering the burning of the arc, made it possible to maintain an arc of given length and current at a much lower P.D., and in fact it was found that a 1 mm., 15 amp., arc only required an r.m.s. P.D. of 15 volts. This arc burnt with a brilliant sodium flame, and we thought it would be interesting to find out whether the low P.D. was due to the presence of sodium or silica, in the arc. On trying silica in the form of white sand no appreciable effect was observed; but with common salt (expt. 156) the P.D. between the carbons was greatly reduced, though not to the same extent as with the soda-glass. This seemed to show that the effect was due to the presence of a salt of sodium, an opinion that is borne out by the fact that all the compounds of sodium, which we tried, reduced the P.D., while, further, powdered combustion tube (soda-potash glass) behaved in a very similar manner.

To test whether small quantities of sodium or potassium salts, introduced into the arc, produced a marked effect on the wave forms and the r.m.s. value of the P.D., we soaked a pair of 13 mm. solid carbons in a 4 per cent. solution of sodium carbonate for 24 hours, and then rinsed and

thoroughly dried them. The arc between these carbons when first started burnt with a strong sodium flame, and only required a very small P.D. At the end of half an hour the brilliancy of the sodium flame was considerably lessened; and 45 minutes after striking the arc, the curves (expt. 166) were recorded and the readings taken; as the arc seemed to have then attained a steady condition. In this case the P.D. required to maintain the arc was more than 10 volts less than that which was required for an arc of the same length and current, between the same carbons before soaking, and the power factor was increased from 0·74 to 0·89 by the soaking.

With carbons soaked in potassium carbonate (expt. 167), almost identical results were obtained, except that the arc could not easily be distinguished by eye from an ordinary carbon arc.

To make certain that the effects were due to the salts and not to the soaking, we also soaked a pair of carbons in ordinary tap water and dried them as before; but without effect on the curves or the r.m.s. value of the P.D.

It is thus possible considerably to reduce the P.D. necessary to maintain an arc, of given length and current, without affecting the appearance of the arc, by treating the carbons with a potassium salt, and it seems more than probable that the low P.D. and steady burning properties of the cored arc are largely due to the presence of potassium silicate in the core. A remaining trace of this salt probably accounts for the low P.D. required for the arc between a solid and a hollow carbon, which hollow carbon was made by drilling out the core of a cored carbon.

When foreign substances, which are not in, or reduced to, the metallic state, are present in the arc, they produce a reduction of the P.D. between the carbons, for a given length and current, and generally reduce the reaction on the wave form and increase the power factor.

We have already mentioned the fact, in the early part of our paper, that a new pair of carbons when first used, always require a smaller P.D. between them, for a given length and current, than when they have reached the stable condition; this is therefore clearly due to slight traces of impurities, which volatilise as the carbons burn into shape, and the probable presence of potassium silicate in the core

DIAGRAM

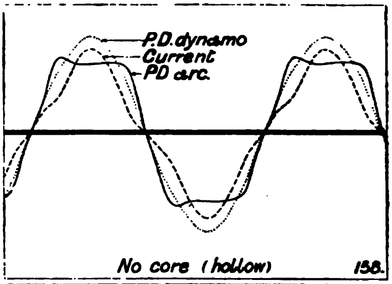
ARCS BETWEEN ONE SOLID CARBON AND ONE

Top, 13 mm. solid

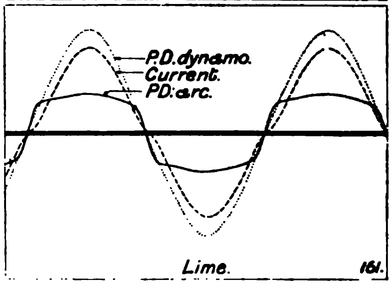
Bottom, 13 mm. "Apostle" carbon

ON FERRANTI

(*b*) Constant arc conditions. Arc length, 3 mm.
Resistance in series varied.



Nature of core - - - - No core (hollow) 158.



Nature of core - - - - - Lime. 161.

Scales.

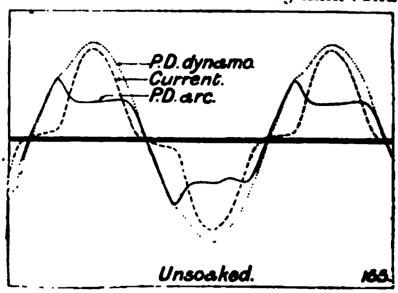
For P.D. Dynamo - 1 mm. = 10 volts.
P.D. Arc - - - 1 mm. = 5 volts.
Current - - - 1 mm. = 2 amps.

EFFECT OF SOAKING

Both carbons 13 mm. solid

Scales.

For P.D. Dynamo - 1 mm. = 10 volts.
P.D. Arc - - - 1 mm. = 5 volts.
Current - - - 1 mm. = 2 amps.



Unsoaked. 165.

NOTE.—*These diagrams are*

XVI.

CARBON CORED WITH A FOREIGN SUBSTANCE.

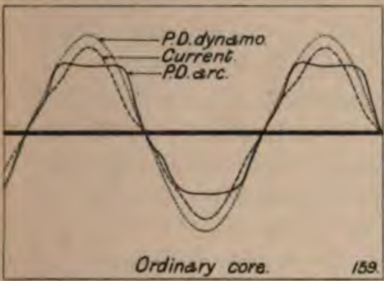
"Apostle" carbon.

cored with a foreign substance.

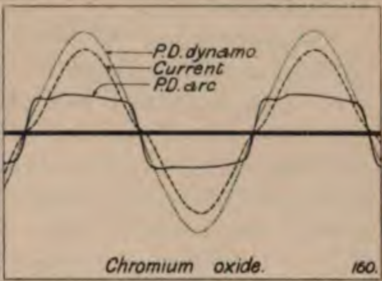
ALTERNATOR.

Current, 14·8 amps. Frequency, 100 ∿ per sec.

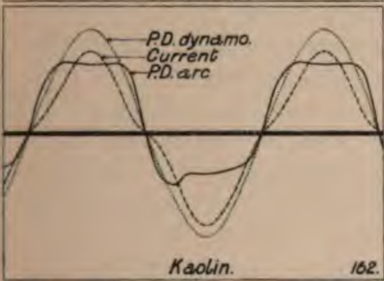
Top curve is top carbon positive.



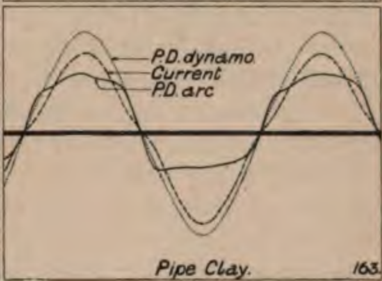
Ordinary core. 159.



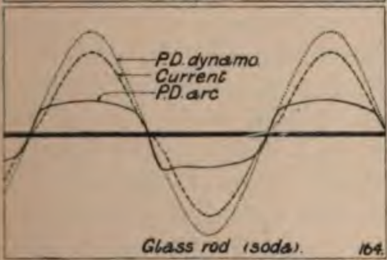
Chromium oxide. 160.



Kaolin. 162.



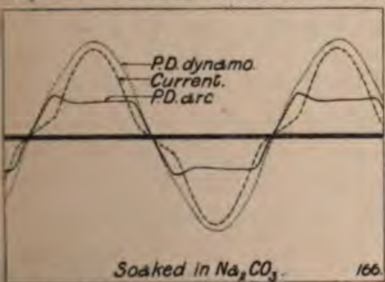
Pipe Clay. 163.



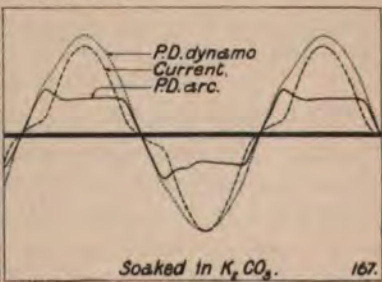
Glass rod (soda). 164.

CARBONS IN AQUEOUS SOLUTIONS.

"Apostle" carbons; other conditions same as before.



Soaked in Na_2CO_3. 166.



Soaked in K_2CO_3. 167.

here reproduced half-size.

also explains the reason why the effect is more marked with cored carbons.

When considering the methods of obtaining the wave forms, we alluded to the fact that one wave may differ from the next, and this is well exemplified in expts. 152, 154, and 155 of diagram XV.

The appearance of the arc always shows the flame colouration characteristic of the substance of which the core is composed, and very brilliant effects were produced by some of the substances used. In particular a very curious effect was obtained with chromium oxide; the oxide was apparently reduced to metal in the crater, and ejected in the form of drops, which burnt with brilliant scintillations in falling. It seems as if some of the highly coloured arcs might be employed for purposes of ornamental illumination, and the sodium flame of the arc with the glass-rod core has already proved very useful in spectroscopic work.

It must be borne in mind, however, that foreign substances introduced into the arc probably reduce the temperature of the craters, and thereby lessen the luminous efficiency; judging by eye the illumination produced by most of the arcs containing foreign bodies, as compared with that produced by the solid arc, we should say that the reduction in luminous efficiency is very large.

In many cases, near the crater of the solid carbon we observed small bright spots in rapid motion, notably with alumina, baryta, strontia, lime, and kaolin.

We carefully observed in all cases whether any incandescence of the core occurred, but we were not able to detect any, even in those substances which are known to become incandescent in the oxy-hydrogen flame; this, however, is probably due to the superior brilliancy of the arc masking the incandescence of the core.

SECTION 9.—ARCS BETWEEN CARBONS AND METALS.

(Diagram and Table XVII.)

Many investigators have noticed curious results when burning arcs between carbons and metals, notably Messrs. Jamin and Maneuvrier, Dr. Sahulka, and M. Arons. We have, therefore, recorded the wave forms of some of these arcs to elucidate their results.

To make the conditions as simple as possible the carbons and metals used were of the same diameter (namely 6 mm.), and all the results were obtained on a circuit having very small self-induction. Owing to the melting of the metals the arcs were of such an exceedingly transitory nature, that a determination of the arc length and the r.m.s. values of the electrical quantities was impracticable.

The curves which are very singular in shape may be broadly divided into three main types. The first type (expts. 172, 174, 176, &c.) occurs with very short arcs, generally less than 0·5 mm., and we think it is produced when particles of carbon are deposited on the metal. The second type is found when the arc is longer, about 0·5 to 1·0 mm. (expts. 170, 173, 175, &c.), and is by far the most general form of the curves. The third type occurs only in a few cases, notably with zinc, iron, and phosphor bronze.

When the current is zero, the P.D. arc and P.D. dynamo curves coincide, so that during this time our three wave forms are reduced to the P.D. dynamo curve, which is identical with the P.D. arc curve, and the zero line which is also part of the current curve.

For all the metals except zinc and tin we give in the diagrams two sets of periodic curves, one for a short arc, and the other for as long an arc as we could maintain. In the case of zinc we give four sets of curves; three of these are of the general types mentioned above, while the fourth (expt. 177) shows how the arc went out. With tin, owing to its low melting point, it was difficult to record any waves at all, and the only result which we obtained (expt. 182) is very curious, as the arc seems to have gone out and re-lit itself again one period later.

Our wave forms establish the fact that, in most cases of an arc between carbon and metal (see curves type 2), there is a complete interruption of the current over more than half the period, this interruption occurring when the carbon is positive, the P.D. between the electrodes attaining a high value during the stoppage of the current.

Thus our wave forms explain the observations of Dr. Sahulka, who pointed out that in addition to the alternating P.D. there seemed to exist a P.D. in a constant direction between the electrodes, and of M. Arons, who found that a larger current flowed if the metal was positive.

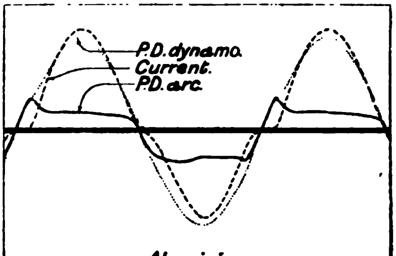
DIAGRAM

ARCS BETWEEN

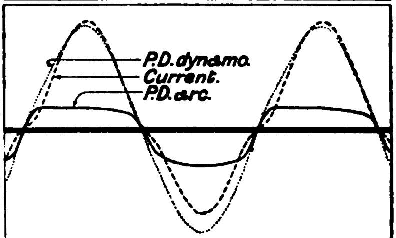
ON FERRANTI

Top Electrode Wire of Metal, 6 mm. diameter. Resistance in

Bottom Electrode, solid carbon, 6 mm. diameter.



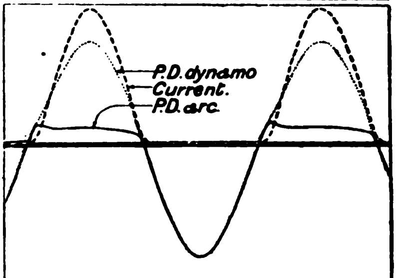
Aluminium
Arc Length 1·5 m.m. 168.



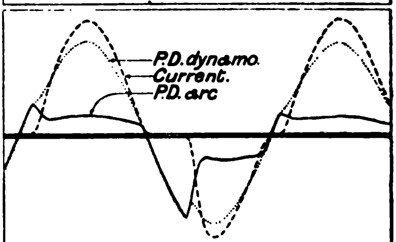
Aluminium.
Arc Length 3 m.m. 169.

Scales.

For P.D. Dynamo - 1 mm. = 10 volts.
P.D. arc. - - - 1 mm. = 10 volts.
Current - - - 1 mm. = 2 amps.



Iron. Type 2. 170.



Iron. Type 3.
{*Note:— For Iron resistance in series was reduced to 3·2 ohms.*} 171.

NOTE.—*These diagrams are*

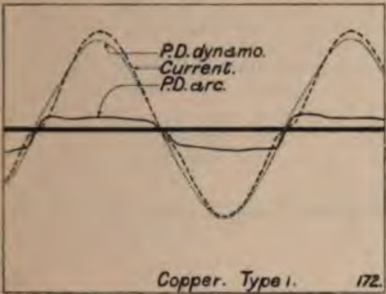
XVII.

CARBON AND METALS.

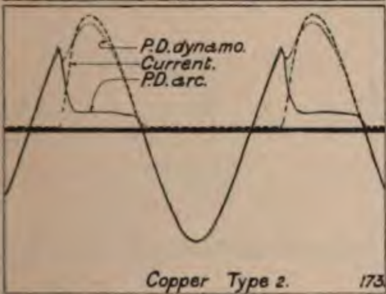
ALTERNATOR.

series, 3·9 ohms. Frequency, about 100 ∿ per sec.

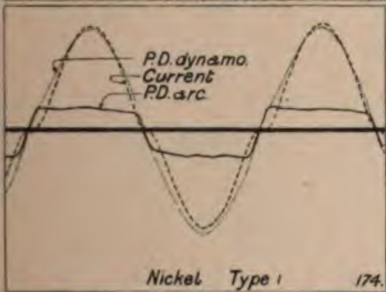
Top curve is Metal Positive.



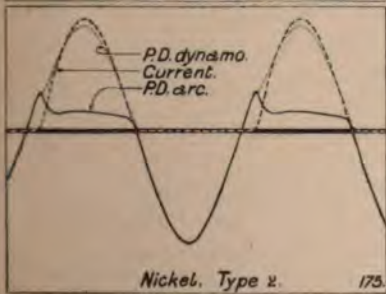
Copper. Type 1. 172.



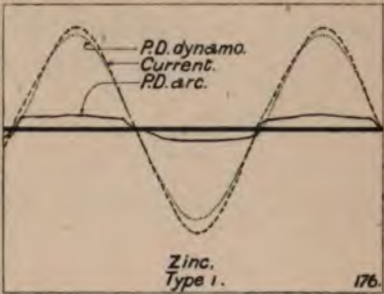
Copper Type 2. 173.



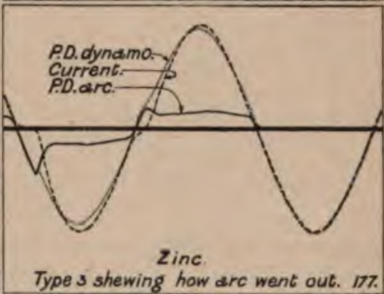
Nickel Type 1 174.



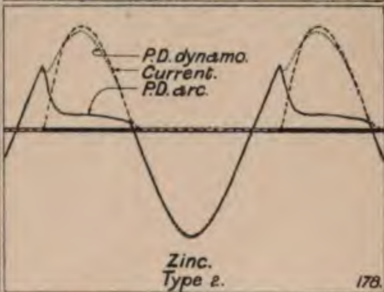
Nickel. Type 2. 175.



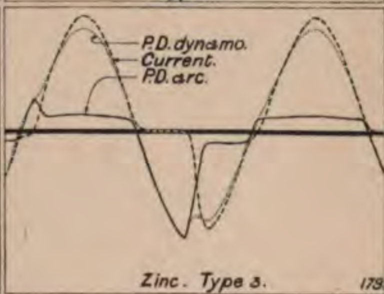
Zinc. Type 1. 176.



Zinc Type 3 shewing how arc went out. 177.



Zinc. Type 2. 178.



Zinc. Type 3. 179.

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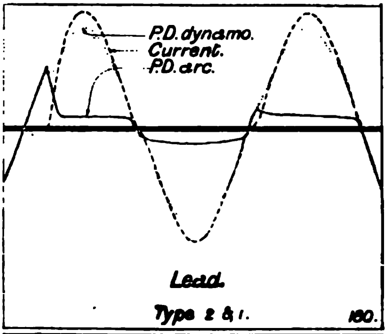
DIAGRAM

ARCS BETWEEN

ON FERRANTI

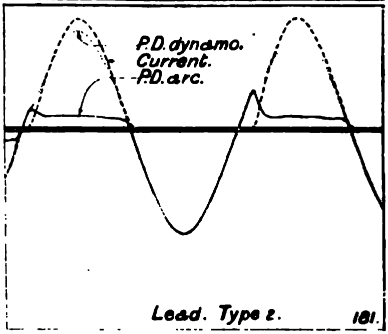
Top Electrode Wire of Metal, 6 mm. diameter. Resistance in

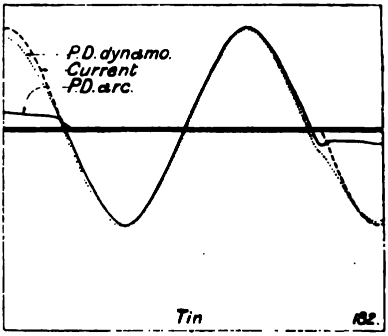
Bottom Electrode, solid carbon, 6 mm. diameter.



Scales.

For P.D. Dynamo - 1 mm. = 10 volts.
P.D.Arc. - - - 1 mm. = 10 volts.
Current - - - 1 mm. = 2 amps.





NOTE.—*These diagrams are*

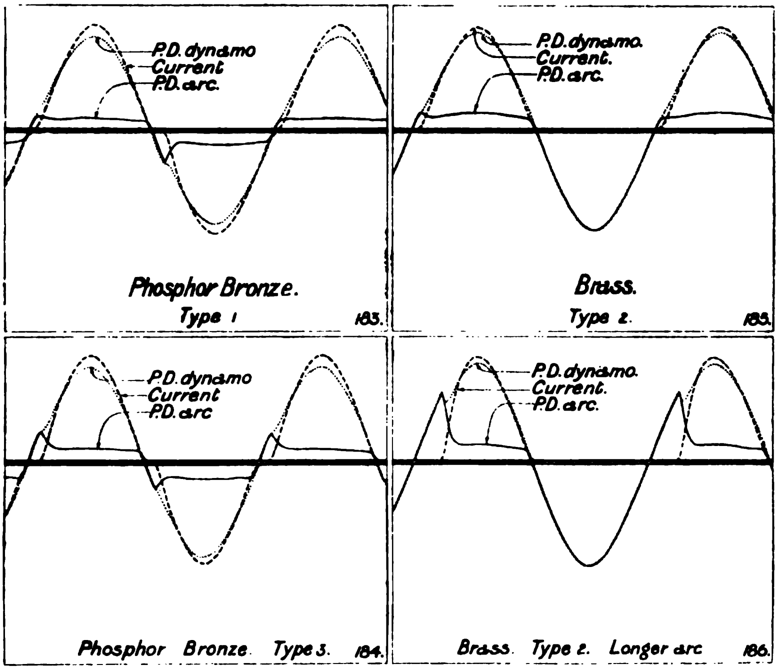
XVII.—(*continued*).

CARBON AND METALS.

ALTERNATOR.

series, in 3·9 ohms. Frequency, about 100 ∿ per sec.

Top curve is Metal Positive.



here reproduced half-size.

There is a striking similarity between some of our curves in diagram XVII. and those obtained by Messrs. Archbold and Teeple[1] in their experiments on the alternating arc between a metallic ball and a metallic point, the ball corresponding in its effect on the wave forms to our metal electrode.

This suggests that the more rapid cooling of one electrode facilitates the flow of the current from it, and resists the flow towards it. And this conclusion is borne out by the fact that, enlarging the diameter of one carbon, in an arc between carbon electrodes, has the same kind of effect as changing the carbon for a metal. A still more direct confirmation of our supposition is given by the experiment of Messrs. Cross and Shepard,[2] who cooled the positive crater of a direct current arc, and found a considerable drop in the P.D. between the carbons.

It has been suggested that the arc is an electrolytic phenomenon, and if this is so, it seemed probable that a similar distortion of the wave forms would be produced if the metal and carbon electrodes were immersed in an electrolyte, and an alternate current sent through it. On trying the experiment, however, we found that, though in some cases, notably with aluminium, we did obtain considerable distortion of the wave forms, the curves were never similar to those obtained with an arc between the electrodes, in that the current was not completely interrupted in either direction.

We believe that no wave forms of metal-carbon arcs such as those given in the diagram have previously been published, as owing to their transitory nature they can only be recorded by an oscillograph method.

Section 10.—The Enclosed Arc.

(Diagram and Table XVIII.)

In view of the great progress that the use of enclosed arcs has made during the last few years, we considered it advisable to test their reaction on the wave form, and this we were enabled to do by the courtesy of the Davy Elec-

[1] *American Journal of Science*, 1891.
[2] *American Academy of Sciences*, 1896, p. 227.

trical Construction Company,[1] who placed one of their 8·5 amps. enclosed arcs at our disposal, and on which we made the following tests.

The experiments were carried out in the same way as before, except that the choking coil, which was supplied with the lamp, was always used in series with it.

In all the tests the regulating coils of the lamp were in circuit; but the P.D. arc curve was taken between the carbon holders and does not include the regulating coils.

The experiments were tried on both the alternators, first with the Siemens cored carbons as supplied with the lamp (expts. 187 and 189), and afterwards an attempt was made to use both solid "Apostle" carbons. This we were only able to do when using the inductor alternator (expt. 191), as the available E.M.F. with the Ferranti was insufficient for stability.

After obtaining the wave forms with the carbons in the enclosed lamp, the experiment was in each case repeated with the same carbons in the hand lamp (expts. 188, 190, 192), every condition of the circuit remaining as before, *i.e.*, the regulating coils of the lamp and the choking coil were kept in series, and the arc length and current were brought to approximately their former values. The P.D. arc curve was taken between the terminals of the hand lamp.

A comparison of the two sets of curves shows that *the effect of enclosing the arc is to produce a high peak on the P.D. arc wave, with but small effect on the current wave;* this is exceedingly marked in the case of solid "Apostle" carbons (expts. 191, 192).

We have previously found that the front peak is due to the resistance of the gaseous column, and it therefore appears as if enclosing the arc increases the resistance of this column.

The change produced by enclosing the arc on the r.m.s. value of the P.D. between its terminals for the same length and current is very marked, this increase being no less than 65 per cent. with the cored carbons supplied with the lamp.

In all cases, enclosing the arc has reduced the power factor, and increased the stability of the arc.

[1] *The Electrician*, 1897, vol. xl. pp. 2 and 71.

ON FERRANTI ALTERNATOR.

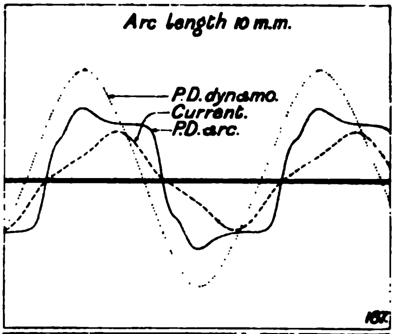
Frequency, 100 ∿ per sec.

Carbons, 13 mm. "Siemens," both cored.

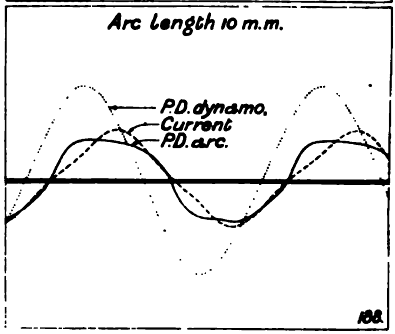
DAVY ENCLOSED ARC.

Scales.

For P.D. Dynamo - 1 mm. = 10 volts.
P.D. Arc - - 1 mm. = 10 volts.
Current - - - 1 mm. = 2 amps.



Same Carbons in Hand Lamp.



NOTE.—*These diagrams are*

SECTION II.—MISCELLANEOUS EXPERIMENTS.

(Diagram and Table XIX.)

Resistance in Parallel with the Arc.

It often occurs in practice that an arc is burnt on the same circuit with incandescent lamps. To imitate this case we tried an arc in parallel with a non-inductive resistance between the dynamo terminals, and we found that the reaction of the arc on its wave forms occurred as usual.

Arcs in Series.

Experiments were made with four hand-regulated arcs in series, one of the arcs being used as a test arc. The

XVIII.

CLOSED ARC.

ON PYKE & HARRIS ALTERNATOR.

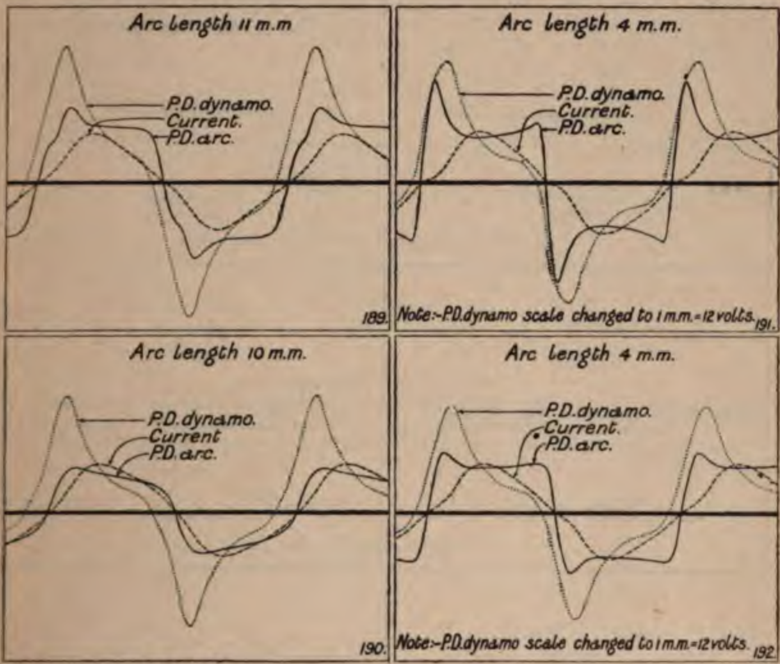
Frequency, 97 ∿ per sec.

Carbons, 13 mm. "Siemens," both cored.

ON PYKE & HARRIS ALTERNATOR.

Frequency, 97 ∿ per sec.

Carbons, 13 mm. "Apostle," both solid



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curves in the first row in the diagram are P.D. between the alternator terminals and current; and those in the second row P.D. between the terminals of the test arc.

The reaction on the P.D. test arc curve is larger than that produced on it when there are no other arcs in series. The effect on the P.D. dynamo wave is similar to that on the test arc, and the current tends to keep a small value for a considerable part of the period. The solid-cored and cored arcs also react on the wave forms to a greater extent than when one arc is used alone. Owing to the difficulty of regulating all four arcs in series by hand the r.m.s. values given in the table are only approximate.

DIAGRAM

MISCELLANEOUS

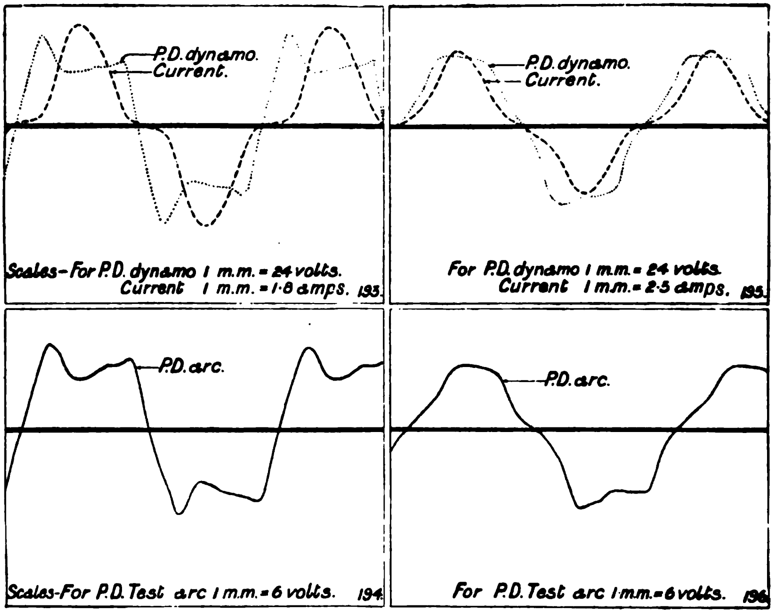
FOUR ARCS

ON PYKE AND

Carbons, 13 mm. "Apostle." Current,

Resistance in

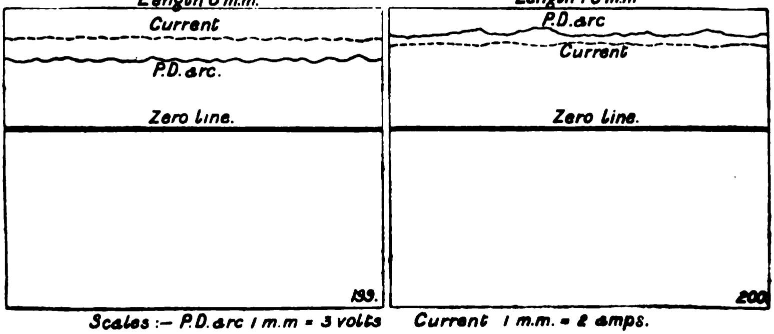
SOLID ARCS. SOLID-CORED ARCS.



DIRECT CURRENT HISSING ARC.

Carbons +11 / −9 } m.m. "Apostle" solid. *Resistance in series 2·2 ohms.*

Length 0 m.m. *Length 1·6 m.m*



Scales :– P.D. arc 1 m.m = 3 volts *Current 1 m.m. = 2 amps.*

NOTE.—*These diagrams are*

XIX.

RESULTS.

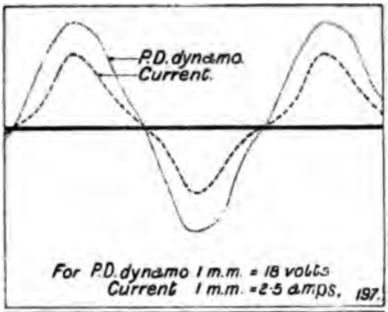
IN SERIES.

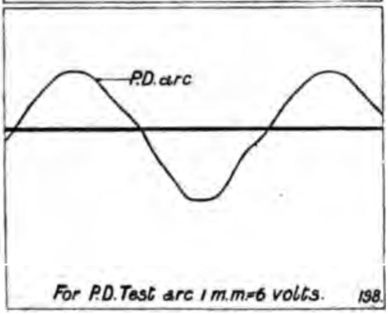
HARRIS ALTERNATOR.

14·8 amps. Frequency, 95 ∿ per sec.

series, 0·3 ohm.

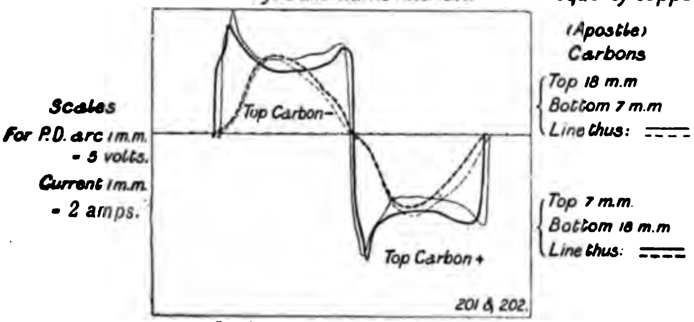
CORED ARCS.





SOLID ARC

Unequal Carbons.
On Pyke and Harris Alternator



Resistance in series 0·25 ohms

here reproduced half-size.

Direct Current Hissing, Solid Arc.

In many of the P.D. arc wave forms which we have obtained there is a waviness in the horizontal part of the curve, and this in nearly all cases corresponded with the arc hissing, and we thought that there was some connection between the hissing and the waviness.[1] We therefore tried some experiments to see if the oscillograph could delineate the variations of P.D. and current which are known to exist when the direct current arc hisses.

Two records were obtained for direct current solid arcs (expt. 199), for a hissing arc of no visible length, and (expt. 200) for a longer arc which was hissing steadily. These records show clearly the variations in P.D. and current which occur when the arc hisses, and the similarity to the waviness, in some of our alternate current wave forms, confirms us in the supposition that this waviness is due to the arc hissing. In some of our alternate current wave forms we have found the waviness in the P.D. curve on one side of the zero line only (see expts. 131 and 135), and this indicates that the arc hissed only when the current was flowing in that direction.

The frequency of the oscillations in the P.D. and current of the hissing arc between solid carbons (expt. 199) averages about 1,000 per second; and it is of interest that *these oscillations occur, so that an increase of current is accompanied by a decrease in P.D.* The larger irregular variations (in expt. 200) seem to be due to the arc trying to pass from the hissing to the silent state and returning to the hissing state again.

Arcs between Unequal Carbons.

We have recorded the wave forms for an alternate current arc burning between two solid carbons differing greatly in diameter. Two pairs of curves were obtained, the first (expt. 201) was for P.D. arc and current when the smaller carbon was at the top, and the second pair (expt. 202) was for the same carbons with the larger one at the top, the other conditions remaining the same. This reversal of the position of the carbons was made to eliminate a possibility that the observed effects were due to the position of the carbons and not to their difference in sizes.

[1] This waviness has almost entirely disappeared in the reproduction of the wave forms.

The curves show that the two halves of the wave forms on opposite sides of the zero line are not identical, the current being larger and the P.D. smaller when the large carbon is positive, and that this effect is independent of which carbon is uppermost in the arc. These results agree with those found by Messrs. Jamin and Maneuvrier and by M. Blondel.

This last result was obtained before we commenced using an oscillograph, and is by the Joubert contact method.

STABILITY.

The stability of the arc, considered as an electrical phenomenon, is greatly increased by a large impressed E.M.F. in its circuit, and with the alternate current arc the necessity of this E.M.F. is greatest when the instantaneous value of the current is small, so as to reduce the time during which the arc is extinguished in each period. This time of extinction is very important, for the P.D. between the carbons required to re-light the arc increases rapidly with the duration of the extinction. A high E.M.F. may be impressed on the circuit if we use sufficient resistance or self-induction in series with the arc, the latter giving the greater stability, as it ensures a high E.M.F. when the current vanishes. Self-induction has also the advantage, over resistance, that it wastes no power in itself ; but it may impair the efficiency of the generator by causing the current to lag.

The position of the self-induction, or of the resistance, in the circuit seems of very little importance, so that the use of an inductive alternator or of a transformer gives stability ; if, added to this, the machine or transformer possesses a very drooping characteristic, then sufficient stability may be obtained without the addition of outside self-induction or resistance.

The reduction of frequency lengthens the time of extinction and reduces the stability, so that, for a given circuit and impressed E.M.F., there is a limiting frequency, below which a given arc cannot be maintained ; this limiting frequency being higher with solid than with cored carbons. It must not, however, be forgotten that high frequency arcs are less efficient than low ones.

The effect of the nature of the carbons on the stability is very marked, cored and solid-cored arcs being very stable, while solid arcs are much less so on the same circuit and for the same arc length and current.

The vapours of foreign substances introduced into the arc generally seem to increase its conductivity during the extinctions, unless they are present in the metallic state. This increases the ease with which the arc relights, and improves the stability. The addition of a compound of sodium or of potassium is very effective, and it is probable that the stability of the cored arc is largely due to the presence of these bodies. These substances, however, lower the P.D. at which the arc burns, and a low voltage arc is generally less efficient than a high voltage one; so that the gain in stability by this means is probably accompanied by a loss of efficiency.

Any attempt to increase the stability of the alternate current arc, by adding resistance or self-induction in series, or by changing the frequency or the nature of the carbons, usually either wastes more power in the circuit, or decreases the efficiency of the alternator or of the arc. Thus, increase of stability by these methods may result in decrease of efficiency of the whole arrangement as a means of producing light.

The power factor of an arc of medium length gives an idea of its stability, as a low power factor usually arises from the time of small current being considerable. With increase of arc length, however, the power factor improves, while the stability becomes less; and enclosing the arc improves the stability, but reduces the power factor.

The best means of securing stability in practice seem to be: First, *if the circuit supplies arcs only, to use an inductive generator or transformer with a drooping characteristic.* Second, *if the arcs are supplied from a constant pressure, circuit to use a transformer as above or self-induction in series with the arc.* In either case the use of carbons, as hard and as free from foreign substances as is consistent with stability, is advisable so as to increase the luminous efficiency.

Conclusions.

Pages 25 to 30.

(1) If a resistance in any alternate current circuit is replaced by an arc which transmits the same r.m.s. current: the wave forms of P.D. between its terminals, and current through it, are no longer the same; and the r.m.s. value of the P.D. between its terminals is increased.

(2) With high-voltage solid carbons, and *small* self-induction in the circuit: the P.D. arc wave is flat topped, with high front and smaller back peak; the current remains small for a considerable part of the period; the power factor is low; and the arc is unstable.

(3) With high-voltage solid carbons, and *considerable* self-induction in the circuit: the P.D. arc wave is rectangular, with much smaller peaks; the current no longer remains small for any sensible time; the power factor is higher; and the arc is much more stable.

(4) With soft-cored carbons, the reaction is generally small; the P.D. arc wave has a dent in the front side; the power factor is nearly unity; and the arc is very stable.

(5) With one solid and one cored carbon, the reaction is intermediate. At any instant the nature of the carbon which is positive determines to a large degree the reaction at that time.

Pages 31 to 38.

(6) Increase of arc length with solid carbons first increases both peaks on the P.D. arc wave until the hissing point is reached; further increase of length produces a high front peak and no back peak. Very long arcs with all kinds of carbons give a P.D. arc wave with a large front peak. The power factor, with solid carbons, decreases with increase of length up to the hissing point, and then increases; with cored carbons the power factor is generally higher for longer arcs.

Pages 38 to 43.

(7) The large front peak on the P.D. wave for long arcs is due to the high resistance of the gaseous column after each extinction of the arc.

(8) The distribution of the fall of P.D. in the alternate current arc is similar at any instant to that in the direct current arc.

Pages 43 *to* 53.

(9) Resistance in series has little effect on the P.D. arc curve, but causes the current wave to become more like the open-circuit wave of the particular machine; the larger the resistance, the greater this effect.

Pages 54 *to* 58.

(10) Lowering the frequency increases the reaction, increases the peaks with the solid arc, and the time during which the current is small, in all cases. It reduces the power factor and the stability, but improves the efficiency.

Pages 59 *to* 63.

(11) With transformers having a very small magnetic leakage, that is designed to give constant secondary P.D., the distortion of the secondary wave forms is accompanied by a similar distortion of the primary wave forms. With transformers having a large magnetic leakage and intended to give constant current, the distortion is not appreciably transmitted from the secondary to the primary circuits.

Pages 63 *to* 66.

(12) The distortion of the wave forms by the arc occurs with all makes of carbons, the amount depending on the hardness and the impurities contained.

Pages 66 *to* 74.

(13) Foreign bodies in the arc generally reduce the reaction, and lower the r.m.s. value of the P.D. for a given length and current. Small quantities of the salts of the alkali metals may be used to lower the voltage at which the arc burns; but they probably also reduce the efficiency.

Pages 74 *to* 80.

(14) Arcs between carbons and metals are generally unilateral; the current flowing much more easily from the metal to the carbon. The more rapid cooling of one electrode facilitates the flow of the current from it, and resists the flow towards it.

Pages 80 *to* 82.

(15) Enclosing the arc increases the reaction, the front peak on the P.D. arc wave becoming more marked. The r.m.s. value of the P.D. is raised, and the power factor reduced.

Pages 82 *and* 83.

(16) The reaction still occurs when the arc is only part of the load on the alternator.

(17) The reactions are more marked with arcs in series.

Pages 84 *and* 85.

(18) With arcs between unequal carbons the current is larger when large carbon is +.

Page 86.

(19) When the direct current arc hisses, the P.D. and current vary rapidly ; these variations, in the case of a solid arc, had a frequency of 1,000 per second, and take place so that an increase of current is accompanied by a reduction of P.D.

(20) In all questions relating to the alternate current arc, the distortion of the wave forms by the arc must be taken into account, as these distortions considerably affect the efficiency, the stability, and the power factor.

In conclusion, we wish to express our sincere thanks to Professor Ayrton and Mr. Mather for the valuable assistance and advice which they have given us, and for the great interest that they have shown in all our experiments, as well as for the use of the apparatus of the physical laboratories of the Central Technical College, where our investigations were carried out. We also wish to thank Mr. King and Mr. Read, who have assisted us in a large number of the experiments.

TABLE I.

PRELIMINARY EXPERIMENTS.

Carbons, 13 mm. "Apostle." Current, 14·8 amps. Arc length, 3 mm.

ON FERRANTI ALTERNATOR.

Frequency, 100 ∿ per sec.

Expt. No.	Carbons. Top.	Carbons. Bottom.	Resistance in series. Ohms.	S. I. in series. Henrys.	Impedance of Inductance Coil. Ohms.	P. D. arc. Volts.	Power arc. Watts.	Power factor. Arc.	Dynamo excitation. Amps.	Notes.
1	3·04 ohms in place of arc		3·3	Very small	...	45·1	666	1·00	20	To be compared with No. 2
2	Solid	Solid	3·3	„	...	53·2	581	0·74	20	Slightly hissing
3	„	Cored	3·9	„	...	37·4	521	0·94	20	Not hissing
4	Cored	„	3·9	„	...	32·0	470	0·99	18·5	„
5	2·94 ohms in place of arc		0·3	$7·6\ 10^{-3}$	4·77	43·4	642	1·00	20	To be compared with No. 6
6	Solid	Solid	0·3	7·6 „	4·94	50·2	635	0·855	20	Slightly hissing
7	„	Cored	0·3	7·6 „	4·79	37·8	536	0·96	16·3	Not hissing
8	Cored	„	0·3	7·6 „	4·80	31·6	461	0·985	16·0	„

ON PYKE & HARRIS ALTERNATOR.

Frequency, 97 ∿ per sec.

Expt. No.	Carbons. Top.	Carbons. Bottom.	Resistance in series. Ohms.	S. I. in series. Henrys.	Impedance of Inductance Coil. Ohms.	P. D. arc. Volts.	Power arc. Watts.	Power factor. Arc.	Dynamo excitation. Amps.	Notes.
9	2·7 ohms in place of arc		0·3	Very small	...	39·9	590	1·00	0·74	To be compared with No. 10
10	Solid	Solid	0·3	„	...	50·0	654	0·885	0·74	Slightly hissing
11	„	Cored	0·3	„	...	37·8	536	0·96	0·69	Not hissing.
12	Cored	„	0·3	„	...	33·3	483	0·98	0·68	„

TABLE II.

EFFECT OF LENGTH.

ON FERRANTI ALTERNATOR.

Frequency, 100 ∿ per sec. Carbons, 13 mm. "Apostle." Current, 14·8 amps. Resistance in series, 3·9 ohms.

Expt. No.	Carbons. Top.	Bottom.	Arc length. mm.	P. D. arc. Volts.	Power. Watts.	Power factor.	Notes.
13	Solid	Solid	0	26·2	309	0·795	Giving roaring sound and small green flames
14	"	"	1	43·5	404	0·63	Hissing steadily
15	"	"	3*	53·2	581	0·74	Near hissing point and slightly hissing
16	Solid	Cored	0	24·9	334	0·91	Hissing
17	"	"	1	31·0	426	0·93	Not hissing, but near hissing point
18	"	"	3	37·4	521	0·94	Not hissing ; steady
19	Cored	Cored	0	20·9	303	0·985	Not hissing ; steady
20	"	"	1	25·6	375	0·99	" "
21	"	"	3	32·0	470	0·99	" "
22	"	"	7	36·2	515	0.96	" "

* Note the resistance in series had to be reduced to 3·3 ohms to get this length.

TABLE III.

EFFECT OF LENGTH.

ON PYKE & HARRIS ALTERNATOR.

Frequency, 97 ∿ per sec. Carbons, 13 mm. "Apostle." Current, 14·8 amps. Resistance in series, 0·3 ohms.

Expt. No.	Carbon. Top.	Carbon. Bottom.	Arc length. mm.	P. D. arc. Volts.	Power. Watts.	Power factor.	Notes.
23	Solid	Solid	0	26·7	321	0·810	Small green flames and roaring sound
24	"	"	1	43·0	502	0·790	Hissing
25	"	"	3	53·8	688	0·865	Very slightly hissing
26	"	"	6	63·6	807	0·860	Not hissing
27	"	"	15	83·0	1,100	0·89	Humming sound, same pitch as dynamo
28	"	"	20	91·5	1,220	0·90	Same; very unstable
29	Solid	Cored	0	28·4	388	0·925	Hissing
30	"	"	1	33·6	476	0·955	Not hissing
31	"	"	3	38·6	550	0·965	"
32	"	"	6	45·8	653	0·965	"
33	"	"	15	66·0	949	0·97	"
34	"	"	30	91·6	1,315	0·97	Humming sound of dynamo
35	Cored	Cored	0	23·4	338	0·975	Not hissing
36	"	"	1	27·4	394	0·975	"
37	"	"	3	33·3	483	0·980	"
38	"	"	6	34·8	507	0·980	"
39	"	"	15	52·3	745	0·965	"
40	"	"	30	81·5	1,135	0·95	Giving loud humming sound of dynamo
41	"	"	45	107	1,605	1·01	Same flaming; very unstable; readings only rough approximations

TABLE IV.

SEARCH CARBONS IN ARC.

ON PYKE & HARRIS ALTERNATOR.

Frequency, 97 ∿ per sec. Carbons, 13 mm. "Apostle." Current, 14·8 amps. Resistance in series, 0·3 ohms.

Expt. No.	Carbons. Top.	Carbons. Bottom.	Arc length. mm.	P. D. arc. Volts.	Power. Watts.	Power factor.	Notes.
42, 43	Solid	Solid	6	74·5	901	0·825	A sharp ridge formed across craters where protected by search carbons
44, 45	"	"	15	93	1,155	0·83	
46, 47	Solid	Cored	6	54·7	786	0·97	Same note
48, 49	"	"	15	70·5	1,025	0·98	
50, 51	Cored	Cored	6	46·7	688	0·995	Same note
52, 53	"	"	15	60·5	874	0·975	

TABLE V.

EFFECT OF RESISTANCE IN SERIES.

ON FERRANTI ALTERNATOR.

Frequency, 100 ∿ per sec. Carbons, 13 mm. "Apostle." Current, 14·8 amps.

Exp. No.	Carbons. Top.	Carbons. Bottom.	Arc length. mm.	Resistance in series. Ohms.	P. D. alternator. Volts.	P. D. arc. Volts.	Power. Watts.	Power factor.	Notes.
54	Solid	Solid	1	0·96	51·5	42·1	360	0·58	Hissing and unstable
55	"	"	1	3·9	92	43·5	404	0·63	Hissing
56	Solid	Cored	3	0·96	51	38·2	524	0·93	Not hissing; steady
57	"	"	3	3·9	94·5	37·4	521	0·94	Not hissing; steady
58	Cored	Cored	3	0·3	37	31·4	457	0·98	Not hissing, but unstable
59	"	"	3	0·96	46	31·6	457	0·975	Not hissing; steady
60	"	"	3	3·9	90	32·0	470	0·990	Not hissing; steady

TABLE VI.

EFFECT OF RESISTANCE IN SERIES.

ON PYKE & HARRIS ALTERNATOR.

Frequency, 97 ∿ per sec. Carbons, 13 mm. "Apostle." Current, 14·8 amps.

SOLID ARC.

Expt. No.	Arc length. mm.	Resistance in series. Ohms.	P. D. alternator. Volts.	P. D. arc. Volts.	Power. Watts.	Power factor.	Notes.
61	0	0·3	31	26·7	321	0·810	Green flames & roaring sound
62	0	5·1	97	25·0	300	0·810	" "
63	0	10·2	172·5	26·9	294	0·740	" "
64	1	0·3	47·5	43·0	502	0·790	Hissing
65	1	5·1	112	44·7	452	0·680	"
66	1	10·2	181	45·1	417	0·625	"
67	3	0·3	58	53·8	688	0·865	Very slightly hissing
68	3	5·1	120	51·6	583	0·765	Hissing
69	3	10·2	188	49·3	536	0·735	"
70	6	0·3	68·5	63·6	807	0·860	Not hissing
71	6	5·1	136·5	62·2	766	0·835	"
72	6	10·2	202·5	60·2	720	0·810	"

TABLE VII.

EFFECT OF RESISTANCE IN SERIES.

ON PYKE & HARRIS ALTERNATOR.

Frequency, 97 ∿ per sec. Carbons, 13 mm. "Apostle." Current, 14·8 amps.

SOLID CORED ARC.

Expt. No.	Arc length. mm.	Resistance in series. Ohms.	P. D. alternator. Volts.	P. D. arc. Volts.	Power. Watts.	Power factor.	Notes.
73	0	0·3	32·5	28·4	388	0·925	Hissing
74	0	5·1	101	26·7	360	0·910	"
75	0	10·2	168·5	25·6	306	0·810	"
76	1	0·3	37·5	33·6	476	0·955	Not hissing, but near a hissing point
77	1	5·1	106	31·9	423	0·900	Hissing
78	1	10·2	173	29·5	392	0·900	"
79	3	0·3	43	38·6	550	0·965	Not hissing
80	3	5·1	112	37·3	515	0·935	"
81	3	10·2	181·5	37·0	512	0·935	"
82	6	0·3	50	45·8	653	0·965	"
83	6	5·1	119	44·1	628	0·960	"
84	6	10·2	188	43·4	600	0·935	"

TABLE VIII.

EFFECT OF RESISTANCE IN SERIES.

ON PYKE & HARRIS ALTERNATOR.

Frequency, 97 ∿ per sec. Carbons, 13 mm. "Apostle." Current, 14·8 amps.

CORED ARC.

Expt. No.	Arc length. mm.	Resistance in series. Ohms.	P. D. alternator. Volts.	P. D. arc. Volts.	Power. Watts.	Power factor.	Notes.
85	0	0·3	27·5	23·4	338	0·975	Not hissing
86	0	5·1	97·5	22·3	319	0·970	"
87	0	10·2	167	20·7	303	0·990	"
88	1	0·3	31·5	27·4	394	0·975	"
89	1	5·1	101	26·4	376	0·965	"
90	1	10·2	168	24·5	354	0·975	"
91	3	0·3	37·5	33·3	483	0·980	"
92	3	5·1	106·5	30·6	445	0·985	"
93	3	10·2	172·5	29·0	420	0·980	"
94	6	0·3	39·5	34·8	507	0·980	"
95	6	5·1	109·5	33·9	489	3·975	"
96	6	10·2	177·5	33·1	480	0·980	"

TABLE IX.

EFFECT OF FREQUENCY.

ON FERRANTI ALTERNATOR.

Carbons, 13 mm. "Apostle." Arc length, 3 mm. Current, 14·8 amps.
Resistance in series, 3·9 ohms.

Expt. No.	Carbons.		Frequency ∿ per sec.	P. D. arc. Volts.	Power. Watts.	Power factor.	Notes.
	Top.	Bottom.					
97	Solid	Solid	100	53·2*	581	0·74	Near hissing point, and slightly hissing
98	,,	,,	200	50·8	597	0·795	Slight inclination to hiss
99	Solid	Cored	100	37·4	521	0·94	Not hissing; steady arc
100	,,	,,	200	37·4	546	0·985	,, ,,
101	Cored	Cored	100	32·0	470	0·990	Not hissing; steady arc
102	,,	,,	200	31·1	461	1·00	,, ,,

* Resistance in series reduced to 3·3 ohms to get the length.

TABLE X.

EFFECT OF FREQUENCY.

ON PYKE AND HARRIS ALTERNATOR.

Carbon, 13 mm. "Apostle." Arc length, 3 mm. Current, 14·8 amps. Resistance in series, 0·3 ohms.

Expt. No.	Carbons. Top.	Carbons. Bottom.	Frequency. ∿ per sec.	P. D. arc. Volts.	Power. Watts.	Power factor.	Notes.
103	Solid	Solid	127	53·6	688	0·870	Not hissing; steady
104	"	"	97	53·9	688	0·865	Inclined to hiss, but not hissing
105	"	"	70	54·3	650	0·805	Very inclined to hiss and be unstable
106	"	"	57	54·7	606	0·75	Very unstable, and giving a loud roaring sound
107	Solid	Cored	127	38·4	553	0·975	Not hissing; steady
108	"	"	97	38·6	550	0·965	" "
109	"	"	70	38·6	535	0·935	" "
110	"	"	46	38·6	522	0·915	Not hissing; inclined to be unstable
111	Cored	Cored	127	33·0	480	0·985	Not hissing; steady
112	"	"	97	33·3	483	0·980	" "
113	"	"	70	33·0	472	0·97	" "
114	"	"	46	32·4	461	0·96	" "
115	"	"	29·2	31·6	447	0·955	Not hissing; stable; unpleasant to look at

TABLE XI.

ARC BURNING ON A "WESTINGHOUSE" TRANSFORMER.

Ratio of transformation, 1·1. Carbons, 13 mm. "Apostle." Arc length, 3mm. Current, 14·8 amps. Resistance in series with arc, 0·2 ohm.

ON FERRANTI ALTERNATOR.

Frequency, 100 ∿ per sec. Resistance in series with primary of transformer, 2·4 ohms.

Expt. No.	Carbons. Top.	Bottom.	Alternator. P. D. Volts.	Current. Amps.	P. D. arc. Volts.	Power. Watts.	Power factor.	Notes.
116, 117	Solid	Solid	92·5	17·6	50·9	557	0·74	Slightly hissing
118, 119	„	Cored	84	16·4	37·4	533	0·965	Not hissing
120, 121	Cored	„	77	16·1	31·3	452	0·975	„
ON PYKE & HARRIS ALTERNATOR. Frequency, 97 ∿ per sec. Resistance in series with primary of transformer, 0·2 ohm.								
122, 123	Solid	Solid	61·3	17	52·9	678	0·865	Non-hissing
124	„	Cored	44·3	16·5	37·8	531	0·950	„
125	Cored	„	39·6	16·3	31·0	452	0·985	„

TABLE XII.

ARC BURNING ON A "MORDEY VICTORIA" TRANSFORMER.

Supplied with current from the Ferranti Alternator, the current being first transformed up.

Frequency, 93 ∿ per sec.

Brush arc lamp used with the regulating coils in circuit.

Experiment numbers	126 and 127
Carbons. "Conradty" both cored diameter	15 mm.
Arc length	2 mm.
Current through arc	14·7 amps.
P. D. between arc lamp terminals...	37·6 volts.
Resistance in series with arc lamp	0·2 ohm.
Power supplied to lamp	531 watts.
Power factor of lamp, including regulating coils ...	0·96
P. D. high tension, or primary circuit	2,080 volts.
Current	0·44 amp.

TABLE XIII.

ARCS BETWEEN DIFFERENT MAKES OF CARBONS.

ON FERRANTI ALTERNATOR.

Frequency, 100 ∿ per sec. Constant dynamo excitation. Carbons, 13 mm. diameter. Resistance in series with arc, 3·64 ohms.

Expt. No.	Carbons. Top.	Carbons. Bottom.	Arc length. mm.	P. D. arc Volts.	Current. Amps.	Power. Watts.	Power factor.
			Conradty "Norris" Carbons.				
128	Solid	Solid	2·1	52·8	13·3	535	0·76
129	Cored	,,	7·2	43·1	13·5	567	0·975
130	,,	Cored	8·0	36·2	15·0	532	0·98
			Brush Carbons.				
131	Solid	Solid	2·1	49·5	14·6	598	0·83
132	Cored	,,	5·9	44·9	13·6	566	0·925
133	,,	Cored	6·3	42·8	16·0	665	0·97
			Carré Carbons.				
134	Cored	Cored	3·4	39.4	14·8	541	0·93

TABLE XIV.

ARCS BETWEEN DIFFERENT MAKES OF CARBONS.

ON FERRANTI ALTERNATOR.

Frequency, 100 ∿ per sec. Constant dynamo excitation. Carbons, 13 mm. diameter. Resistance in series with arc, 0·25 ohm. Self-induction of impedance coil in series with arc, $7{\cdot}6 \times 10^{-3}$ henry. Impedance of coil (with sinusoidal current) at 100 ∿ per sec., 4·77 ohms.

Expt. No.	Carbons. Top.	Carbons. Bottom.	Arc length. mm.	P. D. arc Volts.	Current. Amps.	Power. Watts.	Power factor.	P. D. between terminals of Impedance Coil. Volts.	Impedance of Coil. Ohms.
			Conradty "Norris" Carbons.						
133	Solid	Solid	2·1	52·8	13·1	551	0·795	67·3	5·14
136	Cored	,,	8·5	46·7	15·1	665	0·945	74·5	4·93
137	,,	Cored		35·7	15·8	564	0·995	77·9	4·93
			Brush Carbons.						
138	Solid	Solid	2·1	49·2	13·5	595	0·895	66·9	4·96
139	Cored	,,	5·1	45·2	15·0	639	0·945	71·8	4·79
140	,,	Cored	6·3	43·2	15·3	657	0·995	72·8	4·76
			Carré Carbons.						
141	Cored	Cored	2·1	39·2	15·0	564	0·96	74·4	4·96

TABLE XV.

ARCS BETWEEN ONE SOLID CARBON AND ONE CARBON CORED WITH A FOREIGN SUBSTANCE.

ON FERRANTI ALTERNATOR.

Frequency, 100 ∿ per sec. Constant dynamo excitation. Carbons, 13 mm. "Apostle." Diameter of core of foreign substance, 4 mm. Arc length and current varied. Resistance in series, 3·9 ohms.

Expt. No.	Nature of Core.	Arc length. mm.	P. D. arc. Volts.	Current. Amps.	Notes.
142	No core (hollow)	2·1	38·6	14·8	Not hissing
143	Alumina	5·7	39·4	14·8	The alumina fused and boiled. Bright spots on crater of solid carbon
144	Baryta	10.7	40·2	14·8	The baryta fused. Arc peach coloured with greenish - yellow envelope. Flamed. Bright spots on crater of solid carbon
145	Chromium oxide	5·0	40·2	14·8	The chromium reduced to metal. Arc blue-green coloured. Molten chromium (?) was ejected, and burnt with brilliant scintillations in falling
146	Manganese di-oxide	5·7	39·4	14·8	The manganese was reduced to metal
147	Strontia	8·6	39·4	14·8	The strontia fused slightly. Did not evaporate or burn away as quickly as the surrounding carbon, and formed a bridge across the arc. Bright spots on crater of solid carbon
148	Tin oxide	3·8	41·0	14·8	Reduced to metal. Looked like a carbon arc
149	Lime	6·6	40·1	14·3	Lime did not burn away or fuse, and formed a bridge across arc. Golden flame. Bright spots.
150	Copper wire	4·6	35·2	17·1	Copper melted and boiled. Green arc
151	Zinc	8·0	44	14·8	Easy-burning purple arc, giving dense white fumes of zinc oxide (?)
152	Pipe clay	8·0	38·8	14·8	Fused, but did not boil. Pale lilac arc (K ?). Bright spots on crater of solid carbon
153	Plaster of Paris	6·6	38·6	14·8	Did not fuse. Golden flame
154	Fire clay	8·3	39·0	14·8	Fused. Steady arc with lilac flame
155	Kaolin	7·1	40·2	14·8	Fused and boiled. Unsteady purple arc surrounded with green flame. Bright spots on solid crater
156	Common salt	5·0	38·6	14·8	Evaporated rapidly. Flaming arc with sodium colouration
157	Glass rod (soda)	12·0	35·6	15·4	Fused. Brilliant sodium flame. Could be burnt very long, and required very low volts
The wave forms of these experiments are not reproduced.	Powdered combustion tube	10·1	38·2	14·8	Fused. Violet arc with outer envelope coloured sodium, and behaved like No. 157
	Iron oxide, Rouge	7	39·4	14·5	Reduced to metal. Arc lilac and seemed to give very little light
	Magnesium oxide	4	39·9	14·8	Arc slightly green and inclined to hiss
	Lead oxide, Red lead	10	42·4	14·8	Reduced to metal, which quickly volatilised. Arc lilac coloured
	White sand	3·6	42·5	14·8	Fused, but seemed to have no effect on the arc

TABLE XVI.

ARCS BETWEEN ONE SOLID CARBON AND ONE CARBON CORED WITH A FOREIGN SUBSTANCE.

ON FERRANTI ALTERNATOR.

Frequency, 100 ∿ per sec. Constant dynamo excitation. Constant arc conditions. Carbons, 13 mm. "Apostle." Diameter of core of foreign substance, 4 mm. Arc length, 3 mm. Current, 14·8 amps. Resistance in series varied.

Expt No.	Nature of Core.	P. D. arc. Volts.	Res. in series. Ohms.	Power. Watts.	Power factor.	Notes.
158	No core (hollow)	42·4	3·53	587	0·93	Low voltage carbon arc
159	Ordinary core	35·7	3·90	521	0·985	
160	Chromium Oxide	23·7	4·94	319	0·91	Reduced to metal. See No. 145
161	Lime	22·3	4·94	306	0·93	See note to 149
162	Kaolin	32·5	4·43	432	0·90	" 155
163	Pipe Clay	25·2	4·70	365	0·98	" 152
164	Glass rod (soda)	17·6	5·17	253	0·97	" 157

EFFECT OF SOAKING CARBONS IN AQUEOUS SOLUTIONS.

Carbons both solid. Other conditions as above.

Expt. No.	Solution in which Carbons were Soaked.	P. D. arc. Volts.	Res. in series. Ohms.	Power. Watts.	Power factor.	Notes.
165	Unsoaked	53·2	3·29	581	0·74	When soaked in tap water and dried gave same results
166	4% Sodium Carbonate	42·4	3·50	565	0·90	When first started had a strong sodium flame; burnt 45 minutes before taking readings
167	4% Potassium Carbonate	42·4	3·50	558	0·89	Looked like an ordinary carbon arc; burnt 45 minutes before taking readings

TABLE XVII.

ARCS BETWEEN CARBON AND METALS.

ON FERRANTI ALTERNATOR.

Frequency about 100 ∿ per sec. Potential difference between dynamo terminals when on open circuit about 100 volts. Bottom electrode "Solid Carbon" 6 mm. diameter. Top electrode "Metal Rod" 6 mm. diameter. Current from 13 to 20 amps. Resistance in series, 3·9 ohms.

Expt. No.	Nature of Metal Electrode.	Notes.
	Aluminium	Easy-burning greenish blue arc. Metal melted, but was held together by skin of oxide
168	- - - - - - -	Length 1·5 mm. Amps. 17. Volts. 28.
169	- - - - - - -	„ 3 „ „ ? „ 34.
170, 171	Iron	Intense greenish arc, very difficult to burn, resistance in series reduced to 3·22 ohms
172, 173	Copper	Short bright arc like a carbon arc, current about 16 amps. Short green arc, current 13 amps.
174, 175	Nickel	Bluish arc difficult to burn ; no visible length
176, 177, 178, 179	Zinc	Purple arc very difficult to maintain owing to zinc melting so quickly. White substance (ZnO. ?) was deposited on carbon, and seemed to insulate its end and prevent it from burning away
180, 181	Lead	Purple arc ; lead melted very quickly
182	Tin	Metal melted so quickly that no observation could be made
183, 184	Phosphor Bronze	Green arc burning fairly easily
185, 186	Brass	Greenish arc giving white fumes

TABLE XVIII.

DAVY ENCLOSED ARC.

Carbons, 13 mm. diameter. Choking coil supplied with lamp, and the regulating coils of lamp in series with arc, in *all* the experiments.

ON FERRANTI ALTERNATOR.

Frequency, 100 ∿ per sec.

Expt. No.	P. D. alternator terminals. Volts.	Carbons. Both.	Arc length. mm.	Current. Amps.	P. D. between terminals of arc. Volts.	Power arc. Watts.	Power factor. Arc.
187	100	Cored "Siemens" in Davy enclosed	10	8·4	70·5	542	0·915
188	82·5	Cored "Siemens" in Hand Lamp	10	8·5	42·2	338	0·945

ON PYKE & HARRIS ALTERNATOR.

Frequency, 97 ∿ per sec.

Expt. No.	P. D. alternator terminals. Volts.	Carbons. Both.	Arc length. mm.	Current. Amps.	P. D. between terminals of arc. Volts.	Power arc. Watts.	Power factor. Arc.
189	99	Cored "Siemens" in Davy enclosed	11	8·3	71·6	555	0·935
190	79	Cored "Siemens" in Hand Lamp	10	8·5	43·2	347	0·945
191	106	Solid "Apostle" in Davy enclosed	4	8·5	75·6	460	0·715
192	91·5	Solid "Apostle" in Hand Lamp	4	8·5	61·4	439	0·84

TABLE XIX.

MISCELLANEOUS RESULTS.

FOUR ARCS IN SERIES.

ON PYKE & HARRIS ALTERNATOR.

Frequency, 95 ∿ per sec. Carbons, 13 mm. "Apostle." Current, 14·8 amps. Resistance in series, 0·3 ohm.

Expt. No.	Carbons.		P. D. alternator Volts.	Test arc length. mm.	P. D. Test arc. Volts.	Notes.
	Top.	Bottom.				
193, 194	Solid	Solid	210	2	49	Hissing. Readings only rough approximations
195, 196	Cored	Solid	174	5	39	Hissing. Readings only rough approximations
197, 198	Cored	Cored	160	7·5	35	Hissing. Readings only rough approximations

DIRECT CURRENT ARC.

HISSING.

Carbons $\left.\begin{matrix}+11\\-9\end{matrix}\right\}$ mm. solid "Apostle."

Resistance in series, 2·2 ohms.

Experiment. No.	Arc length. mm.	P. D. arc. Volts.	Current. Amps.
199	0	28·4	24·6
200	1·6	36·2	22

ARC BETWEEN UNEQUAL CARBONS.

ON PYKE & HARRIS ALTERNATOR.

Frequency, 95 ∿ per sec. Carbons, both solid "Apostle." Resistance in series, 0·25 ohm.

Expt. No.	Diameter of Carbons.		P. D. arc. Volts.	Current. Amps.	Notes.
	Top.	Bottom.			
201	7 mm.	18 mm.	53·6	13·3	Hissing arc
202	16 mm.	7 mm.	53·6	13·5	Hissing; same carbons as above

ORIGINAL COMMUNICATION.

NOTES ON ELECTRIC TRACTION BY THREE-PHASE ALTERNATING CURRENTS.

By ERNEST KILBURN SCOTT, Associate Member.

EVER since Mr. C. E. L. Brown surprised the electric world in his characteristic way with the Lugano tramway, Electrical engineers have been more or less on the *qui vive* with regard to this new application of multiphase current. In the meantime there has been steady progress, and with the exception of the last, the following lines are now at work.

Name.	Opened.	Length. Miles.	Maximum Gradient.	Powers Station.	Voltage at Generating Station.	Periodicity per second.	Number of Passengers.	Speed. Miles per hour.	Contractors for the Electrical Equipment.
Lugano	June, 1896	3	6%. Adhesion only	Hydraulic power station at Meroggia, 7½ miles distant. Head of water, 750 feet	5,500	40	24	9½	Brown, Boveri & Co., Baden
Zermatt Gornergrat	Nov., 1897	5½	20%. Rack all the way	Hydraulic power station at Zermatt. Close to line. Head of water, 325 feet	5,400	40	110	4¼	Brown, Boveri & Co., Baden
Jungfrau Little Scheidegg to Eiger Glacier	June, 1898	1¼	25%. Rack all the way	Hydraulic power station at Lauterbrunnen, 4½ miles away	7,000	38	40	4	Brown, Boveri & Co., and the Oerrlikon Co., of Zurich. Overhead line work by the Jungfrau Co.
Evian-les-Bains, Lake of Geneva	June, 1898	19	10·2%. Adhesion only	Generating station 8 miles away	5,200	50	14	6¼	Ganz & Tarsa, of Budapesth Lombard-Gerin & Co., of Lyons
Stansstad-Engelberg	Oct., 1898	14	25% by rack; 5% by adhesion	Hydraulic power from Obermatt. Head of water, 1,250 feet	750 volts on trolley wires	40	40 luggage and mails	3 on rack, 12½ by adhesion	Brown, Boveri & Co., Baden
Burgdorf-Thun, Canton of Berne	Now being equipped	25	2½%	Hydraulic power established on the Kander, near Spiez	16,000 750 volts on trolley wires		Will take through railway carriages. Goods trains 100 tons.	22½	Brown, Boveri & Co., Baden

For some time after it was equipped the Lugano line was subjected to a good deal of hostile criticism, but the cars continue to run satisfactorily and the line is an undoubted success from the financial point of view. The extra expense of the double trolley line, &c., appears to have been much more than covered by the economies effected in the rest of the plant and only made possible by adopting three-phase current.

As the subject is an important one, the following notes may be of interest to the members as showing the very real progress which has already been made.

The *first railway* to be equipped entirely with three-phase alternating current plant was the Zermatt-Gornergrat line. It is a rack railway 5½ miles long, and the heaviest gradient is 20%. The road bed is of the usual mountain railway construction with bonded Vignoles rails and an Abt rack lying midway between them. The trolley wires have each a diameter of ·315 inches, and are supported throughout by span wires. There are two double trolleys of the "lyre" or sliding bar pattern (two per phase), each being insulated in the middle so as to take current from the two wires. The employment of this pattern instead of trolley wheels greatly simplifies the turnouts, &c., and the locomotive can be reversed without trouble, as the "lyre" simply turns over by the friction between it and the trolley wires. There is also no jumping of the wires.

Each train consists of a locomotive and two coaches, one closed and one open, and at its lower end the closed coach rests on the locomotive, thus decreasing any possibility of the toothed driving-wheels riding out of the rack. The locomotive pushes the coaches before it.

When made up the total weight of the train is about twenty-eight tons, thus:

Electric locomotives	10½ tons
Closed coach	5¼ „
Open coach	4 „
Sixty passengers in closed coach...	4½ „
Fifty „ open „ ...	3¾ „
Total ...	28 tons

Two 90 horse-power motors are fitted on each locomotive, and they are of the asynchronous three-phase induction type with wound rotor and contact rings permitting the insertion of resistances. They are capable of starting under full load without absorbing a current higher than that required at full load and normal speed, and they are also capable of starting under a higher load with a proportionately greater current. They have six poles, and run

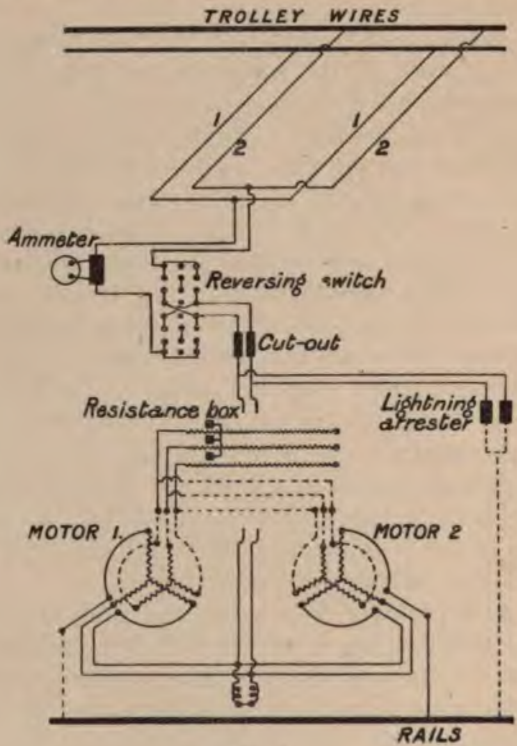


Fig. 1.

at a slightly lower speed than 800 revolutions per minute with a frequency of 40 periods per second. Each motor weighs about 2½ tons, and actuates the rack wheel through double reduction gearing having a total ratio of 12 to 1. The normal speed of the train is 4¼ miles per hour, and the total tractive effort is about 6 tons. Two hand brakes and an automatic emergency brake are provided, this latter acting when the circuit is interrupted or when the speed exceeds the maximum limit.

The figure shows the method of connecting up the motors, switches, &c. The rotor currents are collected by double carbon brushes from slip rings, whence it passes to the resistance placed horizontally above the motors.

Independently of the brakes above mentioned, the motors themselves furnish a simple means of moderating the speed. In descending, the connections are so arranged that, as soon as the train has attained a speed corresponding to that of synchronism, the motors commence to act as generators, the energy developed by the descent of the train being returned to the line, excepting, of course, what is lost in the machines and gearing.[1] The maximum speed which the motors will attain is that of synchronism slightly increased in accordance with the slip, and this constitutes an important advantage of polyphase working on railways with steep gradients.

As it may happen that the descending train is the only one on the line, a resistance is provided (at the Turbine House) which is automatically switched in by the turbine governor when nearly closed. This resistance is arranged to absorb a little more than the power given back into the line, thus leaving a small surplus to enable the turbine regulator to keep proper speed.

This property of being able to restore the energy developed by the descent of the train has been thoroughly proved in actual work. In fact, its use on the Gornergrat line has been so successful that at the suggestion of Messrs. Brown, Boveri & Co., it has also been adopted on the Jungfrau railway. It may be noted that in the event of one wire being interrupted, the motors would continue to rotate as single-phase machines of reduced power. If connection were to be entirely broken the automatic emergency brake would come into use.

As is the case with practically all electrical transmission work in Switzerland the generators are driven by turbines, the heaviest traffic during the season just happening to be at the time when the water power is greatest by reason of the melted snow and ice.

The Generating Station is at Zermatt and utilises the water power of the Findelen River. There are four gene-

[1] On the Paris-Saint Denis accumulator line the motors pump back current into the cells on the down grades.

rating sets, each consisting of Bell & Co.'s horizontal turbine coupled to a Brown Boveri three-phase alternator. Current is generated at 5,400 volts, 40 periods, and this is transformed down to the trolley wire voltage of 540 at three sub-stations, two groups of three 30 kilowatt single-phase transformers being installed in each.

For short descriptions of the Lugano tramway, Jungfrau railway, Evian-les-Bains tramway, Stansstad-Engelberg railway, and Burgdorf-Thun railway, *see* Appendices A, B, C, D, and E.

In street tramway work the duplicated overhead material required for three-phase working is objectionable, but this hardly applies to country tramways or light railway lines. Moreover, with reference to city tramways there is another side to the matter in that conduit lines are making great headway both on the Continent and in America (for example in New York there are something like 90 miles of conduit track either laid down or in hand at the present moment). Now conduit lines always have a complete metallic circuit, that is to say there are two insulated conductors, the rails not being used. With three-phase a third conductor could be added, or else two could be employed as at present, with the rails for the return. It should be noted that there is not the same objection to the use of the rails for three-phase current that there is in the case of continuous currents, because with alternating currents there is absolutely no trouble with electrolysis. In a densely populated area like London, with its millions of pipes leading in all directions, this matter of electrolysis is a most important consideration, and no Board of Trade Regulation will prevent it if continuous current is used largely on earth returns.

When we come to ordinary railway working the question of picking up the current becomes comparatively simple. In heavy railway work, and especially on those lines which run through tunnels, the conductor rails may be laid along the sleepers, and the locomotives provided with hinged contact slippers as on the Liverpool Overhead railway. Three-phase currents would require two conductors (the rails forming the third). In a 4 ft. 8½ in. track there is ample room for *three* conductors, should the Board of

Trade insist upon having them. Several of the Swiss lines are only metre gauge.

The control of three-phase motors being entirely rheostatic, is exceedingly simple and cheap, three ordinary low voltage resistances being connected up to the rotor circuits by slip rings. Although not quite the same thing as the rheostatic control of a series-wound continuous current motor, it has been objected to as being less efficient than series-parallel control with continuous current. It should be noted, however, that series-parallel controlling mechanism is only advantageous where the motors are continually being started and stopped as on a street tramway service. With railways having stated stopping places the advantage of series-paralleling becomes less and less pronounced as the distances between the stations is increased.[1]

A point in connection with three-phase traction which stands out very prominently and may possibly be of immense service in the future in connection with railway working, is the fact that with the controller at the full speed notch, the revolutions of the motors are within limits, independent of the change of load and gradient. It therefore follows that the service may be very regular.

Suppose, for example, that some improvement was introduced on our present steam railways by which the locomotives should always run at certain speeds, how greatly it would simplify the working and organisation generally! Again, on light railways which run alongside the main highways, the fact that it would be impossible for the driver to exceed a certain speed should be a great recommendation

[1] Troubles from sparking on the controlling contacts which proved such a nuisance until the introduction of the magnetic blow-out are almost entirely absent in the three-phase control. In this connection it is interesting to note that alternating current regulating switches may be very easily and cheaply fitted with a small choking coil, which automatically comes into circuit by means of a double brush at each jump from contact to contact, and annihilates arcing as effectively as does the magnetic blow-out. The small resistance used on cell regulating switches is somewhat similar, but is not nearly so effective as a coil having a mass of soft iron entering into its construction.

In order to get a wide range of torque and speed it has been proposed to have a step-up transformer on the locomotive. Varying the number of poles on the Stator by switching coils into parallel is another method of altering speed, whilst it is of course just possible that some satisfactory method of applying the series-parallel system to induction motors may be introduced. For the present, at any rate, simple rheostatic control of the rotor currents gives all the variation that is desired.

in obtaining powers for such lines. The double trolley wires would, of course, be necessary, but on the other hand, ordinary traffic could not come near them, because of the rails and sleepers immediately beneath.

The disturbing effect on telephones, which at first promised to be troublesome, can be prevented. The telephones at Lugano, for example, had mostly earth returns at the time the tramway was put down, but since they have been provided with metallic returns no difficulties have been experienced. On the Engelberg and Gornergrat lines the service telephone lines connecting the various stations run alongside the alternating current wires, and by simply crossing them every 100 or 200 yards or so the noise from induction is prevented. On the Jungfrau line the telephone wires were not crossed so frequently, and there was slight interference, especially on wet days, when the insulation was reduced. It has been objected that inductance of the rails would raise their electro-motive force and so cause trouble, also that a fault on crossed overhead wires is awkward to find, but the fact remains that up to the present, at any rate, no difficulty has been experienced.

In view of the success attained on these Swiss lines, it would appear that all railway lines—the Central London for example—might very well be worked entirely by three-phase, and rotatory converters dispensed with altogether, for, surely, if the three-phase motor will give the necessary torque for working mountain railways with 20 and 25 per cent. gradients, it may be confidently expected to fulfil all the tractive requirements of any railway line in this country.

On the Central London line the rotatory converters simply add another link to the system, very liable to derangement, as it is necessary to use step-down transformers in any case. The rotatory converter is a beautifully simple and efficient piece of apparatus, but the writer cannot help thinking that its position in traction work is an anomalous one. If the distances from the generating station to the sub-stations are such that sufficient economy of copper is given by adopting any pressure under, say, 2,000 volts, a modified form of the Oxford system would appear to meet the case and the plant could be continuous throughout. But if, as will undoubtedly be the case on most railways, the distances are so great that voltages of 5,000, 10,000, or even 16,000 volts

are required, then by all means adopt three-phase alternating currents, not for the transmission alone, but in its *entirety* as on these Swiss lines.

The position of electric traction for railways is especially interesting at the present moment, on account of the proposed changing over of the Metropolitan and District lines from steam to electric. It may be submitted that on certain sections of these lines the stations are so close together that practically all the work is spent in accelerating, but the proper way to look at this question must surely be to take the whole of the system and not just the City portion. The matter really resolves itself into this query: whether it is better to waste a little energy in resistances on the City portion of the lines, or, for the sake of series-paralleling, to dot the whole of the system with rotating mechanism in sub-stations requiring more or less constant attention.

Outside this particular case the writer thinks it will be acknowledged that the three-phase system, pure and simple, has a great field on lines where the cars run long distances between stops and where the advantage of the alternate current transformer *in requiring no attendance* is most fully realised. For example, the Burgdorf-Thun line, see Appendix E., on which ordinary railway passenger traffic is to be dealt with, has eleven transformer stations in a length of twenty-five miles. If rotatory converters were used, either they would be bunched together in two or three sub-stations, in which case more copper would be required, both in the feeders and line, or there would be say eight or nine attendants, one at each sub-station, to look after the rotating machinery, bearings, brushes, &c., and to put the rotatories back into synchronism if they should by any chance get thrown out. For a given drop in the rails more transforming points are necessary with alternate currents, but the equipment is exceedingly simple and cheap, and there is much better regulation.

In preparing these notes the writer is indebted to Mr. C. E. L. Brown, Mr. A. E. Levin, Messrs. Ganz & Co., and others.

APPENDIX A.

The Lugano tramway was the first line to be equipped by three-phase current. The line is divided into three portions and connects the suburbs Paradiso, Molino Nuovo, and Cassarate with the town of Lugano.

The power station is at Maroggia 7½ miles from Lugano, water power is used and the head of water is 750 feet.

The generating plant consists of one horizontal shaft turbine of 300 horse-power direct-coupled to two Brown three-phase inductor type alternators. Current is generated at 5,500 volts, 40 periods, when running at 600 revolutions per minute.

The exciters are small 2-pole iron-clad machines directly coupled to the alternator shafts.

The trolley wires are fed with current at 400 volts. They are ·236 inch diameter spaced 10 inches apart and are 18 feet above the rails.

Each motor car seats 24 passengers and is equipped with one 12-pole motor connected through single reduction gear to the driving axle. The maximum speed of the motor is 400 revolutions per minute (the speed of synchronism) and the car travels at 9½ miles an hour.

It should be mentioned that part of the current generated at the power station is used for stationary motors for power work in Lugano. One 45 horse-power motor is used for the Salvatore Railway.

APPENDIX B.

The Jungfrau Railway is being equipped in a similar manner to the Zermatt-Gornergrat line, and the section from Little Scheidegg to Eiger Glacier, a distance of 1¼ miles, is already completed and working.

The power station at Lauterbrunnen is supplied with water from the White Lutschine, 115 feet head of water being utilised.

The pipe line is 1,500 yards long, 5 feet 11 inches diameter, and is in lengths of 19 feet 8 inches and 24 feet.

The turbines are four in number by Rieter & Co. They are of the twin Gerard type taking 314 gallons of water at full load and giving 500 horse-power at 380 revolutions.

The generators, by the Oerlikon Company, are of the inductor type direct coupled to the turbines and generating three-phase currents at 7,000 volts, 38 periods.

The transmission line from the power-house to Little Scheidegg is 4½ miles long and consists of three bare copper wires ·295 inch diameter mounted on triple-shed porcelain insulators 4½ inches diameter, 7 inches high. The fall of potential at full load is about 10 per cent.

The poles are of wood 33 feet high with galvanised iron brackets.

Sub-stations are spaced 1080 yards apart and are fitted with two 200 kilowatt transformed to reduce the voltage to a little over 500 volts. The secondary feeders are ·355 inch diameter and of hard drawn copper.

The rails are of Vignoles Section 40 lbs. per yard gauge. They are bonded at each joint with two Chicago bonds ·275 inch diameter and cross-connected every 55 yards with ·314 inch copper.

The rack (Strub) weighs 68½ lbs. per yard and is in 11 feet 6 inch lengths. The teeth are conical-shaped in cross section for the emergency brake to clutch against.

The locomotive body is by the Société Suisse pour la Construction de Locomotives et de Machines. Its total weight is 13 tons, and the two pinion driving wheels have each 22 teeth. Three brakes are provided—(*a*) The ordinary hand-brake, which applies brake blocks to a wheel ; (*b*) An emergency brake which grips the head of the rack ; and (*c*) An electrically controlled brake which when current is switched off applies a band brake to the motor spindles, a dashpot preventing slight momentary increase in speed.

The motors and controlling gear are by Brown, Boveri & Company. Each motor gives 150 horse-power, at 760 revolutions per minute. The weight of each motor is 2·6 tons, and four trolleys (two per phase) are employed.

There is one passenger car to each train capable of carrying 40 passengers. It is lighted with 15 lamps, 5 between each pair of wires.

APPENDIX C.

The Evian-les-Bains tramway is a small single track line. Three-phase currents at 5,200 volts are transmitted from Cevenos Electricity Works, eight miles away from the line, and a 30-kilowatt transformer converts the pressure down to 200 volts. The motor is in circuit both in ascending and descending, and in the latter it acts as a brake running at a speed slightly faster than synchronism—that is, 6¼ miles an hour. The same primary mains supply both light and power.

The trolley wires are ·236 inch diameter of hard drawn copper, spaced 11¾ inches apart. The rails form the third conductor and are bonded in the usual way. Two trolley poles of the wheel type are provided.

There is only one motor car, and its weight is 3·8 tons. It carries 8 passengers sitting and 6 standing, and makes 60 double journeys per day.

The motor develops 15 horse-power at 750 revolutions per minute, and can give 25 horse-power for a short period. Double reduction gear is used, the countershaft driven by spur gearing, and the driven axles by chain gearing.

As the gradients are heavy and there is no rack, very large and efficient sanding boxes are supplied.

APPENDIX D.

The Stansstad-Engelberg line was only opened in October, 1898. It is a combination line ; 13 miles being worked by adhesion and rather under 1 mile by Riggenback ladder rack. The maximum gradients are 5 per cent. on the adhesion section and 25 per cent. on the rack.

The passenger service is effected by motor-cars which run by themselves at 12½ miles an hour on the adhesion section, and on the 25 per

cent. gradient they are pushed from behind by a special locomotive, which climbs the rack at 3 miles an hour.

The power station is at Obermatt at the foot of the rack section. The pipe line is 1,790 yards long, 11¾ inches diameter, and the useful head of water is 1,250 feet.

The turbines are two in number by Bell and Co., Kriens. They are of the radial flow type, each giving 180 horse-power at 650 revolutions with a flow of 11 gallons of water per second.

The alternators, by Brown, Boveri and Co., are direct coupled to the above turbines. They have revolving armatures and generate three-phase currents at 750 volts. Another unit is to be added later.

There are two exciters separately driven by 12 horse-power turbines running at 1,700 revolutions. An automatic switch is placed in the exciter circuit, which cuts off current from the alternators in the event of an earth on the high tension line. This line has a pressure of 5,000 volts upon it, and runs alongside the railway for a distance of six miles.

The alternators are connected in parallel on to the bus bars and feed the trolley line direct at 750 volts. Part of the energy is also taken by three 30 kilowatt step-up transformers, and carried by the above-mentioned high tension line to a sub-station midway between Stansstad and the power station where the voltage is reduced to 750.

The trolley wires are ·295 inch diameter, and are suspended 3 feet 2 inches apart and about 14 feet above the rails. The line is divided into five insulated sections, each of which may be supplied with current independently of the rest of the line. Wurts lightning arresters are used.

In one place a tramway worked by continuous current crosses the three-phase line, but although the overhead wiring is considerably complicated, no trouble has been experienced by working. The trolleys are of the "lyre" pattern.

The rolling stock will eventually consist of eight motor cars, nine goods cars, and three locomotives. There are five motor cars in work at present, each weighing fourteen tons, and driven by two 35 horse-power motors running at 480 revolutions per minute. The motors are suspended on the front bogie frame, and single reduction cast-steel gearing running in oil is employed to reduce the speed.

The cars are lighted and heated electrically, current for the lamps being supplied by a small transformer (ratio 750 to 100 volts) placed in the luggage compartment.

Each locomotive is fitted with two 75 horse-power motors running at 650 revolutions, which are alternatively geared to the driving axle to give speeds of three miles or 7½ miles (when on adhesion section) an hour. There are two hand shoe brakes, one on driving wheels and one on motor spindles, and a centrifugal action brake, which automatically comes into gear as soon as the speed exceeds 4¼ miles an hour whilst descending the rack.

If it should happen that the descending tram is the only moving object on the line, a water resistance is switched into the alternator circuit to absorb the surplus power.

APPENDIX E.

Of the very many lines which are in hand or projected the equipment of the railway from Burgdorf to Thun in the Canton of Berne (to be running in April, 1899), is perhaps the most interesting because it is standard gauge and ordinary through passenger traffic from other railways will pass over it. The railway is 25 miles long, and has a maximum gradient of only 2½ per cent. The ordinary passenger service will be effected by motor-cars running at 22½ miles per hour with trailers—these latter being as a rule the through carriages of other companies.

The goods trains will be drawn by locomotives capable of taking a load of 100 tons at a speed of 11¼ miles an hour, and if occasion so demands these same locomotives are also capable of running at 22½ miles per hour for the passenger service.

Current will be collected by means of "lyre" pattern trolleys and the overhead contact lines will be fed at 750 volts from 11 transformer sub-stations distributed along the line. The high tension feeders carry current at the enormous pressure of 16,000 volts, power being derived from the hydraulic station, which is being established on the Kander near Speiz.

The Three Hundred and Twenty-Fifth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, January 26th, 1899—Professor SILVANUS THOMPSON, F.R.S., Vice-President, in the Chair.

The minutes of the Ordinary General Meeting held on January 12th, 1899, were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Associates to that of Members—

Alfred Ernest le Rossignol.

From the class of Students to that of Associates—

Benjamin Shuttleworth Hornby.

Messrs. F. W. Bowden and C. J. Robertson were appointed scrutineers of the ballot for new members.

The CHAIRMAN: It is my duty, in accordance with the Articles of Association, to announce to the meeting that the Council, acting upon the powers conferred upon it by the new Articles of Association, has elected an Honorary Member. Only one Honorary Member can be elected in each year. Under the late Articles of Association the choice of the Council was restricted. They could only elect as Honorary Members persons of distinction who were not in actual practice as electricians or electrical engineers; and all persons in actual practice were excluded.

For two or three years past it had been the wish of the Council to do honour to the greatest living electrical engineer and electrician—I mean Lord Kelvin—by electing him an Honorary Member, but they were debarred from doing so by the effect of the old Article 13, which forbade the election of one who was in active practice. When the new Articles were under consideration, and were being drawn up in the summer and early autumn of last year, this Article was revised in order that we might have in the future the power from which we had been excluded in the past. The new Articles contain no such restriction on that point, and as they came into operation on January 1st, the Council naturally took the earliest opportunity of carrying out its desire, and proceeded to nominate Lord Kelvin for election as an Honorary Member. It communicated its desire to Lord Kelvin, and he has been good enough to say that he is gratified by the wish of the Council to put him into that position, as the first Honorary Member under the new rules, and he has kindly intimated that he accedes to the wish of the Council; whereupon the Council, by virtue of its powers, has this evening elected him an Honorary Member of the Institution.

We have now to enter upon the main subject of the evening, viz., the discussion of the three papers which have been presented upon the question of Rules for Wiring, and which will, as has been announced, be taken as read. The three papers are by Mr. Pigg, "Rules for the Regulation of the Wiring of Premises for connection to Public Supply Mains"; by Mr. Wordingham, "The Regulation of Wiring Rules"; and by Mr. Crompton, "The Institution Wiring Rules."

RULES FOR THE REGULATION OF THE WIRING OF PREMISES FOR CONNECTION TO PUBLIC SUPPLY MAINS.

By J. Pigg, Associate Member.

The importance of ensuring the performance of good work in the wiring of premises for connection to Public Supply Mains for lighting or power purposes can scarcely be over-estimated from whatever standpoint it may be considered. That the importance of this subject is recognised is shown by the attention recently given to it. Hitherto, however, public attention has been mainly concentrated upon the merits of different systems of wiring, or, more correctly, upon systems of protecting wiring, to the exclusion of other matters of hardly less importance.

The writer has recently had occasion to examine the rules issued by various authorities for the regulation of the wiring of premises for connection to Public Supply Mains, with results such as would seem to justify calling attention to them. Such rules partake of the nature of a specification for the proper performance of certain work; but, whilst it is recognised that such rules have, necessarily, to be drawn somewhat broadly to cover a variety of cases, the divergencies in practice, and requirements, observable on examination are so great for what is, practically, the same class of work, and the attainment of the same objects, as to excite surprise.

A cursory examination of the subject shows that there are three classes directly interested in the execution of reliable work. These are—(1) the Consumer, (2) the Guarantor, (3) the Undertaker. Besides these there are others hardly less directly interested, such as the Board of Trade, and the representative bodies of the profession.

Of the three classes enumerated the first is seldom in a position to specify how the work to be executed on his behalf shall be done, or to recognise good work from bad.

Where the conditions under which the work is to be done are issued by the Consumer they are, as a rule, due to the employment of a consultant, by whose advice he is guided. There are, of course, cases where individuals, or companies, engaged, primarily, in other than electrical business find it necessary to employ an electrical staff; and where such firms find it desirable to employ outside labour the specifications are drawn by their own engineers, and the work is carried out under their supervision. In many cases, however, and perhaps in the majority, the magnitude of the work is not sufficient to justify the employment of a consultant, and the Consumer is compelled to place himself under the protection of one, or both, the other classes enumerated.

The second class referred to as directly interested in the execution of good work are the Corporations which, individually or collectively, guarantee the Consumer against such risks as are incidental to the occupancy of the premises, for the purposes for which they are utilised. Such Corporations are the various Fire Insurance Companies who, from the nature of the risks they undertake have, necessarily, to estimate the possibilities of each case submitted to them in all its aspects. Such Corporations in their capacity of Guarantors issue rules which are, more or less, applicable to the generality of cases, and which are subject to modification to suit special circumstances. The Guarantor, where the risk is undertaken, occupies the position of greatest responsibility, in respect of the premises, and represents the Consumer, in so far as any damage sustained by the latter can be satisfied by a pecuniary recompense. It is only necessary to say that, whilst the Consumer obtains advantages of a solid character under such a guarantee, the conferring of these advantages is contingent upon the striking of a bargain between the Consumer and the Guarantor, and where no such agreement exists the Corporations referred to have little or no interest.

The third class interested are the private Companies or Municipal Authorities which undertake to supply the Consumer with electrical energy. The interests of the Undertakers differ materially in character from those of the Consumer or the Guarantor, but their maintenance acts equally in demanding reliable work. Whilst the Consumer,

or the Guarantor on his behalf, is principally interested in the execution of work which shall not endanger his premises or their contents, the Undertakers are more concerned with the safety of their generating and distributing plant, and the maintenance of a continuous supply to their customers.

Hence we find that Undertakers invariably disclaim any responsibility for whatever may happen to the Consumer, or his premises, or their contents, through defects in the work carried out in adapting the premises to the utilisation of electrical power, which they are to supply; even when such work is done under their rules, and when such rules specially ask for facilities for its inspection on behalf of the Undertakers during its progress. The repudiation of responsibility is, of course, natural, but it should not be forgotten that the Consumer obtains an indirect guarantee by the issue of such rules, and from the inspection and testing—and gets it for nothing.

The Board of Trade is interested in the execution of good work in its capacity of guardian of the safety of the public. The representative bodies of the electrical profession are interested in the reputation of the profession, and are looked to by individual members for guidance and advice. Of these representative bodies only one has, as yet, issued such rules. As these bodies, however great their moral authority may be, have no direct control over those entrusted with the execution of such work as is under consideration, it is difficult to see how any rules issued by them can, however admirable they may be, ever become more than the recommendations they are stated to be.

The methods to be employed in adapting premises for the utilisation of electrical energy are, in the main, the same everywhere, and the objects aimed at in the issue of rules do not differ materially. It would, therefore, appear natural to expect some degree of uniformity in the methods of specifying the objects to be attained, at any rate, amongst authorities in the same class. An examination of the rules issued by various authorities for the control of such work shows very considerable differences in practice, and the magnitude of the results to be attained in the completed work; and the differences exist, not only between the

different classes interested, but also between members of the same class.

The Board of Trade regulations for securing the safety of the public refer in very meagre terms to internal wiring and, practically, only fix a maximum leakage as a limit of connection to the supply mains.

The General Rules recommended by the Institution of Electrical Engineers are not capable of being enforced by that body; and it would, therefore, seem as if the specifications under which wiring *must* be done are narrowed down to those issued by the Consumer, or on his behalf by the Guarantor, and those issued by the Undertakers. Whilst this is the case it will not be without interest, if the rules issued by all the classes referred to are contrasted.

One of the most important points of any installation of wiring which is intended to be connected to Public Supply mains is the insulation resistance required by those responsible for, or interested in, the proper performance of the work. The subject of insulation resistance is equally important from the standpoint of any of the classes interested, and it is one upon which there does not seem much room for divergence of opinion. Very considerable differences of opinion, however, do exist in the requirements of different authorities in this respect, not only in the magnitude of the results required, but even in the manner of specifying the results to be obtained, and in the bases upon which the specifications are drawn.

In many cases "insulation," or "insulation resistance," is specified without any indication being afforded of the points between which the difference of potential is to be established for the purpose of the test. In some such cases, however, the method of making the tests may be inferred from the directions respecting the apparatus to be included in the test; but in other cases the question of whether the insulation resistance will be measured "to earth," or "between mains," or both, cannot be determined from the specification. In other cases, again, the insulation resistance specified is "to earth," and, in still further instances, the insulation resistances required "to earth" and "between mains" are both specified.

The manner in which the insulation resistance required for any installation is specified varies greatly with different

authorities. The Board of Trade regulation, No. 41, indicates the minimum insulation resistance by specifying the maximum leakage allowable as a proportion of the maximum current used for the purposes of the installation. The regulation does not directly specify the method of making the tests to determine the value of the leakage current, or the testing pressure to be used, but the only reasonable interpretation would lead to tests "to earth" and "between mains," with a pressure equal to that ordinarily used in the installation. Under such conditions of test, the testing current would have the same value as the leakage current from the wiring during ordinary use.

Such a method of specifying the results to be obtained may be put in the general form :—

$$\text{Leakage Current} = \frac{A}{p}$$

$$\text{Whence} \qquad R = \frac{p\,E}{A}$$

Where p is a constant, depending for its value upon the opinion of the framer of the rule, A is the maximum strength of the working current, in amperes; E is the testing pressure, in volts; and R is the insulation resistance required, in ohms.

The specification of the Liverpool and London and Globe Insurance Company is similar to the Board of Trade regulation already referred to, but differs, however, in that the maximum strength of the leakage current is only half that allowed by the Board of Trade regulation, with double the working pressure for testing.

The rule of the Institution of Electrical Engineers relating to the insulation resistances to be exacted gives the values as the ratios of a constant resistance, to the maximum strengths of the working currents of the respective installations, or, in a general form

$$R = \frac{R_1}{A}$$

where R_1 is expressed in megohms, and A is the maximum current, in amperes, used on the installation. Under this rule also the testing pressure is to be twice that used in ordinary working; and, with the exception of a set of rules issued quite recently, the rule has the distinction of being

the only one that has come under the author's notice in which the duration of electrification is specified.

Another form of specification, very closely allied to that of the Institution of Electrical Engineers, is that in which the insulation resistance is *indicated* by the number obtained by dividing another number by the number expressing the maximum strength of the working current in amperes. In this form of specification the differences exist in the values assigned to the number which is to be divided.

The method adopted by the Phœnix Fire Office differs from those already referred to in that the actual resistances required for installations of different numbers of lamps are directly specified in ohms, and are not to be derived from a general equation. The range throughout which the insulation resistances are specified is between 12 and 1,000 lights, irrespective, as far as the specification shows, of the currents required by individual lamps, and, within the limits to which the rules are applicable, of the pressure used on the installations. The Phœnix Fire Office rules have the distinction of being the only set of rules, with which the author is acquainted, in which the insulation resistances required for installations using alternating currents are double those required for, otherwise, exactly similar installations where continuous currents are used.

A number of authorities specify the insulation resistances required in the same way; but the actual values required vary considerably. In one case, however, which has come under notice, the figures are specified as for 33-watt lamps or their equivalent.

An examination of the figures given by different authorities shows that the requirements differ not only between the authorities concerned, but that differences exist in the requirements for installations of different numbers of lamps to be supplied by the same authority.

Thus the Phœnix Fire Office specifies as under :—

12 lights	... ...	1,000,000 ohms.
25 „	... ...	500,000 „

These figures are equivalent to 12 megohms, and 12·5 megohms, per light, respectively, and the latter figure is maintained throughout the whole subsequent range of the specification. If the figures specified by other authorities

who use this method are examined, in the same way, it will be found that they are by no means so consistent in this respect. In one case, for example, the requirements commence with 60 megohms per lamp, rise to a maximum of 112 megohms, and fall again to 60 megohms, all in a range of 300 lamps. Thus, consumers having installations of 10, 150, and 300 lamps would have to obtain insulation resistances at the rates of 60, 112, and 60 megohms respectively, for connection to the same supply mains.

Another method of specifying the insulation resistance required is that where the values are expressed as the ratio of a constant resistance to the total number of lamps fixed on the respective installations. Such requirements may be expressed, generally, by—

$$R = \frac{R_1}{N}$$

where R is the insulation resistance required; R_1 is a constant resistance for any one set of rules; and N is the number of lamps fixed.

Since R_1 is the insulation resistance per lamp, and for any one set of rules is constant, we see that the differences between such specifications and that of the Phœnix Fire Office is in the wording of the rules, and the values assigned to R_1 only.

Another variation of this manner of specifying the requirements is that in which the insulation resistance is specified as to be not less than

100 lamp-megohms

for installations of above a certain size.

Yet another method of specifying the requirements in this respect is that in which the insulation resistance is to be not less than

$$R = \frac{R_1}{n}$$

where R_1 is a constant resistance, and n is "the number of points."

Besides the differences already noted, considerable differences exist in specifying the insulation resistance between mains, in those instances where it is recognised. In this respect the two Insurance Companies whose rules have been referred to, specify the insulation resistances

required between mains in the same way, *i.e.*, it must not be less than that specified to earth. The actual values for any installation will, of course, differ to the same extent as the difference in their specifications of insulation to earth.

The rule of the Institution of Electrical Engineers does not provide for a test of the insulation resistance between mains, and many other authorities ignore such tests, so far as can be ascertained from their rules.

Some authorities specify that the insulation resistance between mains shall not, in any case, be less than 75,000 ohms, a value, in many instances, considerably lower than that exacted to earth. In one case examined the insulation between mains is to be not less than one-third of that specified to earth. In another case, however, the insulation between mains is required to be double that specified to earth.

Another point of some importance, brought out by an examination of the specifications, is the variation in the limit of insulation, as fixed by various authorities, at which the right is reserved to discontinue supply to an installation already connected to the mains, in consequence of a decrease of the insulation resistance from its original value.

As already stated, the Board of Trade limit for connection is $\frac{A}{10{,}000}$ and many authorities quote this as the limiting value of the leakage current allowable. In other cases, however, specific figures are given for the minimum insulation resistance after connection, and these range from 10,000 to 500 ohms.

Whilst considerable divergences in practice have already been found to exist in the specifications for insulation resistance, a closer examination reveals further differences.

The necessity for specifying insulation resistances is obvious, and arises from the importance of confining the value of the leakage *current* within safe limits. The author has no experience which would justify the assertion that "Electricity can readily change to fire," but that fires may be the direct result of leakage is undeniable.

The strength of the leakage current from any installation depends, not only upon the value of the insulation resistance,

but also upon the pressure acting across that resistance. Hence, it would seem desirable that the insulation resistances specified should bear some relation to the pressure employed as well as to the size of the installation. Given a maximum leakage current, it is obvious that the insulation resistance which is just sufficient to confine the leakage from a 100 volt circuit within the limit imposed, will not be sufficient to do so for 200 volt or 250 volt circuits. Under such a condition the insulation resistances of otherwise exactly similar installations would require to be proportional to the pressures employed.

The other factor determining the strength of the leakage current, the value of the insulation resistance, depends, in some proportion, upon the size of the installation. Other things being equal, the installation containing the largest number of lamps will have the lowest insulation resistance. It by no means follows, however, that the proportion existing between the insulation resistances of different installations will be so regular as would seem to be implied by the proportions given in some of the rules which have been considered, such as, for example,

$$R = \frac{p\,E}{A}, \qquad R = \frac{R_1}{A}, \qquad R = \frac{R^1}{N}$$

The insulation resistance obtained on any installation will depend greatly upon its design as a whole, the grouping of the means of utilising the power required, the number and character of auxiliary fittings, the design of the fittings employed and the care with which they are fixed and connected, and the character of the workmanship in other directions. The equations given above all assume that the means of utilisation of power increase in a regular order, and that leakage will increase in the same degree. In respect of the rate at which the insulation resistance will vary with the size of the installation the equation

$$R = \frac{R_1}{n}$$

is nearer to the exact conditions obtaining.

Consider the equation expressing the Board of Trade regulation already referred to

$$R = \frac{p\,E}{A}$$

where E = testing pressure, A = maximum current used, and

R = the insulation resistance to earth or between mains according to the points between which the testing pressure is established.

If E also equals the working pressure we obtain the insulation resistance as the ratio of the product of the working pressure by a constant, to the maximum current used on the installation. Or, since $\frac{E}{A}$ equals the minimum conductor resistance, the minimum insulation resistance must be equal to the product of the minimum conductor resistance and the constant, or,

$$R = r p$$

It may also be noted that the results obtained are true resistances, and that no exception can be taken to the character of the equations obtained under the regulation.

If we further consider the equation $R = \frac{p\,E}{A}$ we find that the insulation resistances exacted by the regulation for installations taking the same maximum currents, but at different pressures, are directly proportional to the pressures; but for installations taking the same powers at different pressures, the insulation resistances are in proportion to the squares of the pressures.

This equation, as already noticed, assumes that the insulation resistances will vary inversely as the maximum currents employed. This, of course, is not quite correct for all cases. Given two installations with lamps of different candle-power, but of the same efficiency, and in other respects exactly alike, there is no reason to anticipate any difference in the insulation resistances to be obtained. The Board of Trade regulation, however, would require a higher insulation resistance for the installation containing the lamps of lower candle-power. In exactly the same way, it will be noted, that in two installations, otherwise exactly alike, that in which lamps of the highest efficiency are used will require the highest insulation resistance to comply with the regulation.

The grouping of a number of lamps on one fitting will also tend to increase the insulation resistance obtainable, as compared with that obtainable with single lamps, owing to the number of points for leakage being diminished.

If the equations expressing the Board of Trade regula-

tion are further examined we find that they furnish two methods of specifying insulation resistance in addition to that obtained indirectly by specifying the maximum leakage.

From $R = \frac{p\,E}{A}$ we may obtain—The minimum insulation resistance to earth, or between mains, must be such that, if tested with a pressure equal to the product of the constant and the working pressure, the resulting current shall not exceed that specified as the maximum current required for the installation.

From $R = r\,p$ we may obtain—The minimum insulation resistance must not be less than the product of the constant and the minimum effective conductor resistance when all appliances are in use.

Consider next the case of a specification, the wording of which gives the insulation resistance of an installation as

$$R = \frac{R_1}{A}$$

If E_1 = testing pressure, and C = testing current we have—

$$\frac{E_1}{C} = \frac{R_1}{A} \text{ and } E_1\,A = C\,R_1$$

If we consider the first equation, we find that since the denominators of both members of the equation are of the same nature, a pressure must be the equivalent of a resistance; and from the second equation we are led to the conclusion that a power is the equivalent of a pressure. Further, we see that unless the testing pressure is equal to the working pressure, neither member of the second equation has an actual existence.

If we consider the equation $R = \frac{R_1}{A}$ in the equivalent form $R = \frac{r\,R_1}{E}$ where r represents the minimum conductor resistance, and E the working pressure, we see that the insulation resistances required for installations using different pressures, are inversely proportional to the pressures employed.

In two installations taking the same maximum currents, but employing different pressures, the insulation resistances exacted under any one set of rules will be—

$$\frac{R_2}{R} = \frac{r_2\,E}{r\,E_2} \text{ and since } \frac{r_2}{r} = \frac{E_2}{E},\ R_2 = R$$

If the two installations take the same maximum powers at different pressures—

$$\frac{R_2}{R} = \frac{r_2\,E}{r\,E_2} \text{ but since } \frac{r_2}{r} = \left(\frac{E_2}{E}\right)^2, \frac{R_2}{R} = \frac{E_2}{E}$$

If the maximum currents required in the two installations are such as make $r_2 = r$ then $\frac{R_2}{R} = \frac{E}{E_2}$

Under the Board of Trade regulation the insulation resistances required in the three cases considered would be as under:—

With the same maximum currents $\frac{R_2}{R} = \frac{E_2}{E}$

With the same maximum powers $\frac{R_2}{R} = \left(\frac{E_2}{E}\right)^2$

With currents in the same proportion as the pressures $\frac{R_2}{R} = 1.$

The strengths of the leakage currents in the three cases considered under this rule, and under the Board of Trade regulation, are set out below.

Under $R = \frac{R_1}{A}$ *Leakage Currents.*		Under $R = \frac{p\,E}{A}$ *Leakage Currents.*
$\frac{C_2}{C} = \frac{E_2}{E}$	Same maximum currents	$\frac{C_2}{C} = 1$
$\frac{C_2}{C} = 1$	Same maximum powers	$\frac{C_2}{C} = \frac{E}{E_2}$
$\frac{C_2}{C} = \left(\frac{E_2}{E}\right)^2$	Currents in proportion to pressures	$\frac{C_2}{C} = \frac{E_2}{E}$

A very similar method of specifying insulation resistances is that adopted in some cases, in which the wording of the rule is such as indicates that the authors recognise that a *resistance* is not legitimately obtained by dividing another resistance by a current. Hence we find specifications which read as under:—

"The insulation resistance . . . should not be less than the number of ohms which shall be indicated by the number resulting from the division of 40,000,000 by the number expressing the maximum current in amperes . . . ," which, if somewhat involved, has the advantage of being precise. In the actual results obtained this method differs in no way from that just considered. The only difference lies in the

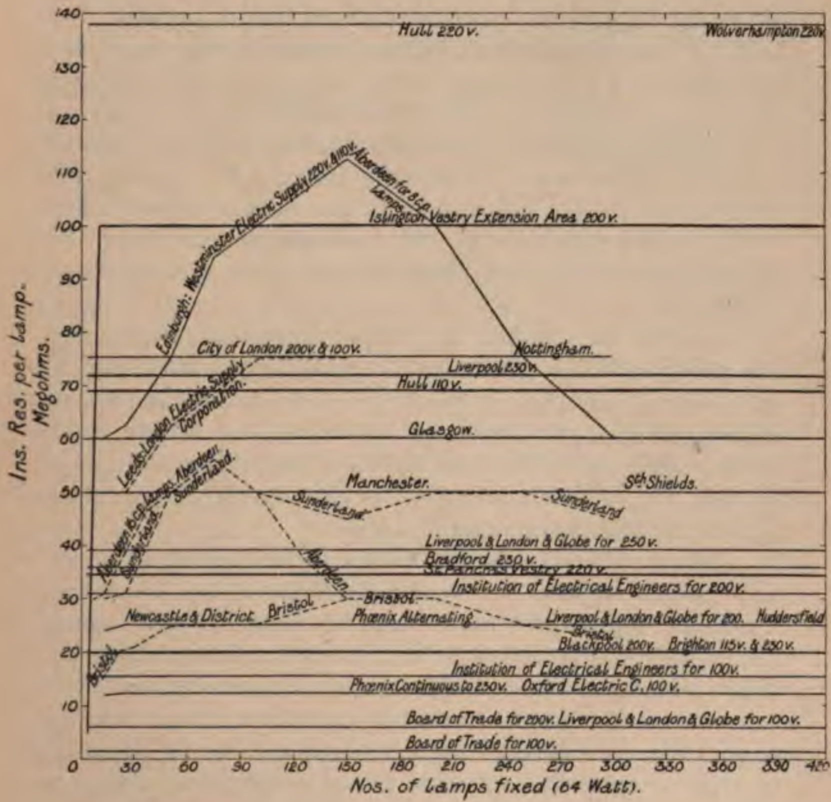
language employed to express the results desired. Obviously it matters little whether we call R_1 and A numbers only, or resistance and current respectively, so long as it is intended that the result of certain operations in which they are involved shall indicate the magnitude of a certain physical quantity. At the same time there is something to be said for precision of language, and against flagrant disrespect of dimensional units and their applicability to the identification of physical quantities.

Some reference has already been made to the method of specifying adopted by the Phœnix Fire Office, and followed, more or less closely, by others. As already remarked, the specification named is based upon a, practically, constant resistance per lamp, although specific figures are given for installations containing given numbers. Hence the specification named is capable of being expressed in the general form $R = \frac{R_1}{N}$, which, of course, is the general form for specifications where the insulation resistance required is defined as the ratio of a constant resistance to the number of lamps fixed. The fact that R_1 is not constant for all specifications and, in some cases, not constant throughout the same specification has already been referred to, and no further comment is needed here.

Such a method of specifying the requirements to be met takes account of only one of the factors involved in the determination of the strengths of leakage currents, viz., the size of the installation, and that not in an exact way, as has already been referred to. The specifications ignore altogether the effect of the pressure employed, although in some instances where this form of specification is in use, different pressures are in use, or allowed.

One of the specifications which is drawn in a similar manner to that of the Phœnix Fire Office, specifies the figures for numbers of 33-watt lamps, or their equivalent. The specification is not drawn on a regular basis throughout its whole range. Had it been so the general form would have resolved itself into $R = \frac{R_1}{E\,A}$ which differs from any other form considered. As the value of R_1 varies at one point to a figure very nearly double that at either end of the range the specification appears to be somewhat arbitrary.

Of the form of specification $R = \frac{R_1}{n}$ where n is the number of *points* it is unnecessary to refer further to it than has been already done, except to point out that it also ignores the effect of variation in the pressure employed and that, for the average installation, the insulation resistance exacted will be greater than where the form $R = \frac{R_1}{N}$ is in use, the value of R_1 being assumed to be equal in the two cases.



Probably the greatest differences in the various specifications are those that exist in the magnitudes of the insulation resistances exacted. In order to show these differences fully it is necessary to reduce them to a common basis. In the accompanying diagram the insulation resistances, per lamp, required under various specifications are shown, for 64-watt

lamps in every case; and where the insulation resistance required depends in any way upon the working pressure, the pressure for which the value of the resistance per lamp is suitable is marked. As will be seen, the values of the insulation resistance required per lamp vary, with the higher pressures, between 138 megohms to 6¼ megohms, which is the Board of Trade limit for 200 volt supply. In the case of representative bodies, the diagram shows the insulation resistances required for the same maximum powers at different pressures. Thus, under the Board of Trade regulation, and the rule of the Liverpool and London and Globe Insurance Company, the insulation resistances are proportional to the squares of the pressures employed. Under the rule of the Institution of Electrical Engineers, and similar specifications, they are proportional to the pressures only. Under the Phœnix Fire Office rule they do not vary in any way with the pressure used. The diagram also shows in a very conspicuous manner the irregular character of the basis of some of the specifications examined.

If we further examine the various specifications, we find that those in which the insulation resistance of the complete installation is inversely proportional to the maximum current employed cover all classes of apparatus that may be used, whilst other forms of specifications have necessarily, or should, to be complete, to specify directly for other appliances than incandescent lamps.

Some diversity is also exhibited in the requirements for such special cases. In some specifications the insulation resistance required for motors and arc lamps is to be equal, respectively, to that required for the number of incandescent lamps taking the same *energy*. In other cases the insulation resistance is to be equal to that exacted for a certain number of incandescent lamps, irrespective of the power taken by the special apparatus specified for, or of the power required by the incandescent lamps which form the basis of the requirements for the special apparatus. In other cases, again, the insulation resistance is to be "to the engineer's approval," and in still other cases the requirements are not specified or referred to in any way.

In one case it will be noted that, although the insulation resistance required for motors follows from the insulation

test for the complete installation, a minimum insulation resistance of 1 megohm is specified. In this case, on the basis of the insulation resistance required for the complete installation, the minimum is reached for a motor taking a little more than 3 E.H.P. Under the Board of Trade regulation the insulation resistance would require to be only 230,000 ohms for such a motor. In another case in which the insulation resistance required for such a motor follows from the insulation resistance exacted for the complete installation, and would be, under the rule, 2 megohms, a minimum of 50,000 ohms is fixed. Under other rules, of similar construction, the insulation resistance required varies between 1 and $2\frac{3}{4}$ megohms approximately.

In other cases where the insulation resistance is specified in terms of a number of incandescent lamps irrespective of the power required, the minimum for any size of motor is 2 megohms.

In another case where the insulation resistance of an installation is specified irrespective of the power taken by those lamps, so far as the specification shows, the insulation resistance required for motors is specified as to be equivalent to that required for the number of lamps taking the same *energy*.

In the case of the use of arc lamps the conditions differ somewhat owing to the differences between the pressure required for one lamp of ordinary construction and the usual pressures of supply. Where the insulation follows from the prescribed insulation test, the result is not the insulation per lamp, but that per circuit of such lamps. Hence the insulation resistance per lamp will vary with the pressure and the current. Thus, under the Institution of Electrical Engineers rule the resistance exacted for a circuit of arc lamps, taking 10 ampères, would be 1 megohm, and, under other specifications of a similar character, 4 megohms. If these lamps are arranged two in series on 100-volt circuits, the insulation resistance, per lamp, would be 2 and 8 megohms respectively. If arranged four in series on 200-volt circuits, the insulation resistance, per lamp, would be 4 and 16 megohms respectively. Under the Board of Trade regulation the insulation resistances for 100-volt and 200-volt circuits would be 100,000 and 200,000 ohms respectively, and the insulation resistances, per lamp, 200,000 and 800,000

ohms respectively. Under other specifications of various construction, the insulation resistances, per arc lamp, irrespective of the power required, varies between 2 and 5 megohms. In another case, where the insulation resistance of arc lamps is directly specified, the figures given are those which are expected *per circuit* divided by the number of lamps on such circuit, and therefore not independent of the pressure employed.

Another point in which some diversity of practice is apparent is the mounting of motors. In the great majority of cases motors are to be efficiently insulated from earth, together with all their accessories. This is varied somewhat in certain specifications by requiring the frames of motors to be earthed for pressures above a certain specified limit. In one case the limit is 250 volts. In another case it is 460 volts. In another case, however, the framework of motors is to be connected to earth by a copper conductor of a sectional area equal to that of the leads up to a maximum of 10 square millimetres.

Another point which may have attention called to it is the inference which might be drawn by any one sufficiently depraved from the wording of a number of the specifications with reference to the testing pressure to be employed. In many cases we find that "*current* will not be supplied, *if, when testing with a pressure of — volts*, the insulating resistance is below. . . ." The italics are the writer's, of course.

Leaving on one side the fact that what is supplied is "power," and what is charged for is "work," the remainder might cause an inquiry as to whether resistance was dependent upon the pressure used in measuring it. Although it may be desirable, *before measuring*, to break down an incipient fault by the use of a higher pressure, the use of that pressure *for the purpose of the measurement* has no effect on the value obtained.

At the same time it may be worthy of consideration whether the use of a pressure higher than that used in working, of such values as are ordinarily available, is of much use in breaking down an incipient fault under the ordinary conditions of wiring. It is not desired to advocate a return to the method of testing which has been described as being done with "a forty-guinea bridge, a

leaky cell, and a detector which stuck on the slightest provocation," but it would seem to be a particularly bad case upon which the usual testing pressures would have the desired effect, the more especially as the testing battery or generator seldom has much power. The real object of testing insulation resistance is the ascertainment of the leakage to be expected under working conditions, although this seems to be sometimes partly lost sight of, and it would seem, seeing that no greater pressure is to be applied in working, that the working pressure would be most suitable for testing. There are, of course, cases where higher testing pressures may be of advantage, but these are in the case of installations which have become defective.

Another point may be shortly referred to in connection with the wording of specifications in which pressures for testing purposes are to be higher than the working pressures. For instance we find the following, "The whole of the lamps or appliances for utilising the energy having been connected to the conductors, and all fuses being in place, an E.M.F. equal to twice the *E.M.F. which will be ordinarily used. . . .*" It need hardly be pointed out that installations taking power from public supply mains have a difference of potential established at the terminals, but not an *E.M.F. in ordinary use.*

It will be observed that in several of the specifications the insulation resistance required for motors is to be "equivalent to the number of lamps as represented by the *energy* required for the motor."

If *energy* is measured by its power to do work the element of time will require taking into account, and it would be possible so to proportion the relative times of the use of the lamps and the motors respectively as to make the insulation resistance which the latter should have, under the rules, of any desired value. Obviously the specifications should have reference to the "power" required for the motor.

Besides the differences already touched upon, a study of the specifications reveals numerous other differences of greater or less importance. To deal with these in detail would be tedious in the extreme. There are considerable differences in the insulation resistances exacted for conductors. These range from 2,500 megohms to 300 megohms as minima in different specifications. In many specifications

different values of insulation resistance for conductors are assigned for use in different positions, and differences exist amongst these In some cases tests are specified for ensuring the quality of the cable, and details of time of immersion, temperature of water, time of electrification, and testing pressure are all duly set forth ; and in some cases the maker's certificate that these conditions have been duly complied with *may* be required, presumably as a guarantee ! In some cases different tests are specified for lead-covered cables ; in one specification "lead-covered wires should not be used under any circumstances."

Some slight differences exist with reference to joints, and the methods to be followed, but the surprising point in this respect is the number of specifications which ignore joints altogether.

Differences also exist in the maximum current density allowed ; in the maximum "drop" to the farthest consuming device ; and in the per-centage conductivity, with or without a standard. Other differences with respect to conductors exist in the specification for the smallest wire to be used, and in the smallest unstranded wire. Again, "bunching" of "positive conductors together, or of negative conductors together" may only be done, in some cases, by "permission." But wires which under certain conditions are practically at the same potential, may, under other conditions of ordinary use, have the full difference of potential between them. It does not follow that a wire has a potential of the same sign whether the consuming device is in operation or not.

Differences also exist with reference to cut-outs, ceiling roses, and lamp switches. In some cases double-pole cut-outs are specified ; in other cases there must be two single pole cut-outs. Ceiling roses in some cases must not be provided with fuses ; in other cases they must have them. In one case the only possible interpretation of the rule requires two single-pole cut-outs to be provided for every ceiling rose. On the other hand the writer heard of, but did not see, a specification which called for ceiling roses with two fuses in the fitting. Differences exist in the specifications for lamp switches chiefly in the length of "break" required. In one specification the use of double pole switches, or two single-pole switches, is preferred

throughout the whole installation. In some cases the specifications require "the metal part (of switches and cut-outs, &c.) preferably arranged so that the screw heads, and so forth, do not come through to the bottom of the base; should they do so, the holes must be filled up with plaster of Paris, well shellaced, or some equivalent material." The writer has seen such fittings so protected, with large breaks, insulated covers, and everything handsome about them, which, when fixed, provided a distance of about $\frac{1}{16}$" between the live parts of the switch and the head of the screw fixing the switch on the block. In the case of some so-called high insulation ceiling roses, the wires are required to pass through holes separated by a wall of about $\frac{1}{16}$" thick, and in many cases the size of the holes has been found insufficient to allow the wires to pass through without stripping off the insulation.

Differences also exist in the requirements for main switchboards. In most cases where all three cables on a three-wire system are taken for connection to an installation, the use of three poles, single or coupled together, is specified; in other cases, the use of a treble-pole main switch is not recommended; and in still other cases each side is to be treated as a separate two-wire system, and complete disconnection provided for.

Other instances of differences of practice might be readily adduced; but probably sufficient has been already given to show the desirability of more concerted action in drawing up rules for the regulation of such work as has been considered. It is true that, in some cases, specifications do bear a certain resemblance to each other, but the resemblances are not necessarily the result of concerted action, but are more probably due to different specifications being drawn up under the direction of one person. Of the necessity for the taking of counsel together for the drawing up of rules which may, at least, form a basis for securing greater uniformity in such specifications there can be very little doubt. Differences of opinion are, no doubt, indicative of originality of thought, but such differences may be eliminated in consultation. Rules drawn up as the result of a conference between those responsible for the issue of specifications for the guidance of those to whom the work is entrusted, could scarcely fail to secure a greater degree of uniformity than exists at present.

APPENDIX I.

EXTRACTS RELATING TO INSULATION RESISTANCES OF COMPLETE INSTALLATIONS, &c.

CLASS I.

BOARD OF TRADE REGULATION LIMITING CONNECTION TO SUPPLY MAINS.

(*Connection to Consumer not to be made where Leakage would result.*)

THE undertakers shall not connect the wires and fittings on a consumer's premises with their mains unless they are reasonably satisfied that the connection will not cause a leakage from those wires and fittings exceeding one ten-thousandth part of the maximum supply current to the premises; and where the undertakers decline to make such connection they shall serve upon the customer a notice stating their reasons for so declining.

THE INSTITUTION OF ELECTRICAL ENGINEERS.

(Rule 15.)

Testing.—The conductors, fittings, and appliances must be tested in the following manner before the current is switched on:—The whole of the lamps or appliances for utilising the energy having been connected to the conductors, and all fuses being in place, an E.M.F. equal to twice the E.M.F. which will be ordinarily used is to be applied, and the insulation resistance between the whole system and earth must be measured after one minute's electrification. The insulation should then not be less than 10 megohms, divided by the maximum number of ampères required for the lamps and other appliances. The installation may then be set to work, and a second and similar test should be made after an interval of 15 days. In each test, if the insulation of the whole is below standard, the work should be divided up by the departmental switches and tested separately, in order to locate the faulty section.

The value of systematically testing and inspecting apparatus and currents cannot be too strongly urged as a precaution against fire. Records should be kept of all tests, so that any gradual deterioration of the system may be detected. Cleanliness of all parts of the apparatus and fittings is essential. No repairs or alterations should be made when the current is "on."

CLASS II.—GUARANTORS.

Liverpool and London and Globe Insurance Company.

113. The completed installation should be so insulated from earth that the total leakage to earth does not exceed $\frac{1}{20000}$th of the maximum current used, when tested at double the working voltage, or at 200 volts. (See also V., page 6.)

114. The amount of leakage from + to – with all the current producing and consuming devices disconnected should also not exceed the above.

115. In concentric wiring only the latter test need be taken if an uninsulated return system is used.

116. This amount of leakage should not be exceeded, either at the time of the first completion, or at any subsequent period, and it is desirable that every large installation should have suitable leakage indicating devices permanently fitted as part of its equipment.

V. Precautions in excess of those specified in these Rules *may* be prescribed by the Regulations of the Board of Trade or of the various undertakers working under the Electric Lighting Acts (precautions rendered necessary by the peculiar conditions of production or supply of current). In such circumstances these additional precautions should be observed as well as those herein specified, but in no case should the Rules of the Liverpool and London and Globe Insurance Company be deviated from without special permission from the Company, although the Board of Trade or other Regulations may prescribe a lesser degree of safety as being sufficient for the *safety of the public.*

Phœnix Fire Office Rules.

Tests.—34. In any electric light installation in which the current is continuous and has an electro-motive force of 230 volts or under, the insulation resistance both with regard to earth and between conductors over the whole installation must not be below the following :—

Installations of 12 lights	1,000,000 ohms.
" 25 "	500,000 "
" 50 "	250,000 "
" 100 "	125,000 "
" 500 "	25,000 "
" 1,000 "	12,500 "

When the lights are proportionate between the above numbers, then the insulation resistance should be correspondingly proportionate.

The insulation resistance of the separate circuits or branches of the installation should also be taken, and should not be less than the above table.

The minimum insulation resistance for currents of higher electromotive force than 230 volts will be decided with regard to each instance as it arises, so much depending upon the particular circumstances of the case.

For alternating currents of similar electro-motive force the minimum insulation resistance must be twice the number of ohms respectively.

Under normal conditions the fall of the potential in the conductors in a building should not exceed two volts at the farthest point of any circuit when all the lamps are alight.

Under certain circumstances the Technical Adviser of the Fire Office may give permission for the insulation resistance to be less than that contained in the before-mentioned table.

A statement of the insulation tests must be supplied if required.

All tests should be regularly entered in a book kept for the purpose.

An isolated installation should contain an automatic device that would give a warning if a leak were set up to earth in any part of the premises.

The pressure of the testing current should not be less than 100 volts if possible.

CLASS III.—UNDERTAKERS (Municipal).

Aberdeen.

5. . . . The Corporation will not supply energy to any installation—

a. If there are any temporary wires or fittings.

b. If the City Electrical Engineer is not satisfied that the installation is safe and of a substantial character, both as regards material, workmanship, and general arrangement.

c. If the electricity is used in such a manner as to cause fluctuations in the lights of other consumers.

d. If, when testing with a pressure of 220 volts, the insulation resistance is below the following standard :—

For the equivalent of

33 Watt Lamps.		Megohms.	33 Watt Lamps.		Megohms.
12	...	5·00	150	...	·75
25	...	2·50	200	...	·50
50	...	1·50	250	...	·30
75	...	1·25	300	...	·20
100	...	1·00			

When the Corporation's Inspector makes his tests, it will be necessary for the Wiring Contractor to have men present to explain the routes of the different circuits, and to give information regarding the gauges of the different wires, &c., also to provide any ladders, &c., that may be required for examining ceiling roses, &c., and, to save time, it is advisable that, on the arrival of the Corporation's Inspector, the installation should have all fuses inserted and switches on. The lamps, however, should not be in their holders. In this condition the installation will be tested between poles, which test must give at least one-third of the insulation resistance required to earth as given in the above table. After the test between poles has been taken, the Contractor must insert the lamps before the test to earth is taken, and if the Inspector is in

every way satisfied with the installation, the supply will at once be turned on.

13. All motors must be mounted so that they are efficiently insulated from earth; the insulation test required will be in accordance with the equivalent number of 33-watt lamps as represented by the energy required by the motor at full load. . . .

BRIGHTON.

8. The attention of the consumer is drawn to the fact that most Fire Insurance Companies issue regulations as to the laying down of installations, which they require to be complied with before they will accept risks on premises lighted by electricity. The observance by the Consumer of those regulations when the installation is first laid down will probably save subsequent annoyance and expense.

12. Should any leak or "earth" be discovered by the Consumer, the supply must be turned off at the main switches, notice thereof given without delay at the Corporation Electricity Station, North Road, and the defect should at once be remedied.

Should at any time the insulation resistance of an installation connected to the supply mains be found to be below the required standard stated below the Corporation will be obliged to disconnect the installation with or without notice, according to the seriousness of the defect.

16. Requirements of the Corporation as to Wiring, Fittings, &c.

Before any installation can be connected to the Corporation service lines, it must be approved and tested to the satisfaction of the Corporation or their authorised officer, and conform with the following requirements, for the carrying out of which the Consumer should hold his Contractor responsible :—

(*a*) . . .

(*b*) . . .

(*c*) . . .

(*d*) The whole installation must have an insulation resistance at least equal to 20 megohms divided by the number of lamps actually installed (each arc lamp or electric motor being considered as equivalent to 10 incandescent lamps).

BLACKPOOL.

Insulation Resistance.—The whole installation when completed must have an insulation resistance to "earth" at least equal to 20 megohms divided by the number of lamps actually installed (each arc lamp or motor being considered equal to 10 incandescent lamps) save and except as provided under "Insulation of Conductors."

Insulation of Conductors.—The insulation resistance of every rubber-covered cable or lead must not be less than at the rate of 2,500 megohms

per statute mile with one minute's electrification, after immersion in water for 24 hours, and tested with a pressure of at least 400 volts. Cables and wires insulated with material other than rubber and lead-covered, must have a sufficient radial depth of insulating material to ensure the requisite mechanical strength, but a high insulation resistance is not recommended under these circumstances. Cables of this class must be tested with an alternating difference of potential between the conductor and the lead-covering of 800 volts for one hour after immersion in water for 24 hours. The maker's certificate to this effect must be produced if required. Further, installations in which cables and wires of this description are used will not be subjected to an insulation resistance test, but to an alternating pressure of 400 volts between the conductors and the lead-covering for fifteen minutes. All rubber-covered conductors must have a coating of pure rubber, and one of vulcanised rubber, and then braided, or otherwise suitably protected against mechanical injury.

Leakage.—Should at any time the insulation resistance of an installation connected to the supply mains be found to be below the required standard stated above, the Corporation will be obliged to disconnect the installation with or without notice, according to the seriousness of the defect.

Fire Insurance Rules.—Work on consumer's premises must be strictly in accordance with the Rules of the Fire Insurance Companies and of the Institution of Electrical Engineers.

Bradford.

28. The insulation resistance of the entire installation, measured at the House Terminal Box of the Corporation Mains, between the extremity of either of the House Mains belonging to the consumer and the earth (when all the mains, wires, fittings, lamps, motors, and other appliances are fixed), shall not, as proved to the satisfaction of the Electrical Engineer, be less than the total number of ohms, which shall be indicated by the number resulting from the division of 10,000,000 by the number expressing the maximum electric current in amperes, which has been named by the consumer in his application for his supply of electricity.

29. In the event of the insulation at any time falling below the above standard, and continuing so, the Corporation may, after due notice, discontinue the supply of electricity to the premises.

35. Both the electric motor and starting switch to be efficiently insulated in all parts from "earth," and coils of motor to frame of motor to be not less than 1 megohm insulation resistance.

Bristol.

(To be revised shortly.)

All fittings, lamps and wires beyond the terminals of the supply wires must be provided, erected, and maintained in working order by

the consumer; and, except where otherwise specified, the whole of the work and materials must be in accordance with the rules and regulations of the Phœnix Fire Insurance Company, and to the satisfaction of the Electrical Engineer of the Corporation or his representative.

Insulation.—The Corporation will not supply current to any installation—(1) If there are any temporary wires or fittings; (2) If the whole of the work is not completed and all fittings fixed; (3) If when testing with a pressure of 100 volts the insulation resistance is below the following standard:—

For	10 lamps	1,500,000 ohms.		For	150 lamps	200,000 ohms.	
,,	20 ,,	1,000,000	,,	,,	200 ,,	150,000	,,
,,	30 ,,	700,000	,,	,,	250 ,,	100,000	,,
,,	50 ,,	500,000	,,	,,	300 ,,	75,000	,,
,,	100 ,,	250,000	,,				

. . . If at any time the insulation should be found to have fallen to 10,000 ohms or less, or the installation be found defective in any other way, the Corporation may discontinue the supply until the defect has been remedied.

Edinburgh.

10. *Testing.*— . . . The Corporation will not supply current to any installation—(1) If it contains any temporary wires or fittings; (2) If the whole of the work is not completed and all fittings fixed; (3) If when testing with a pressure of 230 volts the insulation resistance is below the following standard:—

For	12 lamps	5	megohms	For	150 lamps	0·75	megohms
,,	25 ,,	2·5	,,	,,	200 ,,	0·5	,,
,,	50 ,,	1·5	,,	,,	250 ,,	0·3	,,
,,	75 ,,	1·25	,,	,,	300 ,,	0·2	,,
,,	100 ,,	1·00	,,				

Previous to a test being made by the Corporation Inspector, every fuse in the whole installation must be inserted, all switches turned on, and the lamps removed; a test for insulation between wires will then be made, and in no case must the insulation fall below 75,000 ohms. After the test under these conditions is completed, all the lamps must be put in, and it is the duty of the Inspector to see that every lamp lights properly.

12. *Motors.*—All motors must be mounted so that they are efficiently insulated from earth, the insulation test required will be in accordance with Rule No. 10, and will be taken at the equivalent number of lamps as represented by the energy required by the motor. . . .

13. Special Rules for Motors suitable for 460 volts pressure:—

The frame and shaft of the motor is to be efficiently connected with earth. The mains from the switchboard to the motor are to be enclosed

in iron or brass pipes, these also being efficiently connected to earth. . . .

17. *Leakage.*—In the event of the insulation resistance of an installation falling below 10,000 ohms to earth, it is liable, at the discretion of the Corporation, to be entirely disconnected from the mains at once. . . .

Glasgow.

(To be revised shortly).

INSULATION.

21. All work on consumers' premises must also be carried out in strict accordance with the rules issued by the Phœnix Fire Insurance Company, but only in so far as these rules are consistent with the terms and condition of supply of the Corporation.

23. *Incandescent Lamps.*—The insulation resistance between the whole system and earth shall not be less than 60 megohms, divided by the number of lamps fixed, irrespective of their power, when tested with 200 volts.

Arc Lamps.—The insulation resistance to earth shall not be less than 5 megohms, divided by the number of lamps, when tested with 200 volts.

Motors.—The insulation resistance of any part of a motor which carries current, together with all leads, sliding contacts, or metal work connected to the circuits of the motor, shall not be less than one megohm, when the magnets, framework and shaft of the machine are connected to earth.

If it is desired to use current for other purposes, special conditions must be arranged with the engineer.

28. The Corporation is prohibited from making connection with any consumer if they are reasonably satisfied that there is a leakage on the wires and fittings to the extent of one-ten-thousandth part of the maximum supply current applied for—thus, for every 10 ampères applied for, the leakage, when all fittings are on, must not exceed ·001 of an ampère.

40. Motors must be efficiently insulated from earth. . . .

Huddersfield.

No special rules issued. Rules of the Phœnix Fire Office in force.

Kingston upon Hull.

Testing.—When testing with a pressure of 200 volts the insulation resistance between the installation and "earth" should not be less than the number of ohms which shall be indicated by the number resulting from the division of 40,000,000, by the number expressing the maximum

electric current in amperes which has been named by the consumer in his application for supply of electricity, thus :—

$$\frac{40,000,000}{\text{amperes required}} = \text{Insulation resistance in ohms.}$$

The insulation between wire and wire should in no case be less than 75,000 ohms. Previous to a test being made by the Corporation, every fuse in the whole installation must be inserted, all switches turned "on," and the lamps removed from the holders.

8. All motors should be mounted so that they are efficiently insulated from earth. . . .

THE VESTRY OF ST. MARY, ISLINGTON. (Extension Area.)

3. *Insulation Tests.*—The lowest permissible insulation will be 100 lamp-megohms where more than 10 lamps are fixed, with a minimum of 5 megohms where the total number of lamps do not exceed 10. The insulation will be measured (1) to earth with all lamps and fittings in position and all switches turned on, and (2) between the two conductors with lamps removed, the tests being at the ends of the house cables, where they connect to the Vestry's terminal box.

The Vestry reserves to itself the right of refusing to connect up to its mains, any installation, the insulation of which is not up to the above standard. Should the installation at any time fall below the standard specified, or the wiring become defective or unsafe, the Vestry may, after due notice, discontinue the supply of electricity to the premises.

The connection of any part of the conducting circuit to earth will not be allowed.

10. *Apparatus.*—All special apparatus, such as motors, heating apparatus, arc lamps, impedance coils, &c., must be approved in writing by the Vestry's Electrical Engineer before being fixed. . . .

Special rules will be issued for supply of current to motors and apparatus liable to injuriously affect the general supply of electricity. . .

LIVERPOOL.

Insulation resistance of the completed installation, or of any section of the same, with all cut-outs fixed and switches turned on, but with lamps out, must not be less than the number of ohms arrived at by dividing 20,000,000 by the maximum current in amperes, the insulation resistance being measured between the mains, and also from each main to earth at a pressure of not less than 200 volts.

Motors must be thoroughly insulated. . . . The insulation resistance of the windings of the motor to the frame must not be less than 50,000 ohms. . . .

Leakage.—No portion of the installation shall be connected to earth, and in the event of the insulation resistance falling below the limit of

safety, the Corporation shall have the right to disconnect the installation from their mains without previous notice. . . .

NOTTINGHAM.

Testing.— . . . The Corporation may refuse to supply current to any installation—(*a*) If the work is badly executed; (*b*) If the whole of the work is not completed and all fittings permanently fixed; (*c*) If, when tested with a pressure of 100 volts, the insulation resistance is below the following values, viz.:—

For 10 lamps	7·5 megohms.	For 100 lamps	·75 megohms.
„ 20 „	3·75 „	150 „	·5 „
„ 30 „	2·5 „	200 „	·375 „
„ 40 „	1·9 „	250 „	·3 „
„ 50 „	1·5 „	300 „	·25 „
„ 75 „	1·0 „		

Previous to the test being made by the Corporation, every fuse in the installation must be inserted, and all switches on.

Leakage.—The Corporation will cut off supply to the whole or part of any installation if the insulation becomes defective.

Motors.—Motors must be insulated from the earth, the insulation resistance must be equivalent to the number of lamps as represented by the energy required for the motor. . . .

VESTRY OF ST. JOHN, HAMPSTEAD.

9. In all cases where are lamps, motors, or other apparatus are intended to be used, special arrangements must be made with the Vestry.

REGULATIONS FOR INSTALLATION WORK.

3. The whole of the work in the installation must be of the highest class, and carried out in accordance with the rules of the fire insurance company in whose office the building is insured, or, if not insured, in accordance with the rules of the Institute of Electrical Engineers.

Testing.—12. The Vestry will not supply current to any installation—(1) If there are any temporary wires or fittings; (2) if the whole of the work is not complete and all fittings fixed; (3) if, when testing with the full pressure of 105 volts, the insulation resistance shall be below the following standards, viz., 75 megohms divided by the number of lamps. . . .

Leakage.—14. In the event of the insulation resistance of an installation falling below 500 ohms after a supply has been turned on, the whole of the installation will be cut off until the fault has been put right. . . .

15. The Vestry reserves the right to test any installation from time to time.

Vestry of St. Pancras.

36. *Insulation of Complete Installation.*—The insulation resistance in ohms between "earth" and the complete installation, measured at the Vestry's main terminal fuses, with every lamp, fitting, or other apparatus connected, must not be less than the number found by dividing 10,000,000 by the total amperes required to feed the installation.

38. *Low Insulation.*—Should the insulation at any time fall below the standard specified herein, the Vestry may, after due notice, discontinue the supply of electricity to the premises.

42. *Fire Insurance Rules Observed.*—Work on consumers' premises must be strictly in accordance with rules of the Fire Insurance Companies, and of the Institution of Electrical Engineers.

South Shields.

Testing.— . . . No current will be supplied to an installation—(1) until the Inspector of the Corporation has tested the insulation resistance, and certified that the work is satisfactory; (2) until the whole of the work is entirely finished; (3) if, on testing, the insulation is found to be below the following standards, viz., 50 megohms divided by the number of lamps; and when all switches are on and lamps out—*i.e.*, between mains—100 megohms divided by number of lamps. All fuses must be put in previous to the test being made.

The Corporation rêtain the right to cut off the supply to any installation, or part thereof, in the event of the installation becoming defective or otherwise unsafe. . . .

Special Apparatus.—All special apparatus must be approved by the Borough Electrical Engineer before being fixed.

If it is proposed to carry out the work in a manner, or to use apparatus other than described above, full particulars must be sent to the Borough Electrical Engineer, and his approval obtained in writing, before the work is commenced.

Manchester.

(36) *Insulation.*—Before connecting the premises of any consumer, the installation will be tested for insulation, and it will be tested from time to time to ascertain that the insulation has not deteriorated. The whole of the lamps or other consuming devices being connected to the conductors, and all fuses being in, the insulation resistance between the whole system and earth will be measured after not less than one minute's electrification. The conditions to be fulfilled by incandescent lamps, arc lamps, motors, radiators, and fittings are set forth respectively in Regulations Nos. 37, 38, 46, 56, and 35. For other consuming devices special conditions, which may be obtained on application to the City Electrical Engineer, will be required to be conformed to.

(35) *Gas-fittings.*—No gas-fittings may on any account be adapted

to electric lamps unless they are efficiently insulated from the gas-pipe at the point at which they are attached to the same; the insulation resistance between a fitting so insulated and the gas-pipe must be not less than 1 megohm, and their use, even when so insulated, should be avoided as far as possible.

INCANDESCENT LAMPS.

(37) *Insulation Resistance.*—The insulation resistance of incandescent lamp circuits must not be less than 50 megohms, divided by the number of lamps fixed, irrespective of their power up to 50 candles—that is to say, for installations of—

25	lamps	the insulation	resistance	must be	2,000,000	ohms.
50	,,	,,	,,	,,	1,000,000	,,
75	,,	,,	,,	,,	666,000	,,
100	,,	,,	,,	,,	500,000	,,
200	,,	,,	,,	,,	250,000	,,
300	,,	,,	,,	,,	167,000	,,
400	,,	,,	,,	,,	125,000	,,

When lamps of higher candle power are fixed, their equivalent in 50-candle power lamps will be taken as the number of lamps for the purposes of this test.

In cases where the lamps are divided up into separate circuits in order to comply with Regulation No. 28, each circuit will be treated as a separate installation.

ARC LAMPS.

(38) *Insulation Resistance.*—The insulation resistance of arc lamp circuits shall not be less than 5 megohms, divided by the number of lamps fixed for lamps taking current up to 15 amperes; for larger sizes their equivalent in 15-ampere lamps will be taken for the purposes of this test.

MOTORS.

(46) *Insulation Resistance.*—The insulation resistance of any part of a motor which carries current, together with all leads, sliding contacts, or metal work connected to the circuits of the motor, shall not be less than 1 megohm when the magnets, framework, and shaft of the machine are connected to earth.

(47) *Framework to be Connected to Earth.*—The magnets, frame, and shaft must be permanently connected to earth by a copper conductor having a sectional area equal to that of either of the two mains leading to the motor. Where these conductors exceed 10 square millimetres in sectional area, the area of the earth conductor shall be not less than 10 square millimetres, but need not be more. It is preferable that the earth connection should be made by means of a water-pipe or copper-plate buried in moist earth. Gas-pipes must on no account be used for the purpose.

SUNDERLAND.

17. *Testing.*— . . . The Corporation will not supply current to any installation—(1) If there are any temporary wires or fittings ; (2) If the whole of the work is not finished and all fittings fixed ; (3) If, when testing with a pressure of 200 volts, the insulating resistance is below the following standard :—

For 12 Lamps or under	2·5 megohms.	Over 100 and up to 150 Lamps	0·3 megohms
Over 12 and up to 25 Lamps	1·25 ,,	,, 150 ,, 200 ,,	0·25 ,,
,, 25 ,, 50 ,,	1·00 ,,	,, 200 ,, 250 ,,	0·2 ,,
,, 50 ,, 75 ,,	0·75 ,,	,, 250 ,, 300 ,,	0·15 ,,
,, 75 ,, 100 ,,	0·5 ,,		

Previous to test being made by the Corporation's Inspector, every fuse in the whole installation must be inserted, all switches turned on, and the lamps removed. A test for insulation between wires will then be made, and in no case must this insulation fall below 75,000 ohms.

After the test is made under these conditions, all lamps must be put in, and it is the duty of the Inspector to see that every lamp burns properly.

Should any installation at any time fall below 5,000 ohms, the Corporation may, after due notice, discontinue the supply of Electricity to the premises until the fault has been put right. Any circuit which is found defective by the Borough Electrical Engineer will be disconnected from the supply, and a printed notice will be affixed at the junction of this circuit with the supply, forbidding the use of this defective circuit until put in order and passed by the Inspector.

WOLVERHAMPTON.

12. The insulation resistance of the entire installation measured at the terminal box of the Corporation service mains between the earth and the extremities of the consumers' house mains (when all mains, wires, fittings, lamps, motors, and other appliances are fixed), should not be less than the number of ohms which shall be indicated by the number resulting from the division of 40,000,000, by the number expressing the maximum electric current in amperes, which has been named by the consumer in his application for his supply of electricity.

13. All motors should be mounted so that the beds are insulated from the foundations, and the holding down bolts from the beds, the insulation of the motors from earth, being in accordance with the above rule. Suitable regulating resistances should be provided for starting the motors.

20. Should any leak, or earth, or other serious defect, be discovered by the consumer within his installation, he should, without delay, give notice to the Borough Electrical Engineer.

THE CITY OF LONDON ELECTRIC LIGHTING COMPANY, LIMITED.

12. In all cases where special apparatus, motors, or arc lamps are intended to be used, the Engineer of the Company should be com-

municated with, and the type of motor, lamp, choking coil, &c., must in all cases be subject to his approval. . . .

13. All wiring must be tested by the Company before connection to the mains, and must not have a lower insulation resistance than at the rate of 75 megohms per point $\left(\frac{75}{\text{points}}\right)$, with all fittings and apparatus connected, and any wiring failing to comply with the above will not be supplied with current by the Company. . . .

The London Electric Supply Corporation, Limited.

Testing.—The Corporation retain the right of refusal to supply current to any installation, before testing the insulation of the circuits.

Installations not to have lower insulation resistance than the following :—

Up to	25 lamps	...	2	megohms.
,,	50 ,,	...	1·25	,,
,,	100 ,,	...	·75	,,
,,	150 ,,	...	·5	,,

Installations failing this test will not be supplied with the current by the Corporation.

The Corporation will not continue the supply of current to any installation falling below the above tests, and which is so allowed to remain by the consumer. . . .

All special apparatus, such as resistance and retarding coils, to be specially approved by the Corporation.

Newcastle and District Electric Lighting Company, Limited.

No special rules issued. The Rules of the Phœnix Fire Office in force.

The Yorkshire House to House Electricity Company, Limited.

11. Arc lamps, motors, and other apparatus. Any apparatus not used for ordinary lighting puposes, such as motors, regulating switches, resistances, retarding coils, heating apparatus, &c., must receive the special approval of the Company's Engineer. . . .

15. *Testing.*—The Company holds itself the right of refusing to connect any house before tests are made, and its Engineer satisfied with the wiring.

The Company reserves to itself the right to refuse to connect up to its mains installations which have not the following minimum insulation resistances :—

25 lamps	of any candle power	...	2	megohms.
50 ,,	,,	...	1·25	,,
100 ,,	,,	...	·75	,,
150 ,,	,,	...	·5	,,

The Company will not carry out any test until it has received a communication from the wiring contractor stating that the installation is quite complete, and making an appointment for the hour of test; and the contractors must send a representative at the time arranged to give any information that may be required.

THE OXFORD ELECTRIC COMPANY, LIMITED.

Testing and Inspection.—No current will be supplied to any installation until the Inspector of the Company has tested the insulation resistance and certified that the work is satisfactory.

The Company retains the right, without previous notice, to at any time cut off the supply to the whole or part of any installation if the insulation is defective, or the installation otherwise unsafe.

No current will under any circumstances whatever, be turned on to any installation until the whole of the work is entirely finished.

Ample notice should be given by the Contractor to the Company when an installation is ready for inspection and testing. All the switches should be turned on, and all fuses in position, but the lamps should not be placed in the holders.

In the event of any additional lights being turned on before they have been tested, and passed by an Inspector of the Company, the Company reserve the right to cut off the whole installation until such tests have been made. The insulation resistance over the whole installation must not be below the following :—

Installations of 25 lights	...	500,000 ohms.
,, 50 ,,	...	250,000 ,,
,, 100 ,,	...	125,000 ,,
,, 500 ,,	...	25,000 ,,
,, 1,000 ,,	...	12,500 ,,

Installations failing to bear this test will not be supplied with current by the Company.

The Company will not undertake to continue the supply of current to any installation falling below the above tests, and which is so allowed to remain by the consumer. . . .

Special Apparatus.—All special apparatus, such as resistance coils, motors, &c., to be specially approved by the Company.

THE WESTMINSTER ELECTRIC SUPPLY CORPORATION, LIMITED.

Testing.— . . . The Corporation will not supply current to any installation—(1) If there are any temporary wires or fittings; (2) If the whole of the work is not completed and all fittings fixed; (3) If when

testing with a pressure of 100 volts the insulation resistance is below the following standard :—

For	12 lamps	5	megohms.		For	150 lamps	0.75	megohms.
„	25 „	2·5	„		„	200 „	0·5	„
„	50 „	1·5	„		„	250 „	0·3	„
„	75 „	1·25	„		„	300 „	0·2	„
„	100 „	1·0	„					

Previous to a test being made by the Corporation's Inspector, every fuse in the whole installation must be inserted, all switches turned on, and the lamps removed ; a test for insulation between wires will then be made, and in no case must this insulation fall below 75,000 ohms.

Motors.—All motors must be mounted so that they are efficiently insulated from earth, the insulation test required will be in accordance with the above rule, and will be taken at the equivalent number of lamps as represented by the energy required by the motor. . . .

Leakage.—In the event of the insulation resistance of an installation falling below 5,000 ohms, after a supply has been turned on, and if the servants of the Corporation are not allowed to find or remedy the fault, the whole of the installation will be cut off until the fault has been put right. . . .

NOTE.—Since the presentation of this paper to the Institution the author has been favoured with a copy of the new rules issued by the Westminster Electric Supply Corporation, of which some notice is desirable as they differ materially from those in use at the time the paper was being compiled, and from which the data relating to the requirements of the Westminster Electric Supply Corporation was obtained.

The principal difference between the new and the old rules lies in the insulation tests. Under the old rules the insulation resistances exacted were set out directly for certain numbers of lamps, and the basis was very irregular. Under the new rule of testing, the insulation is obtained from a general equation, of a different character to any other referred to in the paper.

The insulation resistance per lamp required under the new testing rule is the highest with which the author is acquainted.

Extracts from the rules, and a summary, in the same tabular form as used in the paper, are appended.

THE WESTMINSTER ELECTRIC SUPPLY CORPORATION.
(Revised Rules.)

I.—*General Information.*

WIRING.—All the work upon consumer's premises must be carried out in accordance with the rules issued by the Fire Insurance Company under which the premises are insured, and the rules and regulations issued from time to time by the Corporation. Any special cases not provided for in these rules should be carried out in accordance

with the standard rules recommended by the Institution of Electrical Engineers.

INSPECTION.— . . . If, on such testing, the officer discovers a leakage from the consumer's wires exceeding one ten-thousandth part of the maximum supply current to the premises, . . . the Corporation shall forthwith discontinue the supply of energy to the premises in question. . . .

II.—*Rules.*

11. MOTORS.—All motors must be mounted so that their circuits are efficiently insulated from earth. Their insulation will be tested in accordance with the rule given below, each motor being taken at the number of lamps equivalent to the energy required by it. The starting switch and resistance should be so arranged that the first contact should not give more than 10 per cent. of the full working current, and that the increase of current should take place as gradually as possible. . . .

12. ARC LAMPS.—All arc lamps fixed out of doors must have a porcelain insulator of approved pattern between the lamp and the bracket or support of the lamp, even in cases where the framework of the lamp is not in connection with the circuit. Where the lamp is to be turned off and on intermittently, an approved resistance must be provided to reduce the rate at which current is thrown on or off.

13. TESTING.— . . . The test is made from the Corporation's cut-out, and consists of an insulation test between the wires, and between each wire and earth, when every branch fuse has been inserted, all switches turned on, and the lamps removed.

The Corporation will not supply current to any installation if, when testing with a pressure of 200 volts, the insulation resistance in megohms is less than the number obtained by dividing the pressure by the number of lamps. In no case must the insulation fall below 75,000 ohms.

In installations where there are any temporary wires or fittings, or where the work is incomplete, the Corporation must be duly informed, and they will not connect unless they are satisfied as to the precautions taken.

After the test the lamps must all be inserted, and they will be inspected to see that every lamp lights properly.

Subsequent additions to the lights must be tested in the same manner as the original lights.

APPENDIX II.—INSULATION TABLES.

Authority	Insulation Resistance Complete Installation.		Pressure.		Motors.
	To Earth.	Between Mains.	Testing.	Working.	Insulation required.
Board of Trade.	$\frac{10{,}000\ E}{A}$	$\frac{10{,}000\ E}{A}$	—	—	—
Institution of Electrical Engineers.	$\frac{10\ \text{megohms}}{A}$	Not specified	Double that of working	—	Not specified. Follows from ins. res. test
Phœnix Fire Insurance Co. — For continuous currents.	Lights. Ohms. 12 1,000,000 25 500,000 50 250,000 100 125,000 500 25,000 1,000 12,500	The same as specified to earth	Not less than 100 volts.	Maximum of 230 volts	Not specified
For alternating currents.	Double the above.				
Liverpool & London & Globe Insurance Co.	$\frac{40{,}000\ E}{A}$	$\frac{40{,}000\ E}{A}$	Double that of working	Maximum of 250 volts	Not specified. Follows from ins. res. test
Aberdeen.	33 Watt. lamps. Megohms. 12 5·00 25 2·50 50 1·50 75 1·25 100 1·00 150 ·75 200 ·50 250 ·30 300 ·20	Not less than one third of insulations to earth	220 volts	110 volts and 220 volts	To be equal to the ins. res. required for the number of 33 Watt lamps taking same energy
Blackpool.	$\frac{20\ \text{megohms}}{N}$	Not specified	Not specified	200 volts	$\frac{20\ \text{megohms}}{10}$

Motors. Mounting.	Arc Lamps. Insulation per lamp required.	Minimum Insulation after Supply has been Commenced.	Range of Specification.	Ins. Resistance per 64-Watt Lamp.	Insulation Resistances of Conductors Megohm-Miles.
—	—	—	No limit	100 v. 1·56 meg. 200 v. 6·25 meg.	—
Earthed for 250 volts and upwards	Not specified. Follows from ins. res. test	Not directly specified	No limit	100 v. 15·6 meg. 200 v. 31·25 meg.	Class A, 1,200 to 300 megohms. Class B, 300 megohms (at 60° Fahr. after 24 hours' immersion)
To be insulated	Not specified	Not specified	1,000 lamps	12·5 megohms per lamp for voltages up to 230 25 megohms per lamp for voltages up to 230	400 megohms for dry places; 1,000 megohms for damp places (at 60° Fahr. after 24 hours' immersion)
To be insulated	Not specified. Follows from ins. res. test	$\frac{40{,}000\ E}{A}$	No limit	100 v. 6·25 meg. 200 v. 25·0 meg.	300 megohms (at 90° Fahr.)
To be insulated	Not specified	5,000 ohms	300 33 Watt lamps or equivalent	Varies between 60 & 112 meg. per 33 Watt lamp according to number of lamps installed	In dry places, 300 megohms; in damp places, 600 megohms; in plaster and walls, 1,000 megohms
Not specified	$\frac{20 \text{ megohms}}{10}$	$\frac{20 \text{ megohms}}{N}$	No limit	20 megohms per lamp	Rubber-covered cables, 2,500 meg. after 24 hours' immersion. Testing press, 400 v. Other cables tested with alt. press of 800 v.

AUTHORITY.	INSULATION RESISTANCE COMPLETE INSTALLATION.		PRESSURE.		MOTORS.
	To Earth.	Between Mains.	Testing.	Working.	Insulation required.
BRADFORD.	10,000,000 / A	Not specified	Not specified	230 volts	Specified as 1 megohm. Follows also from ins. res. test
BRIGHTON.	20 megohms / N	Method of making test not definite	Not specified	115 volts and 230 volts	20 megohms / 10
BRISTOL.	Lamps. — Ohms. 10 — 1,500,000 20 — 1,000,000 30 — 700,000 50 — 500,000 100 — 250,000 150 — 200,000 200 — 150,000 250 — 100,000 300 — 75,000	Method of making test not definite	100 volts	105 volts and 210 volts	Not specified
EDINBURGH.	Lamps. — Meg. 12 — 5 25 — 2·5 50 — 1·5 75 — 1·25 100 — 1·0 150 — ·75 200 — ·5 250 — ·3 300 — ·2	In no case less than 75,000 ohms	230 volts	115 volts alt. 230 volts continuous	To be equal to the ins. res. required for the number of lamps taking same energy
GLASGOW.	60 megohms / N	Not specified	200 volts	200 volts	1 megohm
HAMPSTEAD VESTRY.	75 megohms / N	Method of making test not definite	105 volts	105 volts	Special arr
HUDDERSFIELD.	No rules issued.	Phœnix Fire Office Rules in force			
HULL.	40,000,000 / A	In no case less than 75,000 ohms	200 volts	110 volts and 220 volts	Not specified. Follows from ins. res. test

Motors. Mounting.	Arc Lamps. Insulation per lamp required.	Minimum Insulation after Supply has been Commenced.	Range of Specification.	Ins. Resistance per 64-Watt Lamp.	Insulation Resistances of Conductors Megohm-Miles.
Not specified	Not specified. Follows from ins. res. test	$\frac{10,000,000}{A}$	No limit	230 v. 35·6 meg	In dry places, 300 megohms; damp places, 600 meg.; plaster work, &c., 1,000 megohms
Not specified	$\frac{\text{20 megohms}}{10}$	$\frac{\text{20 megohms}}{N}$	No limit	20 megohms	Not specified. Reference to Fire Office Rules
Not specified	Not specified	10,000 ohms.	300 lamps	Varies between 15 and 30 megohms per lamp, according to number of lamps installed	Not specified. Work and materials to be to Phœnix Fire Office Rules
To be insulated ordinarily. 460 v. motor frames and shafts earthed	Not specified	10,000 ohms.	300 lamps	Varies between 60 and 112 meg. per lamp, according to number of lamps installed	In dry places, 300 megohms; in damp places, 600 meg.; in plaster work and walls, 1,000 meg.
To be insulated	5 megohms	Corresponding to $\frac{A}{10,000}$	No limit	60 megohms	Consumer's underground service mains, 2,500 meg. Reference to Phœnix Fire Office Rules
angements with engineer		500 ohms.	No limit	75 megohms	Not less than 600 megohms
To be insulated	Not specified. Follows from ins. res. test	Corresponding to $\frac{A}{10,000}$	No limit	110 v. 69 meg. 220 v. 138 meg.	Never less than 600 megohms

AUTHORITY.	INSULATION RESISTANCE COMPLETE INSTALLATION.		PRESSURE.		MOTORS.
	To Earth.	Between Mains.	Testing.	Working.	Insulation required.
VESTRY OF ST. MARY, ISLINGTON, EXTENSION AREA.	100 lamp-meg. for installations of more than 10 lamps. Minimum for 10 lamps or fewer, 5 meg.	The same as specified to earth	Not specified	230 volts	Special rules are issued
YORKSHIRE HOUSE TO HOUSE CO., LEEDS.	Lamps. Meg. 25 2 50 1·25 100 ·75 150 ·5	Method of making test not definite	Not specified	100 volts and 200 volts	To
LIVERPOOL.	$\frac{20,000,000}{A}$	$\frac{20,000,000}{A}$	200 volts	230 volts	Follows from ins. res. test, but not less than 50,000 ohms is specified
MANCHESTER.	Mc Lamps. Ohms. 25 2,000,000 50 1,000,000 75 666,000 100 500,000 200 250,000 300 167,000 400 125,000	Method of making test not definite	Not specified	100, 200, 300, and 400 volts	1 megohm
NEWCASTLE & DISTRICT.	No rules issued.	Phœnix Fire Office Rules in force			
NOTTINGHAM.	Lamps. Meg. 10 7·5 20 3·75 30 2·5 40 1·9 50 1·5 75 1 100 ·75 150 ·5 200 3·75 250 ·3 300 ·25	Method of making test not definite	100 volts	100 volts and 200 volts	To be equal to the ins. res. required for the number of lamps requiring the same energy.
OXFORD ELECTRIC CO.	Lights. Ohms. 25 500,000 50 250,000 100 125,000 500 25,000 1,000 12,500	Method of making test not definite	Not specified	100 volts	To be

Motors. Mounting.	Arc Lamps. Insulation per lamp required.	Minimum Insulation after Supply has been Commenced.	Range of Specification.	Ins. Resistance per 64-Watt Lamp.	Insulation Resistances of Conductors Megohm-Miles.
Special rules are issued		100 lamp-meg. for installations of more than 10 lamps. Minimum for 10 lamps or fewer, 5 megohms	No limit	100 megohms for installations of more than 10 lamps. From 50 to 5 megohms for 10 lamps or fewer	Not less than 600 megohms
Engineer's approval		Not specified	150 lamps	Varies between 50 and 75 meg. per lamp	Rubber cables. In dry places, 600 meg.; in damp places, 1,000 meg. Lead-covered cables to engineer's approval
To be insulated	Not specified. Follows from ins. res. test	"Limit of safety." Not specified	No limit	230 v. 72 meg.	Not less than 600 megohms
Frame, &c., earthed. Earthing connection of equal size to motor leads	Per circuit, 5 megohms (for 15 amp. lamps)	Corresponding to $\frac{A}{10,000}$	No limit	50 megohms per lamp up to 50 C. P.	Exterior wiring, 2,500 meg.; other conductors in no case less than 600 meg. Rubber-covered cables. Paper or bitumen insulated cables and lead covered, 250 meg.
To be insulated	Not specified	"Becomes defective." Not otherwise specified	300 lamps	75 megohms	Not less than 300 megohms
approved by the Compy.		Lights. Ohms. 25 — 500,000 50 — 250,000 100 — 125,000 500 — 25,000 1,000 — 12,500	1,000 lights	12·5 megohms	Not specified. Conductors to have "one coating pure rubber and one of vulc. rubber of approved thickness"

AUTHORITY.	INSULATION RESISTANCE COMPLETE INSTALLATION.		PRESSURE.		MOTORS.
	To Earth.	Between Mains.	Testing.	Working.	Insulation required.
VESTRY OF ST. PANCRAS.	10,000,000 / A	Not specified	Not specified	220 volts	Follows from ins. res. test. Not otherwise specified
SOUTH SHIELDS.	50 megohms / N	100 megohms / N	Not specified	110 volts and 220 volts	To
WOLVERHAMPTON.	40,000,000 / A	Not specified	Not specified	110 volts and 220 volts	Follows from ins. res. test, and is so specified
SUNDERLAND.	Lamps. — Meg. Up to 12 — 2·5 12 to 25 — 1·25 25 to 50 — 1·00 50 to 75 — 0·75 75 to 100 — 0·5 100 to 150 — 0·3 150 to 200 — 0·25 200 to 250 — 0·2 250 to 300 — 0·15	In no case less than 75,000 ohms.	200 volts	110 volts and 220 volts	Not specified
THE LONDON ELECTRIC SUPPLY CORPORATION.	Lamps. — Meg. 25 — 2 50 — 1·25 100 — ·75 150 — ·5	Method of[1] making test not definite	Not specified	—	Not specified
THE CITY OF LONDON ELECTRIC LIGHTING CO.	75 megohms / Points	Method of making test not definite	Not specified	100 volts and 200 volts	To
WESTMINSTER ELECTRIC SUPPLY CORPORATION.	Lamps. — Meg. 12 — 5 25 — 2·5 50 — 1·5 75 — 1·25 100 — 1·0 150 — 0·75 200 — 0·5 250 — 0·3 300 — 0·2	In no case less than 75,000 ohms.	100 volts	100 volts and 200 volts	To be equal to the ins. res. required for the number of lamps taking the same energy
Ditto (Revised Rules.)	E / N — Minimum for any installation, 75,000 ohms.	E / N	200 volts	200 volts	Ditto

[1] No limit beyond that imposed by minimum of 75,000

Motors. Mounting.	Arc Lamps. Insulation per lamp required.	Minimum Insulation after Supply has been Commenced.	Range of Specification.	Ins. Resistance per 64-Watt Lamp.	Insulatio Resistances Conductor Megohm-Mi
Not specified	Follows from ins. res. test. Not otherwise specified	$\frac{10,000,000}{A}$	No limit	220 v. 34·5 meg.	Not less thar megohms
Engineer's approval		"Defective, or otherwise unsafe." Not otherwise specified	No limit	50 megohms to earth	In dry place megohms ; ir places, 600 Buried or ir sible cond 2,000 megohi
To be insulated	Follows from ins. res. test. Not otherwise specified	Not specified	No limit	110 v. 69 meg. 220 v. 138 meg.	Not specified. ductors to ha coating pure and one of rubber of su thickness
Not specified	Not specified	5,000 ohms.	300 lamps	Varies between 30 and 50 meg., according to the number of lamps installed	Not less than megohms
Not specified	Not specified	Lamps. / Meg.: 25 / 2; 50 / 1·25; 100 / ·75; 150 / ·5	150 lamps	Varies between 50 and 75 meg., according to the number of lamps installed	Not specified. ductors to hav coating pure and one vulc. of approved ness"
Engineer's approval		Not specified	-	—	Not specifie
To be insulated	Not specified	5,000 ohms.	300 lamps	Varies from 60 to 112 megohms, according to the number of lamps installed	In dry place megohms ; ir places, 600 In plaster wo walls, 1,000 n
To be insulated	Not specified	Corresponding to $\frac{A}{10,000}$	See Footnote[1]	200 megohms	At least 600 me places liable to or where wi in or pass walls, 1,000

ohms, which corresponds to an installation of 2,666 lamps.

THE REGULATION OF WIRING RULES.

By C. H. WORDINGHAM, Member.

THE question of the conditions necessary to be attained in order to provide for safety from fire and reliability in the utilisation of electrical energy for various purposes has attracted a very large amount of attention from the earliest days of the industry, and numerous sets of rules and regulations have been promulgated from time to time. Many different authorities have taken upon themselves to issue these rules, and, inasmuch as there are various conflicting interests in an installation of electric light or power, and as each authority is determined to abide by its own particular rules, the difficulties of the wiring contractor are many and great. In consequence of this, and for other reasons, there is, at the present time, a very widely diffused feeling that there should be some definite standard set of rules by which every one will agree to be bound.

In considering this matter it is important to recognise that there is an essential difference between the rules necessary to secure efficient and safe wiring, and those, equally necessary, to secure a satisfactory utilisation of the public supply of electrical energy. The former will, practically speaking, be independent of the system of supply. The latter will vary greatly with the nature of the distribution adopted by the supply company, and also to some extent with the local conditions existing in the town in which the supply is given. It does not appear to the author that this fact has been sufficiently generally recognised. It would appear to him quite possible, and, undoubtedly, most desirable, to draw up a set of regulations for wiring and fittings which should be of universal application; but so far as the public supply authorities' regulations are concerned, it will be necessary either to draw up a single model set of regulations which can be modified for every particular case, or several standard sets of regulations to suit different systems of supply which might possibly meet with general acceptance. In all probability, however, the former would be the more likely to meet with success.

It is important to consider, when framing regulations, what power exists to enforce them. This point was fully discussed by the author in a paper read at the 1896 Convention of the Municipal Electrical Association.[1] It was then shown that the power given in the Electric Lighting Acts is extremely unsatisfactory, and does not by any means give the undertakers a sufficiently strong hand to deal effectively with wiring contractors, and the course adopted by Glasgow was advocated by the author for imitation by other towns. Since then the same powers have been sought and obtained by Manchester, and there should be no difficulty in similar powers being obtained in all cases where municipal authorities are the undertakers. The course referred to is to obtain a special Act to make the regulations in question. In the case of Glasgow this formed part of the Glasgow Buildings Regulations Act, 1892. In the case of Manchester it formed part of the Omnibus Act known as the Manchester Corporation Act, 1897, Section 40.

It might be somewhat difficult for existing companies to obtain similar powers, but the experiment would be well worth trying.

To consider first the question of wiring regulations properly so called. There are several excellent sets of rules in every-day use, but the author ventures to think that those issued by this Institution are the best that have yet been produced. They are the outcome of the most careful deliberation, and no one rule was drawn up until it had been most thoroughly discussed by men actually engaged in the work affected. Indeed, it may be said without any hesitation that these rules represent the unbiased opinion of electrical engineers at the present time as to the best means of securing safety from fire and efficient utilisation of energy.

It is greatly to be regretted, however, that no matter how good the rules may be, they, in the existing state of things, can only be suggestions, and when individuals have drawn up other regulations which they have found to work well in practice, and with the details of which they are thoroughly acquainted, they are extremely loth to adopt "suggestions."

[1] See Proceedings Municipal Electrical Association, p. 30: "The control by Municipal Authorities of consuming devices, and the wiring connecting them to mains."

The only possible way in which uniformity can be attained is by first of all thoroughly thrashing out the whole question at one or more meetings, in which all the parties interested are adequately represented. When this has been done it should be possible finally to decide upon a set of rules which will cover the whole of the points raised in the discussion, and then, if every person will abide loyally by the decision arrived at, the desired result will be attained; but it is futile to hope for this unless persons will sink their own individual preferences, and it may be their own *amour propre*.

It is not the author's object in this paper to enter into the question of particular regulations. He had the honour of forming one of the Committee that drew up the Institution rules, and he might fairly say that these rules are, in his opinion, as good as can be drawn up at the present time; but a few words may not be out of place on the general question of enforcing rules when made and agreed upon.

There is, in the author's opinion, great necessity at the present time for the proper supervision of fittings used in connection with electric lighting and motive power. By fittings it is intended to imply such apparatus as fuses, switches, ceiling roses, wall sockets, lampholders, &c., not ornamental electroliers, brackets, &c., which are after all only metallic casing for wiring. Quite recently the author has introduced into Manchester a system of testing and registration of such fittings. It has only been in operation for a few weeks, but the results are eminently satisfactory. It is found that all makers of repute welcome the action taken, that they are anxious to submit samples, and that they see the value of the certificates and the advantages that will accrue to them by the official recognition of their wares. At the same time, while the manufacturers are benefited, wiring contractors derive great convenience from being no longer in doubt as to what fittings will or will not pass inspection, and further, consumers have the satisfaction of knowing that the apparatus fixed on their premises has been certified by an unbiased authority.

It has long been the practice to test and stamp fittings intended to be used in connection with the supply of water, but, so far as the author is aware, no attempt has hitherto

been made to apply this system to electrical fittings. The treatment necessary in the two cases differs materially, for whereas in an ordinary house but few taps are required in connection with the water supply, the same house would require a large number of electrical fittings, hence it is not possible to attempt actually to test and certify every individual fitting. The course adopted by the author, therefore, is to require manufacturers to submit in duplicate samples of the fittings they propose to supply in Manchester. One of the duplicate samples is then subjected to certain tests, and if it fail to pass, the two samples are returned to the manufacturer; if, on the contrary, it conform to the regulations, the second sample is preserved[1] and kept for reference, so that it may be seen by any contractor or consumer, and a certificate is issued stating that the article in question has been tested and found to conform to the regulations, and may be used. This certificate remains in force for one year, but may be renewed from year to year without further tests. It would be tedious to enter into all the regulations affecting this registration, but members desirous of further information will find a copy of the rules in the Institution library, and the author will be happy to forward a copy to any member desiring to have it.

The author was led to take the step in question very largely in consequence of the extreme difficulty, amounting almost to an impossibility, which exists in defining the conditions necessary in such articles as switches and fuses to ensure safety. This fact was very strongly impressed upon the members of the Institution Committee when drawing up the wiring rules, and at the author's suggestion it was thought that instead of specifying a given length of break, height of cover, &c., it would be far preferable to specify certain conditions which a given fitting should fulfil.

In Manchester the tests specified for a fuse are that when duly fitted up with its cover in position and connected to mains giving the pressure for which it is intended, it shall break the circuit when those mains are short circuited through it with a fuse wire corresponding to its rated

[1] The Author now makes a practice of returning to the manufacturer the sample on which the tests were made, if it be at all costly, thus retaining only one of the duplicates submitted.

capacity placed between its contacts. In carrying out these tests the fuse is tested in three positions, namely: (1) with the base horizontal; (2) with the base in a vertical plane, the fuse wire being horizontal; (3) with the base in a vertical plane, the fuse wire being vertical.

For switches the conditions specified are that a switch when used in the ordinary manner shall be capable of breaking a current 50 per cent. in excess of that for which it is rated at a pressure 50 per cent. in excess of that at which it is intended to work. In addition to these tests there are other tests to insure that the parts are properly proportioned to prevent heating.

It may seem that the tests set forth are unduly severe. As a matter of fact the better class of fittings are found to conform to them with ease, and therefore on this score there can be no objection to the tests.

It may be said that so far the subject has been considered chiefly from the Central Station point of view and that reference has not been made to consulting engineers. Undoubtedly they are in a different position, but, on the other hand, they are in a much stronger position than undertakers not possessing special powers, because the contractor working under their supervision is working to a definite specification under which large powers are given to the engineer. In the author's opinion it would be a considerable boon to consulting engineers, and would tend greatly to uniformity in practice if some central authority —which, seeing that the Board of Trade is probably out of the question, should certainly be this Institution—were to undertake the testing of fittings in a somewhat similar manner to that adopted for local work by the author. It should not be a matter of extreme difficulty to establish a small testing station for the purpose, and it could readily be made self-supporting, for manufacturers would doubtless be prepared to pay a substantial fee for the testing and registration of their fittings if the certificates issued carried the weight of the Institution behind them, for they would greatly enhance the value of their wares. Were such a body as the Institution to undertake this work, consulting engineers would feel every confidence in specifying certified articles, and they would feel assured that their clients would obtain satisfactory apparatus.

The author is aware that fittings are not the only things demanding attention. There is the question of the best kind of insulation to employ for the conductors and the best kind of casing, but these matters are fully dealt with in the Institution rules, which are so drawn as to admit every existing system the efficiency of which has been demonstrated in practical work. We may therefore leave the subject of wiring regulations, and pass on to that of regulations made by undertakers for the efficient supply of energy to the installations of consumers connected to their mains.

Systems of distribution may be divided broadly into two-wire and multiple wire, each class being further subdivided into continuous current and alternating.

The most widely employed system of distribution at the present time is the multiple wire, and perhaps the more important subdivision of this is that in which continuous current is distributed, for there can be little doubt that the use of this will largely extend in the near future. Now, for multiple wire distribution, it is essential that the balancing of the circuits should be good, and for this reason it is most important that contractors should be required so to split up their circuits that they may be evenly divided between the various pairs of mains. For practical purposes it may be said that the three-wire system will be the most widely used, and therefore contractors should be required to divide their installations into two approximately equal circuits when a certain demand is exceeded. In Manchester this limit has been fixed at a low figure, namely, 12½ kilowatts, but the author ventures to think that the results attained by proper balancing entirely justify the slight inconvenience which may be said to be experienced in consequence by the contractor. As a matter of fact, this inconvenience with modern good wiring is not great, for it has become the practice to effect the subdivision in any case, and to limit greatly the number of lamps on a single circuit. Therefore in practice the only hardship is the provision of two small main switches and fuses instead of one large one. Again, in order to secure efficient balancing it is important that, as far as possible, all motors should be supplied from the two outer conductors. For this reason the author has required in Manchester that all motors exceeding 2½ kilowatts shall

be wound for 400 volts. In practice there is no great hardship in this, and the result to the Central Station engineer is that he is practically supplying all his motive power on the two-wire system at 400 volts. Consequently, so far as motors are concerned, he has no trouble as to balancing.

In every system of distribution, whether two-wire or multiple wire, it is important to guard against sudden rushes of current, and therefore it is important to draw up rules with regard to the gradual starting of motors. This involves special switch-gear and interlocking arrangements between switches, but the difficulties are such as can be readily overcome. In the case of large photographic or stage arc lamps it is necessary to take similar precautions, and, of course, in the case also of charging batteries or working electric furnaces.

Another important point which will require different treatment according to the system of supply, is in connection with the earthing of one of the conductors. In the case of a continuous current supply on the multiple wire system of distribution, it does not appear to the author desirable to allow any portion of the consumers wiring to be directly connected to earth whether the middle conductor of the system be insulated or not. There is much to be said in favour of the middle conductor being earthed at the central station and something against it, but the reasons against earthing one conductor of the consumer's installation are extremely cogent, the most important being that, although on the whole the balancing on the network may be extremely good, there must of necessity in some streets be a considerable local want of balancing causing heavy currents to flow through the middle wire. If, then, there be two consumers separated by a few hundred yards, both having one conductor earthed, and if there be a heavy current flowing down the middle wire and causing a fall of potential of several volts along that wire, there will then be currents set up in the earth from the earthed conductor of one consumer to that of the other, and the result may be serious electrolytic troubles, worse in fact than might arise from an improperly bonded tramway return. This reason alone appears sufficient to the author to preclude a system of wiring which requires the earthing of one conductor in a consumer's installation. The same objection does not apply

with equal force if the distribution be effected by means of alternating currents, though in this case trouble may be anticipated if an earth return be used on the local telephone service. The author desires it to be understood that it is on these grounds alone that he deprecates an earthed return system of wiring; for isolated installations there is much to recommend it, and also in cases in which the system of supply is a high pressure one and each consumer is supplied from his own alternating current transformer.

From what has been said it will be seen that the regulations comprised in this second group are of a totally different nature from wiring regulations proper, and the contention that they cannot be made absolutely uniform will probably be thought justified. The matter is one that affects chiefly central station engineers, though, of course, the views of contractors, and particularly of manufacturers, should be given due weight to. It would be most useful if some general consensus of opinion as to the best rules for given systems of distribution could be obtained. The matter has received the attention of the Municipal Electrical Association for some time past, and a committee of their Council is at the present time drawing up a model set of rules for the use of municipal stations. It is greatly to be desired, however, that a general set of rules acceptable also to the engineers to companies should be decided upon.

THE INSTITUTION WIRING RULES.

By R. E. CROMPTON, Past President.

NEARLY four years have elapsed since the Council decided that the Institution wiring rules should be revised. A Committee was then appointed to deal with the matter, and subsequently sat throughout the greater portion of two sessions, with the result that the revised rules were issued in July, 1897.

I have discussed these rules with several gentlemen who are competent to express an opinion on the matter, and I find the general feeling to be that the labours of our Com-

mittee were not misspent, and that the rules in their present form are, of all those now in existence, the best fitted to become the standard set accepted by all parties.

My object, therefore, in writing this note is to try and promote the adoption of the Institution rules as the standard rules to be prescribed by Consumers, Insurance Companies, and Municipal Authorities, and to be worked to by Wiring Contractors. Of these four classes, the Fire Insurance Companies are of course the most interested, as they act as guarantors of the safety of installations passed by them ; and it is to the gentlemen who act as Inspectors and Technical Advisers to the Insurance Companies that I primarily address myself. The objects which the Committee had in view when framing the present rules were identical with those of the Fire Insurance Companies, every principle laid down and every detail described having for its object the avoidance of fire risk. I say this advisedly, for although some few details were intended to prevent either danger to person from shock, or discontinuity of supply, yet even in guarding against these dangers we are indirectly guarding against fire risk also. At present there are issued by Fire Insurance Companies several distinct sets of rules, differing among themselves, indirectly in principle, but, mainly, in the size and carrying capacity allowed for the conductors and in small matters of dimension. In these respects I believe the Institution rules occupy a midway position—that is to say, they are more stringent than some and less so than others ; but all are dealing with the same questions of electrical supply and with the same fire risks. Why, then, should this diversity in rules continue ? Is not this the time for us to invite Insurance Inspectors to appoint a Committee to decide in what points the Institution rules can be improved, made more (or less) stringent, or added to, so that they may be satisfactory to all, and may thus establish that uniformity, which will undoubtedly result in simplifying the work of contractors, and which therefore, assuredly, in the end, will give the consumer better value for his money? The Committee throughout tried to place themselves in the position of Fire Insurance Inspectors, and if I recollect aright, the rules in their draft condition were sent out to a number of Inspectors for their criticisms and remarks ; and all criticisms received were accepted and dealt with as far

as was possible. If it is a fact that these rules differ in points which Fire Insurance Inspectors consider vital, now is the time for these gentlemen to come forward and give us the opportunity of amending or supplementing them; but I sincerely hope that this discussion will open the eyes of every one to the desirability of uniformity, and to the great waste of time, and the annoyance to consumers and every one concerned, that arise from the multiplicity of rules which at present exists. It has been said that the Institution rules cannot be enforced. It is true that the Institution as a body cannot enforce its rules, but in drawing them up it did the next best thing—it appointed, from amongst those of its members who were best qualified to deal with this matter a Committee to draw up a set of rules which would be a guide to any consumer, and which could be adopted and enforced by any one having the requisite authority. In fact, all that is now required is, that wherever the words "should be" occur in the rules, the words "must be" should be substituted.

The CHAIRMAN: Perhaps Mr. Crompton would kindly open the discussion. The Chairman.

Mr. R. E. CROMPTON: I only rise to appeal to those present this evening to consider the main question before them to be that of the desirability of having one uniform set of rules for wiring for the entire electrical industry, with only such modifications as are necessary to suit local conditions and particular risks. Mr. Crompton.

The Committee that sat for a very long period to draw up the Institution rules was very carefully selected from gentlemen who had had the widest and the longest experience of the problems connected with electrical supply, and who were then in close touch with the work, and knew all that was going on, and were conversant with the difficulties that surround the electrical wiring industry. The rules were many times revised, and many engineers outside the Committee of the Institution were consulted during their preparation. I do not say for one moment that the Institution rules are perfect, only that in my humble opinion they are not very imperfect. It may be said that, having been so closely connected with their preparation, I look upon them with too favourable an eye; but I cannot see now that any of the large number of rules which are in force at the present time are equal to them, or are so comprehensive, or deal with so many of the difficulties as our rules do. I therefore beg that those who take part in this discussion will keep this fact before them and, by calling attention to points in our rules which they may consider insufficient to protect the interests they represent, help us to amend or add to our rules so as to meet their standard of perfection. If by

Mr. Crompton. this discussion we achieve this result, it will be extremely useful to our industry; for then the fire insurance companies, the consulting engineers, the municipal engineers, and all those who have power to enforce rules, would only have to specify the Institution rules, the word "must" being substituted where now the word "should" is employed, and add as an appendix to those rules the additions which they require to suit their particular risks or their particular local conditions.

I think that is all I have to say at the present time. No doubt in the course of the discussion certain rules will be attacked and questions will be asked, all of which I shall hope to take notes of, and in my reply satisfactorily to dispose of. In conclusion, I sincerely hope that we shall have an expression of the views of the fire insurance inspectors on this most important question.

Mr. Wordingham. Mr. C. H. WORDINGHAM: What little I have to say is contained in the very brief paper that you have before you. It seems to me that it is a matter of pressing importance to arrive at some definite set of regulations that shall be acceptable to all parties. We all, no doubt, feel that this is a more or less hackneyed subject, but it is none the less important on that account; and in practical, every-day electrical engineering it is a very important one indeed. No one feels it more strongly probably than contractors, because they often have to deal with two, three, or even four different authorities, each authority having its own regulations, and the regulations in these different sets often clash. It is therefore to the interest of contractors to have a definite set of regulations drawn up; and central station engineers also are equally interested in having uniform rules. I have tried to show in my paper that there is a distinction between ordinary wiring rules and rules in connection with supply, and I do not propose to weary you by covering the same ground again.

I do earnestly hope that the discussion arising from these papers may lead to some practical result. It is no use talking about a thing and saying it is very desirable, if the people who talk are not really determined to do, each one, his best to arrive at the result he advocates. We must all be prepared to give up something if we are to secure uniformity, and although it is rather mortifying, perhaps, to give up a pet regulation, I think it behoves everybody concerned to do so, if in this way a uniform standard can be arrived at.

Mr. Drake. Mr. B. DRAKE: Having been associated with Mr. Crompton in the editing of the Institution Wiring Rules, I naturally take an interest in them; and, as a contractor, am disappointed to see to what a small extent they have been generally adopted in practice. The chief cause of this, as far as I can judge, is opposition offered by the insurance companies. In the different specifications which are issued, one generally finds that the insurance companies have the last word, and that unfortunately they have not looked upon the Institution Rules as favourably as might have been hoped. There is, in the case of the insurance companies, a considerable personal element, and it appears to me that they take a pride in having rules of their own different from those of other people. As long as this feeling exists, their argument

must hold good, namely, that as they have to pay the piper, they may reserve to themselves the right to call the tune. There is some truth in this; but if the Institution has not up to the present framed rules which commend themselves to every one, then the sooner suggestions are put forward in a practical way and the rules drafted afresh the better for us all. Mr. Drake.

We all appreciate the impossibility of carrying out work to the specifications which are now issued. Take one simple case, that of a building that is under two or three insurance companies. The contractor has to undertake that he will satisfy all the insurance companies affected. He turns to one set of rules, and finds that every ceiling-rose shall contain a fuse; he turns to the next, and is warned that no ceiling-rose may contain a fuse! Yet he is bound to comply with both, and is left to his ingenuity to find the means of doing so. Look, too, at the stock which the contractor has to keep. He must have patterns of all kinds which will comply with the different rules, and they must be, as far as possible, issued to the different installations according to the bulk of the insurance connected with that particular work. It is impossible to issue patterns which will comply with them all, and one has to comply with the rules of the leading insurance company, and to disregard the others. Then the cost of these materials is greatly increased, for if ceiling-roses have to be made in half-a-dozen patterns, they cannot be made at the same price as if they are made in one. Concerted action on the part of all the contractors appears to be the only cure for this. If they would waive personal feeling, and would all, even at the risk of losing work, agree to back up the Institution, and carry out the work in accordance with the Institution rules alone, those rules would become the standard, and the insurance companies would accept them.

The curves given by Mr. Pigg exhibit the very wide variations that exist with regard to insulation requirements. Now, I remember the report being current that Sir Joseph Whitworth's standard for screws had been fixed, by taking the mean of screws submitted as being then in use by all the leading makers, without any special reference to which was better or worse; and it appears to me that if a mean could be taken of the different insulation requirements now existing, that that would probably meet the case sufficiently well for all practical purposes.

Mr. Wordingham's suggestion that the Institution should undertake the testing and certification of apparatus is, I think, a very excellent one; but I should suggest that this certificate relate only to the actual results obtained, and that the manufacturer should be allowed to see the tests made with his apparatus. The responsibility of saying whether apparatus is, in his opinion, sufficiently good to be made or not, is too great to be accepted by any inspector who may hold office for the time being; moreover, it would manifestly be necessary that the inspector should be absolutely disinterested, and that he should be a man above suspicion in every way. But if he simply certified that this apparatus acted in a certain way (*e.g.*, that the fuses worked under certain conditions with two hundred volts and so on), I think a very

Mr. Drake. admirable adjunct to the profession would be given us. We all, at times, have to design apparatus, which it is difficult to test until we actually get it to work; and if we could take it to some place, and obtain a certificate as to its performance under different conditions of voltage or current, etc., it would certainly be well worth the fee charged for the test.

Mr. Howard. Mr. A. H. Howard: Mr. Crompton, in his paper, has, naturally, confined himself mainly to the general reasons why the Institution rules should be adopted. I should like to call attention to the rule which relates to the sizes of wires and cables to be used with particular currents, and to explain why it was put in its present form.

The formula in question, which is given on the last page of the Wiring Rules, was not deduced by rule of thumb, as I saw stated in one of the electrical papers, but was the result of careful investigation and experiment. In the first place, the curves of the temperature were plotted out, showing the rise due to particular currents on the different sizes of wire when placed in wooden casings, and it was decided by the committee that 20° F. might be regarded as a safe rise. Then, the currents adopted in France and Germany were compared in respect of the rise of temperature produced by them, and they (especially the German) were found to correspond very closely with the 20° rise, preliminarily agreed upon. Finally, a formula was found which would fit in with that, and this formula was prescribed in the rules. There are several reasons why it should be adopted, but the chief is that it is to the advantage of almost all the members of the Institution to use it. It is to the interest of the wiring contractor, because, up to a fairly large cable, he is allowed to use a smaller wire for a small current than he was before, the current allowed in this case being much greater than by the old rule of a thousand amperes per square inch. It is, then, to the advantage of the consumer, because ultimately he will get his wiring cheaper, as the competition between the contractors brings down the price. In fact, it seems to be to the advantage of nearly all the members of the profession, with the exception of the cable manufacturers, who do not, perhaps, get much pity from the general body of the Institution.

General Webber. General C. E. Webber: It is not without interest that the older members of this Institution find themselves told by Mr. Pigg that the recognition of the improvement in the performance of good work in the wiring of premises has been recent. I may fairly say that all of us desire uniformity in principles, and if our members had all along tried to adopt those laid down in the Institution rules in their own specifications, there would have been, ere this, fewer differences than those described by Mr. Pigg.

Mr. Wordingham's little book (the perusal of which I commend to all our members), published for the use of the consumers of the Manchester Corporation Supply, has, as he tells us, included under one cover much matter in addition to the regulations against risk of danger to property and person; and I would ask him to tell us whether, from that book, he desires to extract only those data which especially apply to wiring, or whether he means that the book itself—which I may

say is admirably got up and is better indexed for a book of thirty pages than any book I have ever seen—is to be the type of the single model that he advocates for universal application. General Webber.

I cannot but agree with Mr. Crompton and with Mr. Drake that our members have not given sufficient attention to the Institution rules published in 1897. Some perhaps regard them as the result only of the work of the previous year. As a matter of fact, in 1882 the Council of this Institution appointed a committee to report on the subject; and in 1883 their *Rules and Regulations for the Prevention of Fire Risks arising from Electrical Lighting*—they were not called *wiring rules* then—were issued. There were 19 rules, and they occupied only three pages of the Journal. How many of our members, I would ask, have read these? They deal with dangers to buildings and to persons; and, so far as they went, they are nearly as sound now as they were then. I think one of the earliest published specifications of the general conditions of wiring framed on these rules was made for the Chelsea Company. I will not go into the details of that, and I mention it only to show that engineers at that time—I being the engineer of the Chelsea Company—looked upon the Institution rules of 1883 as being the foundations of the wiring specifications which they issued to their customers.

In 1888, the Institution appointed a committee to revise the rules of 1883. They produced thirty-nine rules, which occupied six pages of the Journal. Both the rules of 1883 and those of 1888 were strictly general; and the committee in each case avoided all attempts to specify *how* work should be done, and aimed at including only broad principles.

This was the more necessary because on each occasion they endeavoured to induce the technical advisers of the fire insurance companies to join them in framing uniform rules that would bear the authority of both the Institution and the insurance companies. In this attempt—and it was a very earnest attempt— they were unsuccessful. It appeared to us that the object of the advisers of the insurance companies was always to frame rules which, on the one hand, attempted specification, and, on the other, left certain conditions open for reference to the personal verdict of the responsible officer of the insurance company. Now the difference between rules compiled in the two ways I have mentioned is shown by the fact that the development of the industry between 1883 and 1888 called for only one revision of the rules of the Institution, whereas during the same period there were, for instance, nine or ten revisions of the Phœnix Fire Office rules.

The rules drawn up by the Committee of 1895–6, and so ably edited by Mr. Crompton, were approved by the Council in 1896 (during Mr. Crompton's presidency), and were issued in 1897. A new departure was then taken and their title was altered to some extent, and they were called "*wiring rules.*" The number of the rules, I may mention, was reduced to fifteen, but they occupied fourteen pages of the Journal and their whole character was changed. Their preamble states that they were arranged in such a form that they might be used as a specification of requirement and precautions. Certain descriptions

General Webber.

were added, as, for example, that relating to the "conductivity of copper leads," which afforded information of a kind not given in the 1888 rules. The committee's first duty was to bring the rules up to date, and, subject to the criticisms or the suggestions which may be made to-night, we believe that they then were, and that they still are so. My own impression is that most of the gentlemen who take an interest in this subject have not sufficiently perused the Institution rules.

As regards the suggestion for a testing laboratory, it seems to me that we have an example under our eyes, and that it is a question mainly of funds. We know that the Kew laboratory for testing instruments is to a certain extent—I am not aware to what extent—under the guidance of the Royal Astronomical and the Royal Society, and I do not see why such a laboratory, for the purpose described, should not be established under the patronage or guidance of this Institution. There can be very little doubt that if the advantage of uniformity could be shown to accrue, the necessary funds would be obtainable from those large bodies interested, not only in uniformity, but also in the best class of apparatus being known and, as it were, sealed by the opinion of an impartial Institution like this.

Mr. Bathurst.

Mr. F. BATHURST: In discussing the question of Wiring Rules, we all agree that we want uniformity, but I think we shall further agree that it would be better to dispense with rules altogether. We cannot, however, reach that point until we shall have found methods of wiring which preclude the necessity for rules. I quite agree with Mr. Crompton that the Institution rules, as they are at present, are a fairly representative set, and that they are certainly the best of any that have yet been published; but I also feel that Mr. Crompton and his committee must have had considerable difficulty in framing them, owing to the fact that they had to make provision for so many different systems of wiring. Before we begin to consider rules for ourselves we ought, I think, first of all to discuss systems of wiring, and in that discussion get down to the first principles upon which the wiring question is, or should be, based.

A glance through the electrical papers will show that at the present moment there are no less than eight or ten distinct systems competing with one another commercially. We might avoid this to some extent if we discussed the matter and found out what were the essential points we want, the one system that we could adopt uniformly, and the essential points that this system should have. It also appears to me that the insurance companies must look at the question from this side. They see each and every one of them described as the ideal, the best, the safest, and "the only" system, but yet they do not know which to adopt. Cannot we try and thrash out the principles and endeavour to find what we really need ourselves, and provide all the requirements that the fire office authorities look for? While there is this confusion amongst ourselves, and until we really get down to the standard basis, we cannot expect the insurance offices to forego the publication of rules. In making rules to cover all systems we handicap ourselves. We have had a discussion on the subject when I brought a paper before the Institution; but it was very evident from the way the discussion

wandered to all the different points, that we were only in what Mr. Herbert Spencer would call "the second stage of progress;" there was a "disagreement of the enquiring." Mr. Bathurst.

The insurance companies have the right to first consideration in these matters, inasmuch as they are financially interested in them. In the former discussion I tried to raise a scare as to what might happen in the way of fires, but not only the Institution, but the insurance people themselves, told me they were not having fires. I would like now to take the other side and say that as we have not had fires, we are evidently doing our wiring by present methods on such a safe basis, and on the score of safety we are so far in advance of paraffin, oil, candles, or gas, that the offices should help us by reducing premiums, or by making some concessions to us in respect of buildings which are lighted by electricity. If they say they must have rules, I think we should see if we cannot get a reduction in premium. It seems to me that the only way to attain this end is to agree amongst ourselves to place all our business with one particular set of fire offices, probably with those who are not working under the tariff rates which govern the majority of the large insurance companies, and try to persuade people whose premises we are wiring to place their risks in the hands of these same offices. In this way we might be able to put some pressure upon the tariff offices with a view to getting reductions. It is simply a question of rates. The fire insurance companies, if they are having no fires at all, are certainly in a very happy position for the time being: and of course we can understand they do not want to move away from it. On the other hand, we must admit that there can be electrical fires; and electricity is certainly the most versatile of all the fire fiends the insurance people have to deal with. My experience has been gained mostly in America, and I know that fires are so numerous there that they have had to find out the causes and examine closely into first principles. Hence they have found that there are more than 100 different headings under which electrical fires must be classed. We have, therefore, to admit that electricity may lead to fire-risk; but the insurance people in England have had such a pleasing experience that they do not know what fire-risk is from electrical wiring. Contractors and insurance companies are apt to say that they are getting on all right; but it is necessary to work out the elementary principles of safe wiring, without any regard to whose system is affected.

That brings me to Mr. Wordingham's paper, which I should like to criticise in closer detail. I agree with Mr. Wordingham that the general tendency of station practice is towards three-wire, continuous current stations, and Mr. Wordingham points out that there is a considerable difference between considering wiring from the point of view of safety, and from that of satisfactory utilisation of current. Of course the contractor does not want to study the former point of view; he would rather do his work at as little cost to his own pocket as he can. I do not think that our switches and devices are so imperfect as Mr. Wordingham would have us believe, and that it is necessary for station engineers to take up the testing of them. If the station engineers

Mr. Bathurst.

throughout the country begin to test switches and to look into matters of that nature, we shall not only be troubled about rules, but shall find that confusion will become worse confounded. On the other side, the contractors may have something to say about it, and Mr. Wordingham's load may then be more "peaky" than it is at present. Of course, if Mr. Wordingham finds serious troubles in the direction he mentions, there is something in his suggestion; but I think the best way is to have the discussion at a meeting of this Institution, and to ask the station engineers to say what their troubles are, and leave it to the manufacturers to provide proper switches and fittings. No doubt the manufacturer who then works out the best fittings will have the best chance in the commercial field. To lay down any rules as to length of break or height of cover, or any similar points about switches, is, I consider, very much like baying at the moon, because each individual fitting has its own peculiarity. The true test of merit is the behaviour of each in actual use.

The point I wish chiefly to criticise in Mr. Wordingham's paper is the question of earthing. I have now been many years trying to develop a satisfactory standard method of wiring, and believe that we shall never get such a method until the station engineers, the insurance companies, and every one interested in the question, allow us to adopt an earthing system. The earthing solution of the question should put us in the way of reducing cost, so that we can compete commercially with gas-piping. All the competition we are now experiencing in electric wiring has, I believe, its genesis in the fact that we cannot yet compete with gas-pipe. If we could so compete, every builder would make provision for electric wiring, instead of fixing gas-pipes in new buildings. This would enable the station engineers to improve their load, and would obviate the necessity of wiring premises after they are occupied, and therefore at a considerably increased cost. To solve this question the insulation will have to be removed from one of the wires. We want an imperishable armoured insulation, and, this given, I believe we can rely upon every element of safety and convenience. We should do well to discuss further this whole question of earthing with Mr. Wordingham's instances before us. Mr. Wordingham said we shall have electrolytic trouble set up in the earth between the premises of two consumers several hundred yards apart, on account of the current flowing down the middle wire and causing a fall of potential of several volts. But he has pointed out in his paper that it is a comparatively easy matter to balance the lights in a consumer's premises; and with a proper system of wiring I believe it is possible to balance to a light on each side of the three-wire system. This being so, I do not know why Mr. Wordingham expects this heavy return current in his third wire. But supposing that the premises are not balanced, and a number of premises are out of balance together, so that there are currents on the third wire (taking as an instance a case mentioned to me by Mr. Wordingham, in which there was a drop of fifty volts per mile), we find that even then, there would be a drop of only two or three volts per 100 yards. I do not think a difference of three volts would so damage the adjacent gas and water pipes that the Board of Trade would step in and say the earthing is not to go on. But if experience should prove the necessity

therefore, the volts could be kept down to any specified drop that might be agreed upon if the station engineers would simply increase the copper capacity of their mains accordingly. Mr. Bathurst.

[*Communicated.*] My point is, that it is the station engineer's business so to arrrange or develop his mains system, as to *assist* the growth of connections. The station engineer urged double voltage (200 volts supply) when he was pinched by the cost of running mains to distant consumers, and I urge that if the consumer himself could now speak, he would ask for such a disposal or "halving of insulation"—that is, earthing one side—as would proportionately reduce the first cost of his electric wiring. It is of comparatively little advantage to reduce the price of the unit if we keep the possibility of being able to use it so far out of the reach of the general public.

Speaking as the advocate of a particular earthed-outer wiring system, *I am convinced* of the possibility of providing low-cost wiring for the consumer; wiring that shall be permanent, durable, and trustworthy; wiring that shall present no danger risk, either to fire office or station supply—which by automatically checking its own construction need necessitate a minimum of control in respect to rules; which shall compete in cost and simplicity with gas-piping, and be as generally acceptable. Before suggesting this, however, it is first necessary to reach unanimity of opinion as to the exact requirements and first principles of wiring, either by discussion here, or by nature's more drastic method—business competition; and whichever way we treat this question, the less red tape and restraining rules we have, the quicker we shall reach a solution.

Professor W. E. AYRTON: At this stage I should like to hear from the representatives of the fire insurance and supply companies who may be here, on what grounds they have decided to demand certain insulation resistances when houses are wired. Some companies are satisfied with about 12 megohms per lamp; some, as Mr. Pigg points out, want 112 megohms per lamp. Now, I do not suppose that these companies have arrived at the numbers wholly at random, and it is therefore of great interest to know whether the 112 megohms per lamp is a wholly unnecessary standard to set up, or whether an insulation resistance of 12 megohms per lamp, which would be possible under one set of regulations (but hopelessly rejected under another) means that the work ought to be condemned. It seems to me that until the representatives of the companies tell us on what principle they have arrived at these numbers, it is no use adopting the suggestion that has been made this evening, to take an average of all the numbers and specify the mean. That course appears to me to be quite wrong. The case of the screw thread is totally different, because there is no question of danger if you use one screw thread, or of the absence of danger if you use another; it is a mere matter of convenience. But here we must assume that a company which says that you must have 112, or perhaps 75, megohms per lamp, and that they will not pass anything like 10 megohms per lamp, must consider that there is a danger or great loss of current to the supply company with such insulation; and so the company which aims at the highest standard might quite rightly say, for reasons which I do not Professor Ayrton.

Professor Ayrton.

know but which I want to get from them, that they will not take any average; while the other company might say, for other reasons I do not know but which I want them to tell me, that 12 megohms was enough, and that there was no reason why you should entertain any much higher standard in order to obtain an arithmetical mean.

The Chairman.

The CHAIRMAN: I am sure we shall be very glad if any one who represents the insurance companies could give us an answer to the enigma propounded by Professor Ayrton.

Mr. Gawthorp.

Mr. C. A. GAWTHORP: We must all recognise the troubles of the contractor when he has to wire a building which is insured in three or four offices and has varying rules placed before him for his guidance. I do not wish to represent the views of the North British and Mercantile Fire Insurance Company officially, but merely to express my own opinion as their Electric Light Surveyor. I recognise that the rules of the Institution are upon a sound and practical basis. To some extent they are more stringent than the rules of my office. In a few sections the requirements are below those of the North British and Mercantile Company, and in one or two cases there are differences that are rather serious, but not sufficiently so to preclude all possibility of agreement. Indeed, I firmly believe that we can, and even think that we shall, come to an agreement.

It appears to me that the test of insulation resistance required by the Institution is unnecessarily high if it is to be uniformly enforced in all conditions and systems of wiring. It is rather hard on the contractor when he has to wire a new and very wet building, with wet plaster all over it, to bring it up to that test. I have known several cases in which the wiring had been carried out apparently perfectly and on the most approved system, and yet when the test was taken at full working pressure the resistance could not be got up to the required standard; and I am sure that it is sometimes impossible to attain to that laid down by the rules of this Institution.

There is, however, a difficulty that has hardly yet been recognised; it is this: the different sets of rules issued by the electrical supply companies are nearly ten times as numerous as those issued by the different fire insurance companies; and unless the supply companies fall into line with the Institution, I am afraid it will not be much use for the insurance companies to agree on that subject. I think it is necessary for the municipal engineers and the engineers to the private companies, as well as the fire insurance companies, to come into agreement with the Institution.

It has been said that the fire insurance companies do not recognise the Institution rules, but I think that that is not strictly correct, and one instance will suffice to illustrate the point. In a case in which I was personally unable to make a survey, the electrician, who was a responsible man, certified that the work was carried out in accordance with the Institution rules, and I therefore passed that work. It is not necessary at this moment to refer to the points of difference which exist, as they would be better discussed in committee than at a General Meeting of the Institution.

Mr. Human.

Mr. H. HUMAN: I am certainly in sympathy with Mr. Crompton in

his laudable endeavour to bring us to a common basis in this matter of rules, and shall be pleased to do all I can to assist him in that direction. Nevertheless, I recognise that there are great difficulties in the way. First, there are the so-called vested interests and imagined rights of the individual; and then different people look upon these matters from different points of view. At one end of the scale we have the engineer of a system of supply who is responsible for one form of distribution with one more or less uniform pressure, and who is practically concerned with but one form of wiring. At the other end of the scale we have the fire insurance office, which has to deal with every known form of wiring, and with pressures varying from 50 up to 250 volts. How are you going to leaven the two? The engineer, for reasons quite apart from the fire-risk, finds it necessary to impose very severe restrictions particularly in the matter of insulation. The fire office, having merely the fire-risk to consider, can be content with less, and moreover, having to meet a greater variety of conditions, its rules must be necessarily more elastic. If you strike a mean, that entails levelling up on the one hand—no great matter—and levelling down on the other—quite a different thing. Is the engineer likely to come down in his high exactions? I think not. These are difficulties which must be faced; but surely there is some reason to hope, that, although we may not be able to realise Mr. Crompton's ideal of a single standard set of rules, we should yet succeed in bringing our several rules into line on all important points. In that case the rules of this Institution would form a connecting link between the other ninety-nine, and in that sense become a standard for all. Mr. Human.

Now coming to the rules, I should like to call Mr. Crompton's attention to one or two clauses which seem to me to require further consideration.

First turning to page 4, I notice under "Concentric Conductors" a great deal is said about the outer dielectric, which rather implies that an outer bare return is not permitted. Then on page 6 I read, "Conductors spaced and separated away from the walls should not be permitted unless they are mechanically protected throughout their entire length." I doubt if any fire office could impose such a rule. There are numerous instances where that form of wiring lends itself admirably for the particular building. All that is necessary is to protect them where in reach or where exposed to injury, but to require protection throughout their length is in my opinion going too far. On page 11, under Dynamos and Motors, now that motors are coming so extensively into use, the fire offices have found it necessary to impose some rather stringent regulations with regard to protecting them against inflammable dust and flyings, for we meet with them by no means alone in engine-rooms or separate compartments, but in workshops and in the mill-rooms themselves. I think, therefore, that rule needs strengthening in the direction I have indicated.

I now come to page 13, where that very knotty question of insulation tests is dealt with. I doubt the expediency of requiring twice the working E.M.F. in all cases, especially as we have now reached 250 volts in our lamp circuits. We should be content, I think, with the

Mr. Human. working pressure, with a proviso in no case less than, say, 100 volts. As to the constant 10 megohms divided by the amperes, that is certainly a reasonable requirement and sufficient, no doubt, for fire purposes. But I doubt whether the engineer will regard it sufficient for his purpose, so that it will need levelling up.

Some few years ago the *Electrician* published a diagram giving some curves plotted from the rules then in vogue. It was interesting, though scarcely edifying, for the difference between the lowest and highest curve was something remarkable. At the bottom, if I remember rightly, was the former rule of this Institution, and at the top was that of one of our London supply corporations. The editor suggested that some simple formula should be devised to meet all cases. In a weak moment, doubtless, I essayed the task, and sent the editor a formula which certainly had the merit of simplicity. I merely took the volts, divided them by the number of lamps, and let the quotient equal megohms. It had this virtue, that, unlike any other rule I know of, it did make allowance for varying pressures. In those days we did not much exceed 100 volts, which meant practically 100 megohms per lamp. That exceeded the highest curve, but I purposely put it high, because a rule to become general must level up, and we were then threatened by the supply companies placing their requirements even higher. Now that we have 250 volts, 250 megohms per lamp is, of course, excessive. Possibly one-fourth would meet the case.

Next as to the divisor being in amperes. Strictly speaking, we should take into consideration the amperes as well as volts when dealing with insulation; but in practice we are more concerned with the number of lamps than with their candle-powers, for the reason that our leakage is mainly one of surface; and surface leakage is represented by cut-outs and switches, which are governed by the number of lamps and not their candle-powers. I prefer, therefore, to divide by lamps rather than by amperes.

Finally, I come to the table at the end which interprets the rule dealing with the current density. I may say, I take a fatherly interest in that rule and table, for I think I was the first to deal with the matter upon a temperature basis. I adopted 10° centigrade as my limit due to current, which gave us all the security we could desire, and at the same time allowed ample scope to the engineer. But I knew that the rule alone would be Greek to the million, and therefore furnished a table to make it understandable, especially to the plumber and gas-fitter. I am pleased, therefore, to see this Institution following on similar lines in adopting a temperature limit and accompanying it with an excellent table. They have reduced the limit, which they can well afford to do without cramping the engineer. I accept the correction, and shall be pleased to adopt both the rule and the table.

Mr. Sheppard. Mr. J. SHEPPARD: I would simply remark that I think if the Electrical Engineer, the Fire Insurance Office Engineer, and the Supply Company Engineers would agree amongst themselves, they would have no trouble whatever.

Mr. Langridge. Mr. L. J. LANGRIDGE: Speaking as Electrical Inspector to the Royal Insurance Company, I may say that I have such hopes of our

Company being able to adopt the Institution rules that I have both refrained from formulating any rules myself and have recommended my Directors not to have any formed. Mr. Langridge.

Mr. R. J. Wallis-Jones: There is one aspect of the question that has not been touched upon by previous speakers, and that is the position of the outside public in regard to it. We are at present in disagreement amongst ourselves as to what are safe conditions of working, and I think that the sooner the Electrical Engineers, the Supply Company Engineers, and the Fire Insurance Inspectors can agree upon this matter, the better it will be for everybody, because the difficulties met with in getting people to adopt the electric light are quite sufficient in themselves, without adding to them by suggesting to the public that there are possibilities of danger of which we ourselves are not certain. Mr. Wallis-Jones.

I think that Mr. Crompton's suggestion is one of the most valuable that has been made for many years. There is no doubt that we must aim at getting standard rules which shall be accepted by all parties.

No allusion is made in the papers before us to the atmospheric conditions under which tests for insulation are made. I am, of course, aware that there are great difficulties in the way, but I think that some difference ought to be made in the insulation standard required in a place which is permanently damp, as, for instance, where the test is made in damp weather or in bacon-curing sheds, where it is impossible to get a test which would be easy to obtain in a dry place.

The two chief points to be considered in wiring are: insulation, and the protection of the circuits of the safety fuses. I think that the rules of the Institution are excellent so far as the fuse goes. It practically amounts to saying that you are quite safe if you run your circuits with a 5-ampere fuse in each pole at the distributing point for 100 volt circuits, and 3-ampère fuses for 200 volts and upwards. If those rules were adhered to throughout, there would be very little trouble, I think, in general wiring work.

Finally, I think it is open to question whether the frames of motors should be earthed, as provided for, when the voltage exceeds 250, because this practice may give rise to a dangerous, or at least an objectionable, shock, if the attendant should put his hand on the frame of a motor which is earthed, and should at the same time touch the commutator.

Mr. Ruthven Murray: In response to the Chairman's invitation, I have only to say that, as a Station Engineer, I am looking forward to the day when we shall have standard regulations, and am awaiting their publication. It is most unsatisfactory in moving from place to place to have to draw up a fresh set of regulations, and to have all the contractors inquiring why the new regulations should be different from those of the insurance companies. Mr. Ruthven Murray.

Mr. Raphael: Like Mr. Pigg, I had occasion to examine the various rules issued by supply undertakers, with the intention of including a summary of them in a pocket-book which will be published this year. There are, as Mr. Pigg has pointed out, very great differences in these Mr. Raphael.

Mr. Raphael.

rules in regard to the question of insulation, the subject which is naturally the most important to the inspector. But from the wiring contractor's point of view it is less important than those relating to such questions as material and position of fuses, whether the motors should be earthed or not, how the main fuses and main switches should be connected, etc. Naturally when one sees so many sets of rules it occurs to one whether it would be possible to make one set of rules, not to occupy the mean position, less strict than some and more strict than others, but more stringent than all; so that if a building were wired according to this set of rules it would be within all of them. As far as insulation resistance is concerned, this could be effected by simply taking the highest curve or by drawing out all the curves and taking one which touches the top points; but in the case of such things as I have mentioned (material, position of fuses, earthing of motors, connections of fuses, etc.) it is impossible, because one set of rules specifies one thing and another set the exact opposite. Mr. Drake has mentioned the case of ceiling roses, and there are many more similar instances.

When going through the various regulations, I observed that one central station uses two sets of rules in which there is an apparent contradiction. One set relates to "General Installation Work"; the other is issued for those houses which have been wired for 100 volts and are to be changed over to 200 volts. The first set specifies: "All fuses to be of pure tin wire, with copper terminals soldered to the fuse wire. Fuses composed of copper will not be allowed. Every fuse must be carefully adjusted so that it will melt at 50% above the normal current it has to take." The other set, for houses in which the pressure is to be changed to 200 volts, says that "Pure tin wire should always be used for circuits of 2½ amperes at 200 volts. But where fuses control from one to three lights, it is better to use *a single strand* of No. 38 or 40 copper wire (taken out of a piece of flexible), for very fine tin wire is too soft and liable to be broken under the heads of the screws in a fuse fitting." It is rather hard for a contractor working on installations under one company to have two sets of rules to follow—harder even than for the provincial contractor who has to work to several sets of rules, depending on the town he is working in.

With all deference to Mr. Wordingham, I think that the differences in these rules issued by Supply Undertakings cannot possibly be accounted for by local circumstances. If one examines them, one finds absolutely no reason at all why some of them should differ. Sometimes one would think a specification, say for the number of lamps connected to one pair of house terminals, ought to be higher in one station than another, judging by the conditions; but on referring to the rules it is found that just the reverse is the case. I am of opinion that the Institution rules if they are not yet sufficient to cover everything required by fire offices and by supply undertakers, could be easily extended so that they should be sufficient. Some differences might have to be made for 2, 3 and 5 wire networks and 200- and 100-volts systems, but this could be easily effected by making a few modifications in the rules. I have no doubt, as Mr. Wordingham is anxious to have a uniform set of rules, that he put this matter before the other members of the

committee, and possibly it was because he was unsuccessful in this that he went back to Manchester and made an additional set of rules of his own. Mr. Raphael.

With reference to the Institution rules, I notice the conductivity of copper is defined by a figure which is the same as Matthiessen's old B.A. standard for pure annealed copper wire. But it is merely stated at so many ohms. If this is taken to mean International ohms, the standard is somewhat below that given by Matthiessen. Perhaps this was intended so as to allow the wire manufacturers to go to this lower limit, something about 98 per cent., or was it unintentional? I think it would be well if perhaps one of the members of the committee would clear up this point.

Lastly, Mr. Pigg is somewhat unkind to the framers of the various rules in criticising their unscientific language, especially as he is guilty of something similar himself. In his table, the last column is headed "Insulation resistances of conductors. Megohm-miles." I suppose he is not content to use the expression which is perhaps scientifically inaccurate, but which has been countenanced by long use by cable companies and submarine cable engineers, "megohms per mile." But in that case, why does he head the previous column "Insulation Resistance per 64-Watt Lamp"? It seems somewhat inconsistent. I would not have pointed it out, only I thought he was somewhat harsh in his criticism on the language of the other rules.

Mr. H. Hirst: In course of the discussion this evening, everybody seems to have agreed that we want rules, and that we need standardising, and yet nobody seems to be acting as if it were so. Many different authorities entitled to draw up rules have been pointed out, such as the station engineer, the contractor, and the fire insurance companies; the manufacturer has not been alluded to, but I will take the liberty of mentioning him. So long as we use a little tact and get the fire insurance companies' engineers to agree with us as to the rules that we fix, I think we ought to have it in our hands to decide upon some standard very easily. Every station engineer, every man of importance in the trade, is a member of this Institution; and everybody is anxious to have some diploma, such as that of the M.I.E.E., or A.M.I.E.E.. Now, I would like to know whether the Institution could not exercise some authority over each member. Mr. Wordingham has mentioned that we might have an officer of our own to test and give certificates; but what guarantee have we that the standards of that officer would not differ from those of the Board of Trade or of the County Council? I should say that if the Institution were to appoint a Committee not only of just a few experts and scientific people, but also of the different factors that form a link in the chain of the wiring business, and if that Committee were properly elected, rules could be devised and enforced on the members of the Institution. It may be a new suggestion for this Institution, but when a man joins a political club he has to acknowledge the rules and the ways of his leaders, and I should say most members would be very glad to follow the leaders who would constitute the Committee of this Institution. Mr. Hirst.

Mr. M. H. Galsworthy: There is one point to which no reference Mr. Galsworthy.

Mr. Galsworthy. has yet been made in the discussion, but upon which I feel rather strongly. I should like to see provision made in the rules for two successive tests of new installations in buildings, a lower standard being adopted for the preliminary test than for the later, which should be made after the lapse of, say, a couple of months. I have often found, on testing a building in which the work has been thoroughly well done, that the test has come out absurdly low, and that about two months afterwards a most excellent test (something like infinity) has been obtained.

Mr. Brown. Mr. FREDERICK BROWN: I rise to speak as a contractor from the Midland counties, where we are some distance removed from the head offices of the fire insurance companies, and where we do not get from their inspectors the careful supervision that I should like to see, as it is to the interest of all contractors that the inspections should be very much better than they are. The present plan is to send a form, which takes a man a considerable time to fill in, with fifty or sixty questions, which the insurance company has no right to ask, and the contractor is not paid to answer. I do not see why he should do it. I have, before now, for the amusement of the thing, sent it back saying, much to the astonishment of the office, that my charge for filling it up would be one guinea. I eventually answered the inquiries without a fee, but at the same time I do not think we ought to be called upon to do so. We find there are many small contractors—the plumbers and gas-fitters were mentioned by one speaker—who undertake a number of jobs at any price without much knowledge. These forms are filled in by them and are accepted by the offices without the signature of any one with long experience. Whether they are checked or not is a question: in fact, I know privately that they are seldom or never checked in the case of one large office dealing with small installations. I know of small private houses away in the country which it is hardly worth the while of the insurance inspector to examine for himself. It would not pay him, as the money he receives is probably only four or five shillings a year in all; he therefore accepts a signature which may be absolutely worthless. Until some means of checking these small inspections is found, we shall never be sure that work is being carried out according to the rules of the offices. That is one of the things the Institution should try to see remedied, if it be possible. It is a difficult thing to do, no doubt; but the root of the whole trouble of electric fires will be manifested presently by such installations as those I have spoken of. It has been suggested that the size of the wire may be reduced, but nothing has been said about the drop of E.M.F. on house wires. I find considerable difficulty in competing with some other contractors who will specify a wire which is in accordance with the rule as to size, although the drop in voltage at the far end of the wire does not enter into consideration in the specification. We often get specifications from architects who will carefully put in all the sizes which are necessary, and there may be a long length of wire (I had one three weeks ago with nearly a quarter of a mile of wire, and there was an allowance of 1,000 amperes to the square inch), but nothing is

Mr. Brown.

said about the drop of E.M.F. Without considering the drop I could have got the order very easily, but then when an allowance of only two or three per cent. has to be made, the price goes up very materially; hence considerable attention must be paid to the drop of the E.M.F. in the main, independent of the carrying capacity of the wires.

If some step can be taken in the Institution to induce that co-operation among the contractors which is greatly to be desired, I believe that the good to the trade would be very great indeed. But it is a very difficult and wide question. If some of our leading London contractors could arrange for a meeting with some of those in the country, I feel sure that good would come of it. This would be a step in the right direction, and while I am here to-night I should like to emphasise the desirability of taking it.

With regard to earthing, I think it is dangerous to earth, particularly as the tendency is to increase the voltage on the various installations. It does not matter much in the case of 100 or 200 volts, but we shall by and by have to deal with much higher voltages. The Central Station Engineer must look to his capital, and for that reason he takes the voltage up and keeps the copper down. That being so, we are sure, in private houses, in cellars, and damp places, to get shocks which will militate against the undertaking. If we have 400 volts to earth, and somebody gets a shock from it, there will be considerable disturbance made, and it is a question, therefore, whether it is desirable to start earthing on one side of the wire.

On the motion of Sir Henry Mance, the discussion was adjourned.

The Chairman.

The CHAIRMAN: I have to announce that the Scrutineers report the following candidates to have been duly elected.

Members:

O. W. Brain.
W. G. T. Goodman.

Associate Members:

Mark Feetham.
David Allan Hamilton.

Associates:

J. Douglas Allan.
Francis James Edgar.
John Arnold Foster.
Herbert Buchanan Harvey.
Thomas Ernest Herbert.
Richard Ernest Holdom.
Edward Astley Johnstone.
Willie Dickson Kilroy.
Robert Valentine Macrory.
Tom Rolls Renfree.
Edward John Saner.
Henry James Tomlinson.
Albert C. Unbehaun.

Students:

Guy Lyndon Drury.
Harry Augustus Nott.
Cuthbert Ernest Wells.
Herbert Tatlock Wilkinson.

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2. The Library is open (except from the 14th August to the 16th September) daily between the hours of 11.0 a.m. and 8.0 p.m., except on Thursdays, and on Saturdays, when it closes at 2.0 p.m.

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JOURNAL

OF THE

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The Three Hundred and Twenty-Sixth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, February 9th, 1899—Professor SILVANUS P. THOMPSON, F.R.S., Vice-President, in the Chair.

The minutes of the Ordinary General Meeting held on January 26th, 1899, were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council :—

From the class of Associates to that of Members—

J. C. Kidd.

From the class of Associates to that of Associate Members—

A. J. Abraham
M. A. Abrahamson
W. Aitken
A. H. Allen
A. V. Anderson
C. A. Astrom
H. S. E. Austin
F. Ayton
W. J. Bache
F. J. Bakewell
H. A. Barnett
C. F. Barrett
E. H. Barton
A. H. Bate
W. Bates
F. Bathurst
J. R. Bedford
W. J. Benton
H. H. Bigland
A. G. Bird
G. M. C. Black
J. R. Blaikie
W. Blenheim
C. J. Bosanquet

G. W. Bousfield
H. V. Bowen
J. J. Bowman
M. A. Boyd
W. Boyd
I. Brady
E. S. Bradburne
C. D. Braddon
G. Bradley
J. H. C. Brooking
R. P. Brousson
Wm. R. Brown
H. J. S. Brownrigg
G. A. Bruce
G. Bryant
J. K. Brydges
M. J. Buckley
H. R. Burnett
A. Burton
E. Cardin
E. R. Carr
S. A. Cartwright
J. McI. Cater
A. J. E. Catto
A. D. Chamen
I. W. Chubb
G. F. Chutter
W. B. Clarke
H. W. Clayden
W. C. Clinton
P. H. Cole
W. W. Cook
W. C. Cooke
W. H. Cooke
A. T. Cooper
J. N. Cooper
W. R. Cooper
W. L. Copp (Jr.)
W. R. T. Cottrell
R. H. Coverton
E. H. Cozens-Hardy
E. H. Crapper
E. Crocker
W. Cross
E. G. Cruise
A. Curtis
J. E. Dane
A. H. Darker
A. L. Davis
T. Davis
P. H. Dawe
A. A. Day
F. E. de Guerrier
M. C. Dent
W. Dickinson
A. W. Dixon
S. P. Doudney
W. Douglas
W. E. D. Duncan
J. D. Dymond
E. S. Eccles
K. Edgcumbe
W. R. Edwards
F. E. Elmore
A. T. Evans
C. J. Evans
W. M. Evans
E. I. Everett
W. H. Everett
S. A. Faber
F. Fairley
C. K. Falkenstein
H. Farmer
H. Fentum-Phillips
R. F. Ferguson
J. H. Field
R. H. Fletcher
J. H. Fooks-Bale
A. C. Fora
W. Fowler
O. F. Francis
J. Frith
C. Furness
M. H. Galsworthy
S. S. Galsworthy
H. J. Garnett
L. Gaster
C. A. Gawthorp
F. A. Glover
E. T. Goslin
A. Gray
G. W. Green
W. E. Groves
E. N. Gulich
H. E. Hall
W. H. Hall
S. Handford
F. S. Hanning
A. G. Hansard
A. C. Hanson

W. Harling
H. R. Harper
H. Hartnell
A. P. Haslam
F. Haward
F. O. Harrison
G. W. S. Hawes
L. W. Heath
B. B. Heaviside
C. F. Hesketh
T. Hesketh
J. R. Hewitson
E. L. Hill
H. C. Hodges
W. D. Hodgson
R. C. Holness
F. B. Holt
B. F. Howard
R. J. Hughes
G. H. Hume
T. E. Ingoldby
E. L. Ingram
H. A. Irvine
J. M. Irvine
A. Jaray
O. M. Jonas
H. W. Jones
H. E. Keen
H. E. M. Kensit
T. A. Kerr
F. W. Lacey
C. G. Lamb
J. F. Lamb
F. B. Lea
H. C. Leake
C. D. LeMaistre
A. E. Levin
E. H. Liebert
S. E. Linsell
G. C. Lundberg
E. W. J. Macdonald
A. P. McDouall
S. L. F. McLauchlan
F. J. Madgen
A. E. Malpas
J. W. Manley
W. Markby
T. E. Marsh
D. Martin
W. Maurice
C. M. Mayson
W. P. Mendham
L. Miller
S. J. A. Mills
S. W. Mitchell
W. Moat
A. E. Mohring
A. Moir
G. F. Moller
G. W. Money
W. M. Morrison
A. G. Newington
H. C. Newton
H. G. Nicholson
W. M. Oliver
M. C. Olsson
M. Osborn
C. G. Paget
G. D. A. Parr
W. S. Parsons
C. S. Peel
J. E. Petavel
H. H. Perry
P. Peters
E. G. Phillips
R. W. L. Phillips
J. Pigg
G. E. Piggott
O. A. Pilcher
G. F. Pilditch
W. B. Pinching
F. A. Pocklington
E. Poole
P. J. Pringle
C. A. L. Prusmann
E. E. Putland
G. Ralph
F. C. Raphael
W. R. Rawlings
A. B. Rayner
S. Rentell
S. Richardson
A. B. Rigby
H. Ritchie
T. E. Ritchie
E. Roberts
H. I. Rogers
L. L. Robinson
W. M. Rogerson
T. A. Rose

J. T. Rossiter
W. Rowland
M. Ruddle
A. E. Ruddock
S. G. C. Russell
A. Rutherford
G. H. Sayer
A. H. Shaw
C. M. Shaw
J. Shaw
R. Shepherd
P. R. Shill
H. H. Simmons
E. L. Simpson
F. C. H. Sinclair
C. E. Skelton
A. T. Smith
H. S. Smith
Wm. H. Smith
Wm. Hy. Smith
J. McF. Smyth
C. N. Staniland
W. Stevenson
Geo. Stobie
A. L. Stocken
W. W. Strode
Leo. Sunderland
R. Sykes
P. S. Tasker
F. H. Taylor
G. E. Taylor
F. R. Thackrah
W. J. R. Thomas
C. S. Thomson
E. G. Tillyer
J. L. F. Vogel
H. Walker
L. H. Walter
W. A. Walton
T. Wardell
F. J. Warden-Stevens
L. M. Waterhouse
G. C. Weston
W. H. Whitehouse
J. H. Whittaker
F. L. Wilder
A. Williams
W. W. Williams
N. J. Wilson
H. A. A. Wood
H. G. Wood
L. W. Woodman
J. C. Woodburn
T. H. Roberts Wray
G. E. Wright
J. Wrigley
T. P. O. Yale
G. A. Zeden

From the class of Students to that of Associates—

Frank Powell.

Mr. C. A. Gawthorp and Mr. J. M. Donaldson were appointed scrutineers of the ballot for new members.

A donation to the Library was announced as having been received since the last meeting from Sir John Wolfe Barry; to whom the thanks of the meeting were duly accorded.

The CHAIRMAN: It has been suggested to me that I should give a word of explanation as to how the Council has dealt with the transfers from the grade of Associate to that of Associate Member. The present Article defines the new class of Associate Member as follows:—

13. Associate Members.—Every Associate Member shall be either an Electrical Engineer or Electrician, and shall be at least 25 years of age, and, whether admitted by election or by transfer, shall come within one of the following descriptions :—

(a) He shall have been on the Register as an Associate on the 31st of December, 1899 ; or
(b) He shall have been educated as an Electrical Engineer or Electrician in a manner which shall satisfy the Council, and either (1) he shall have had subsequent employment for at least two years in a responsible situation as an Electrical Engineer or Electrician, and shall be actually engaged in such situation at the time of his application for election or transfer ; (2) he shall have been engaged for at least five years in one of the branches of Electrical Engineering, and shall be actually so engaged at the time of his application, and shall afford satisfactory proof to the Council of his fitness for election ; or (3), being an Associate, he shall have gained a premium in any year for a paper read at an Ordinary General Meeting of the Institution.

There were received, down to last week (but others are still coming in), something like 350 applications from Associates of this Institution, who were on the Register on the 31st December, 1898. In deciding the transfers the Council, having many other things to do as a Council, selected a committee of six gentlemen from its own body, and entrusted them with the duty of making a selection. That committee has held a number of meetings, each lasting over several hours. It has given great attention to the cases, has considered every individual application, and has, in many cases, entered into correspondence either with the applicant or with some one who might from his claims be reasonably supposed to know something about him ; with the result that while there are a number of cases still under consideration, because the replies have not been received, in a number of other cases the committee has come to a definite decision—in some cases unfavourably to the claim, in other cases favourably. As a first result that committee has to-night sent forward to the Council—and the Council, as you have heard, has approved—a list of 282 names of Associates who are now transferred to the class of Associate Member, and are entitled to put after their names the letters A.M.I.E.E., whilst those who remain Associates are entitled to use the letters A.I.E.E. in the same way. That leaves about seventy of the original applications to be

disposed of; but we may take it that the great part of the work of that committee has been done—and, as I know, very carefully done.

I believe I am stating the unanimous opinion of the Council—though I am not expressing it in any formal way—when I say that the result of this very large transfer, as it will work out in the course of years, will be not that those who may be left behind in the class of Associates will be looked upon as being in any lower class than that of Associate Member, though they may not be technically or professionally either electrical engineers or electricians. There are a number of gentlemen in other professions—for example, such a profession as that of Civil Engineer, Mechanical Engineer, Patent Agent, closely associated with ours, or of analytical chemist, which is also closely associated in many cases with electrical industries—men who have high standing in their own profession but do not belong to our profession, yet whom we would wish to have with us as Associates. Therefore it is no dishonour for any man to be left behind in the class of Associates if he does not actually and *de facto* come under the qualifications which, as understood by the Council, will entitle him to become an Associate Member.

We have now to continue the discussion of the three papers which were before us a fortnight ago. A number of gentlemen then signified their desire to speak on this matter, and I believe Professor Ayrton said on the last occasion that he would like to say something.

Professor Ayrton.

Professor AYRTON: In the few words which I said at the last meeting I suggested that it would be desirable to obtain information as to why such totally different standards of insulation resistance per lamp should be specified for buildings by the various supply and insurance companies. Up to the present time no answer has been given to that by the representatives of the companies. I have myself endeavoured to ascertain, by looking through the numbers, whether any common system seems to influence the selection of the particular standards. One reason for selecting a very different standard of insulation resistance per lamp suggests itself to every one, namely, that although the same lamps may be supplied to the houses, and although the houses may be furnished with the same declared pressure, in some cases the pressure used in distribution may be low throughout, whilst in others the low pressure at the consumer's terminals may be obtained after transformation on his premises. It would seem possible, then, that a company which used any kind of transformation would desire to avoid

Professor Ayrton.

the possibility of accident due to the leakage from the primary to the secondary—from the relatively high to the relatively low pressure—and would require a higher insulation resistance per lamp than a company which worked entirely on the low-pressure system. That suggestion, however, falls to the ground, because, between the two towns Nottingham and Oxford, for example, there is a wide variation in the insulation resistance per lamp required (namely, 75 megohms per lamp at Nottingham, and only one-sixth of this, or 12·5 megohms per lamp, at Oxford); yet Nottingham is on the low-pressure system, while Oxford has a high-pressure direct current system of distribution, and therefore transforms down. Again, the Westminster Electrical Supply Corporation, which is also a low-pressure system throughout, is the one which requires the highest insulation resistance of all. It varies from 60 to 112 megohms per lamp, according to the number of lamps installed. So I am at a loss to understand why these divergent standards are specified by the various supply companies. Why a company which is never using more than 200 to 400 volts in any part of their system should require 75 megohms per lamp, whereas a company which deals with 1,000 volts in their system should be satisfied with 12·5 megohms per lamp, I do not know. What inconsistency and inconvenience it must lead to! In London and in some other towns there are, of course, a number of companies and districts joined together. Indeed, in London we have more than that: we have districts which two companies can supply. Take the case, however, where two companies serve adjoining districts. You can imagine two perfectly similar houses on opposite sides of the street being built at the same time, having about the same number of lamps, and being wired in the same way by the same firm. We will say that the insulation resistance obtained in each house is 20 megohms per lamp. The contractor is told in the case of one house that his work is very good, and that it is passed; whilst in the case of the house over the way, which has the same insulation resistance, and is supplied equally on the low-pressure system but by a different company, he is told that the work is extremely bad; and even if it had three times the insulation resistance it would not be passed!

Now, there is another very curious result that this inconsistency has led to. I have myself known houses in London which have been intentionally changed over from one company to another merely because of the difference in the standard. I could mention a house which was condemned utterly, and the company said that they could not continue supplying current. It was pointed out by the people of the house that they were holding a reception and that there was no other means of lighting the house. The company replied that they would send an inspector, who should wait there the whole evening; and that under that condition, it being left to him to switch the current off the main switch at his discretion, they would, under the special circumstances of the case, continue to light the house. In such a case, if one company wants (say) 75 megohms and the other is content with a lower insulation (say 12 megohms), obviously the best course is to change over. That was done in this instance,

Professor Ayrton.

and no further objection was raised. I have asked some of the insurance companies' representatives what principle has guided them in selecting a standard. One replied that if they adopted a standard lower than that of any known supply company, they would feel quite certain that the work passed by any of the supply companies would conform to their standard. Therefore, in order that there should be no question of the work being refused, they adopted a standard as low as, or somewhat lower than, any that any supply company they knew of would be likely to demand. That was their system. The representative of another insurance office told me very much the same. He said competition was so keen in insuring that they felt, if the work was at all safe, they would be foolish to reject it, for some other company would certainly take the risk. I still hope we may learn some reason for the standards adopted. I have dwelt on the point somewhat in order to correct a wrong impression that my remarks at the last meeting appear to have produced, namely, that I was in favour of a low standard. That is far from what I intended to say. What I did say was, that I am in favour of a high standard; but I want to know why some companies are satisfied with their low standards—that is, whether they have a fair reason for being satisfied with such standards.

The next point I want to take up is the question of the wire. The Institution's specification insists that the wire used shall be not less than so many megohms per mile. The number may be anything between 1,200 and 300 megohms (sometimes up to 2,500 for rubber-covered cables) per mile, all sorts of numbers, many of them fairly high, being specified for the different cases. It is usually ruled that the insulation resistance per mile shall be tested in a particular way, namely after 24 hours' immersion in water at 60° F., with one minute electrification; but I should like to know whether consulting engineers are in the habit of testing any of the wire that is supplied to their specification, and if so, with what result? I have, and have had for some years past, the good fortune to have at my command a well-equipped electrical laboratory; and I have made a point of formulating a specification of great detail, and of endeavouring to see that I got what I asked for, by cutting off samples from coils of wire and properly testing them. Without going into details, I can give you some of the results, taken at random, in the case of wires supplied by contractors to exactly the same specification of potential difference, temperature, time of electrification, and minimum insulation resistance.

One wire showed $\frac{1}{100}$th, another $\frac{1}{100000}$th of the specified insulation. A third result is so interesting that I will give you the actual numbers. It was a twin flexible cord, vulcanised, supplied two weeks ago by one of the firms with a high reputation in London, and was specified to have an insulation resistance of 300 megohms per mile, with one minute electrification after being in water at 60° for 24 hours. The number 300 was specified because, being only flexible wire, it could not be expected to have the resistance looked for in the case of a thick cable. One of the wires in the sample, however, was found to have an insulation resistance, not of 300 *megohms*,

but of 23,700 *ohms* per mile, whilst the other wire showed only 1,000 ohms per mile! The result appeared to me to be so ridiculous, that at first I thought some mistake must have been made; but, on inquiry, I found that it was correct, and that the resistance between the copper wire and the water could be tested even with an ordinary Wheatestone's bridge. A very obvious suggestion was that, as both wires were bad, they might have been cut or damaged, and that it would be very unfair, therefore, to condemn the sample. Each wire in a long coil was then cut into several pieces, and each piece was separately tested, but all sections were practically bad, showing that there was a general leak. Finally, we examined the wire under a microscope, but failed to detect any evidence of mechanical damage. To-day this wire has only 200 ohms per mile insulation resistance. Professor Ayrton.

I do not know whether other electrical engineers have opportunities of testing, or whether they are aware of the sort of material that is being sent out. It may, of course, be said that such high standards are not required; but that is nothing to do with the question. A man who wanted to buy a gold watch, and, having paid for it, was given a nickel watch instead, would not be satisfied with the remark that that would answer his purpose equally well. It is for the engineer to judge whether he wants such wire; but if he pays for it he ought to get it. But it may be urged that he is asking an impossibility, and that the contractor is unable to supply such material. I have here, however, a wire eight times as good as my specification, showing that there is nothing impossible about it. It simply means that absolute rubbish is being supplied, at any rate to me.

It may be interesting, although a little apart from the present subject, to state at what point the wire last alluded to breaks down. Our Chairman asked, some years ago, whether there was any flexible wire in the market suitable for 200 volts. This particular wire stood a strain of 9,800 volts of half a minute application before it broke down, and, when tested in water at 60° F. after one minute's electrification at 110 volts, had an insulation resistance of 2,400 megohms per mile when only 300 megohms were asked for. I mention this because it seems to me that possibly such numbers as 1,000 megohms, which were specified in the rules quoted in the paper, are never obtained at all, and that they are as nominal as the standards of insulation resistance per lamp.

Mr. J. S. RAWORTH: Two of the papers before us are extremely mild and pleasant ones, on a subject to which we have been used for many years past; but that which has been brought forward by my friend, Mr. Wordingham, contains a proposition that is altogether new, and that I will venture to say, if he had been brought up carefully under the training of this Institution, would never have occurred to his mind. But he has been to Manchester, that worst of all schools of municipalisation, where they persuade their ratepayers by some extraordinary effort of the imagination to believe that what they do is all for their benefit. I suggest that if Mr. Wordingham's ideas are carried out, and they are adopted by other municipal engineers, they will make this little island of ours more of a purgatory than it is at present. They will obtain powers to enter our houses on almost all occasions, Mr. Raworth.

Mr. Raworth.

and will be able to dictate to us exactly what kind of electrical fittings we shall use and what we shall not.

Now I hold strong views on this question, and have given it very great attention, for I was the first to introduce the fuse cut-out for the purposes of glow-lamps in this country, and was also the first to take the great trouble necessary to get switches and fuses made with porcelain bases. Consider what was the state of the electrical industry in the year 1881, and from that time until the year when Mr. Musgrave Heaphy's rules appeared. You know that the electric fittings and the wires that we had in those days contained in themselves all the elements for a fire, because we had not then learned how to make them, and they were naturally bad. But the conditions of the industry have entirely changed since then, and electrical fittings are as well made as gas fittings. Electrical manufacturers generally have, on their own initiative, done all that was possible to make their fittings safe, and now it is almost impossible for a careless wire-man to leave odd strands of wire projecting out of the terminal of the binding-screw, thereby making it possible for a short circuit to be set up. In the face of this, how is it that electrical people are, of all others, alone in running down their own trade? Has any one ever heard of the Institute of Gas Engineers considering fire rules for gas supply? I will put to you this question: Among the very worst of bad and dangerous electrical fittings to be bought in any shop in London or the provinces, have we anything so dangerous as a water-slide gasalier, or as a swivel-arm bracket standing out from the centre-post of a window between two lace curtains? Now, you have not to get the permission of a fire-office inspector to use either of those contrivances. You can put a swivel-arm bracket from your window between two lace curtains without using any glass-shade upon it whatever, and can leave it in such a position that a servant can move it to one side or the other and set the lace curtains on fire; and yet you have not on that account to pay any extra premium for fire insurance.

We all know perfectly well that the very worst electric light installation in the world is enormously safer than the very best of gas installations. Let me refer you to one of the worst installations of electric light, namely, that in Cardiff in the year 1886. There we had a very large number of the shops lighted up from 2,000-volt brush machines with the lamps distributed on the group-series system. Two thousand volts were used in the shop windows, and wire of a kind, perhaps rather worse than Professor Ayrton has described, was wrapped around the brass rods which held the fittings in the windows, and the lamps were attached to the ends of them. There were, in many cases, something like 2,000 volts between those wires and earth, yet I never heard of an accident. Then at the Exhibitions, when Mr. Musgrave Heaphy appeared on the scene, we had every imaginable condition for making a first-class conflagration—wires which were, in many cases, only insulated with tape or braiding, with a little tar on the outside; wooden-based switches; and lamps which had very little protection against falling fragments of carbon. But we have never burned down any of these Exhibitions, although I always thought we should. That

these Exhibitions escaped is evidence of how wonderfully safe electric light is. Mr. Raworth.

The question upon which I would ask you to bring your common sense to bear is this: Do we want Mr. Wordingham to extend the use of his rules over the whole of the country; forcing us to have inspection of all our premises and all our fittings? You think, perhaps, it is a small thing that our forefathers died for the liberty of the English nation against the oppressive power of the Government; but the whole thing is being enacted over again by the Municipalities. They are getting, by degrees, powers over us which we do not recognise at the moment, but which we shall understand only too well later on.

Mr. L. B. ATKINSON: Mr. Raworth has already to some extent covered the ground on which I intended to touch. I do not propose critically to examine any of the Rules set before us, for I think, with Mr. Raworth, that there is a much broader point involved. The papers, more particularly that of Mr. Pigg, illustrate the chaotic state into which the multiplication of rules has brought us; and I think the time has now come to ask when and where this making of rules is to finish. Mr. Atkinson.

There are, broadly, three series of rules to which manufacturers and contractors are now being subjected. There are the Institution Rules, which serve mainly as a guide as to the best practice; they are not rules which anybody is bound to accept or reject. Then there are the Fire Office Rules, which are avowedly in the interests only of a particular class of traders, the Fire Underwriters; they are not necessarily at all in the interests of the consumer, and, in fact, may conflict with them. And then there are the Supply Company or Corporation Rules, which affect themselves and relate to the necessities of the system, and which affect (or should affect) the system only.

With regard to the Institution rules, I think it is a very admirable thing that they have been formulated as representing a consensus of engineering opinion on what is, at all events, perfectly safe practice. But let us take them to go no further than that.

Then we come to the rules of the fire insurance offices. The position of these offices is only really strong in as far as, generally speaking, a man has made up his mind beforehand in what office he will insure. He may have already effected the policy, and he then has his installation put in and asks the insurance company to pass it. In my view, however, the insurer should go the other way to work. I recently had to place a policy of fifty thousand pounds on manufacturing premises in which there was an installation of some years' standing which defied every rule. The insurance offices were asked to quote their rates, and all, with one exception, said that the installation must be altered in every possible direction. I told them that I should alter nothing, and it was a question of which quoted the best rate. In the end, they all quoted the same rate as if I had altered the installation. I admit that the insurance companies have very properly a right to exercise some supervision over the risks they take. In certain premises with which I am acquainted, fires, due to boiling tallow, may start about twice a week; but although I have pointed out this risk, no attention is

Mr. Atkinson.

paid to it by the offices. It is always the electrical installation that must be singled out for supervision.

We now come to the rules made by the supply companies. These, with few exceptions, relate more particularly to insulation. No doubt where the installation is directly connected with the mains, they ought to interfere in order to facilitate the testing and localisation of their own faults. But if they supply through a transformer, placed on the consumer's premises, I cannot see what the insulation on those premises has to do with the supplier. Even if defective insulation caused a transformer to break down, that means the supply company has a defective apparatus and that the consumer has to take special precautions, not for his own benefit, but to guard their defective apparatus. Then, again, I think that to some extent the supply companies and corporations are entitled to make regulations with regard to motor loads. There may be a maximum rate at which the current should be put on; but that again is, after all, only because they are not in a position to give a sufficiently regulated supply. But when they specify how fittings are to be constructed and tested, they are, I think, altogether passing the bounds of reason.

It has been argued that a disaster occurring with electric light due to defective fittings has a prejudicial effect on the use of the light. But people know by now that accidents happen, and that they are generally due to their own carelessness. The stage is long since past when the use of electric light could be dependent on whether a man burns out a cut-out or anything of that kind. The case has been cited of testing water fittings; but water is mostly supplied on a rental system, and therefore it is right that the supplier should be able to see that it is not wasted; but where water is supplied through a meter, I never heard of the company checking leakage. I have known 500,000 gallons per quarter to leak away through a meter, without an objection being raised by the supplying parties. So with gas, I have known a thousand feet a day to be leaked for the space of three months, and to pass unobserved through a ventilator into the chimney; but the gas company did not object. Why, then, should the suppliers have the right to interfere in the case of leakage of electricity, so long as it does not upset the conditions which enable them to test properly? The fact is, the industry is being strangled by these attempts to regulate it. We have the consulting engineers, we have the fire offices; and now we are threatened with the municipal engineers, all with regulations, none of them fitting one another. It renders it impossible to make standard articles anywhere, as dynamos, engines, and all the smaller fittings have to be altered accordingly. Further than this, we are face to face with the fact that municipalities are now asking for powers to become our trade competitors, and what safeguard is there that they will not specify that the only thing to be used is what they sell? I think the time has come when, with no uncertain voice, the trade should speak out, and have done with the specifying of particular forms of apparatus to be used, or even to be approved, before the municipality will put the current on from a supply which does not belong to them, but which belongs to the ratepayers—that is, the consumers.

Mr. G. H. COTTAM: Professor Ayrton has alluded to the Hampstead wiring rules. In reference to these, I should like to say, in commencement, that there is a printer's error in the passage relating to cutting off the supply if the insulation drop below 500 ohms; this should be 5,000 ohms. I am not responsible for the drafting of these rules, which were there before my time; but I have seen no reason to alter them. May I ask Professor Ayrton why we should lower our test, when, with good work and materials in dry buildings, it can easily be got? We have to make allowance for damp buildings; and if we adopted a lower test, and then had to make allowance, the consumer might not get any light at all, and still might have an appreciable registration on his meter. Sooner or later, insulation will deteriorate; and the common-sense view is that the better the work, the materials, and the test, the longer will the consumer be free from the trouble of a breakdown. To lower them is, in my mind, like unlocking the stable door to allow the horse to escape. I remember Sir Henry Mance, when in the chair, emphasising the necessity of engineers seeing that their cables and wiring work were in good order, or they would give trouble in the future. Mr. Cottam.

I think the fire insurance companies may well come into line with the Institution as to regulations, as, although I speak subject to correction, I cannot conceive, even if the fuses are in bad order, how a fire could arise, provided that the engineer's test is correct. Some years ago I was called in to examine the wiring of some premises where, contrary to rules, the consumer had added on lights without any previous notice, and where also there was no main switch. Imagine my surprise when I left the building to read on his door, "Agent to the Phœnix Fire Office"!

There is one matter to which I should like to allude. I believe that ceiling-rose fuses are in many places, but not universally, abolished. It may be a matter of considerable inconvenience for a ceiling fuse to go, through some slight fault of a lamp-holder or a lamp, as it may necessitate the disarranging of a whole table of guests to put it right, whereas had it been properly fused on the wall there would have been no trouble whatever.

I do not know whether Professor Ayrton, as he remarked upon our high insulation test, will allow me to give him a courteous reminder that I could mention an establishment connected with our own mains where he was consulting engineer, and where we might have passed the work, but he would not. I envy a station engineer like Mr. Wordingham, who has the opportunity and time to draw up, in such detail, rules and regulations with regard to his supply. But I hope the Institution will not take these as a guide. I should much prefer some more simple and concise way, so that the wireman and contractor can find time to read them. I have made many attempts to read them through, but I have been so often interrupted that I have never got to the end. But what I have noticed reminds me rather of grandmotherly Germany or autocratic Russia than of freeborn England.

Professor AYRTON: I would like, sir, just to correct an impression which I seem to have produced at the last meeting, but which I thought Professor Ayrton.

Professor Ayrton.

I had already succeeded in correcting. Again I would say I am not in favour of lowering the standard to that which is adopted by the Hampstead Company. I had hoped that what I said about wires and the standard of insulation of flexible wire was proof of that. But to make it absolutely certain that I am in favour of high standards of insulation, I shall be happy to send Mr. Cottam, after this meeting, a specification which I drew up for a large building wholly self-contained, having dynamos and steam engines, not connected with any supply company, in which the requirements as regards insulation resistance are higher than anything specified by the Hampstead Company. I think that is sufficient proof that I strongly support Mr. Cottam and other engineers in adopting and maintaining high standards. What I am surprised at is that other companies should take such low standards.

Mr. Andrews.

Mr. J. D. F. Andrews: It appears to me that if this Institution can get the rules into its own hands, it may ultimately, by superseding the great number of others in existence, relieve us to a very great extent of all rules. If we are to have any rules at all, I think they should be rather advisory than, as suggested by Mr. Crompton, compulsory. They might certainly be very valuable in such a way, but they should not deal with details, as, in the present state of the electrical industry, it would be almost impossible to devise rules which could control or regulate details. For instance, in one of the Institution rules, No. 14 wire is suggested as the thickest wire that should be used as a single strand. I have for a long time been accustomed to use No. 12 as the largest size, and with very considerable advantage. I find, in fact, that if we use stranded wires in armoured conductors which have to be drawn into buildings and twisted about, there is a chance of the strands breaking and touching the armouring; and we have constantly been obliged, in order to secure freedom from short circuits, to use solid wires as far as possible. But above No. 12 wire we have found it necessary, on account of the liability to destroy insulation, to use strands.

Now, with regard to earthing, I think there is a general feeling that the time has arrived when this is possible. I recognise the magnitude of the difficulties that are to be met with, but believe those difficulties can be surmounted; and I think conditions can be devised whereby earthing can be instituted with perfect safety. The objections to earthing are chiefly connected with electrolysis, but there is another perhaps connected with the difficulty of making a change. Electrolysis, however, is doubtless the most important, because the amount of current to be conveyed in a practically uninsulated conductor might be very considerable. If, however, the conductor is earthed at a sufficient number of points along its length, say at each house connection, the pressure at each point would be so low that the electrolysis would be practically nil. Another way of guarding against electrolysis would be to expose the conductor to earth, not as a whole, but only at certain points, say at each house connection; and there to expose it in such a way that if any electrolysis should occur it will take place on a surface at which it can do no harm. One method might be to use an iron earth-plate. I suggest iron, because by using

copper we should have galvanic action set up between the pipes or other articles and the earth-plate. The earth-plate might with benefit be somewhat coated, so as to avoid electrolysis. Of course the most perfect way of earthing would be to adopt an earthed concentric system throughout, with the outer conductor but little insulated. The advantages of earthing are very great. With this system every house would practically be self-testing, instead of having to be occasionally tested by experts, and consequently the working expenses of supply superintendence could be reduced. The materials of wiring would be considerably reduced in cost and also much improved. Above all, there would be a possibility of introducing on a large scale the concentric system which has some very obvious advantages over all other systems of wiring. Mr. Andrews.

Mr. S. MAVOR: I think that there is no reason whatever why a code of rules drawn up by the Institution should not be adopted by both the fire insurance companies and the municipalities; if such rules were revised in consultation with representatives of the fire offices and of the municipalities, they would bear an authority which would go far to ensure their general adoption. Mr. Mavor.

Mr. Wordingham very truly says that when engineers have drawn up rules of their own they are very loath to set them aside in favour of suggestions from outside. At the same time he advocates that municipalities all over the country should obtain compulsory powers for enforcing their rules. The prospect opened up is frightful; the engineer to every little municipality would then draw up his own set of rules, and he would have powers to enforce them.

With regard to the insurance companies and this question of megohms, I think the insurance companies neither know nor care about megohms, for I have never seen an insurance inspector with a testing set in his hand. The insurance companies send to my firm the ordinary schedule to be filled up, and they trust to us to fill it up truly. That being so, and as they take no proper steps to see that their schedules of rules are followed out, they might just as well set them aside in favour of the rules drawn up by the Institution, which really cover all their requirements. Among certain rules recently issued by the Tariff Committee, was one relating to the enclosing of motors; but the rule, as it now stands, is a very bad one. It provides that in certain classes of buildings, such as spinning mills, woollen mills, &c., all motors must be entirely enclosed. Now I have a case at present where a 60-h.p. motor is to be put on the floor of a factory. This motor is set on a concrete floor, and is surrounded at a distance of 6 ft. by a glass and wood partition, so that it is entirely isolated; yet the insurance companies insist that an iron box shall be put round it so that it is converted into an oven; although neither they nor any of the other tariff offices appear to be able to explain the nature of the risk they anticipate, nor why the motor should be so enclosed.

The rule is most vexatious, but no one seems to have any direct responsibility; the offices individually have no power to relax the rule. I hold that any committee of fire insurance offices should consult with the Institution of Electrical Engineers before framing or passing such a

Mr. Mavor. rule as that. It is of very great importance that more attention should be given to the switching arrangement, resistances, and appliances in connection with motors than to the enclosing of the motors themselves. Where motors are enclosed, it is nearly always to protect them from dust or damp or dirt, and not because there is any appreciable risk of fire.

With regard to the question of megohms, every one knows that in a dry house one may have 100 megohms between the grate and the water-pipe, and that an installation may be ruinously bad and still give a ridiculously high insulation resistance, whilst another may be a perfectly sound job and give a lower insulation resistance than is allowed for by any of the rules.

Mr. Shoolbred. Mr. J. N. SHOOLBRED: I should like to refer to one or two of the points raised in these papers, and in the first place to the Insulation Test. The appendix of Mr. Pigg's paper shows what an immense diversity there is in these various tests. I think that, although the question of basing an insulation test upon lamps or points answered very well at first, it is now proving itself fallacious, because when it comes to a question of motors, or of current and energy, it fails completely. My attention was drawn many years ago to this fact when discussing with Major Cardew a series of by-laws, intended for the Bradford Installation, for the approval of the Board of Trade. The insulation test that he suggested, and which I was glad to use, has been adopted at various places. It is based upon the current and the demand by the customer, and takes into consideration the energy; and not merely the number of lamps or points used. A rule based on the energy is much more useful than one simply based on lamps or points. The suggestion of Major Cardew, when the installation-pressure was about 100 volts, was ten megohms divided by the number of amperes as demanded by the customer. Later, when 220 volts or thereabouts became the ruling pressure, it was raised to 20 megohms. At each of these pressures the formula has been used satisfactorily. Mr. Pigg, in the appendix to his paper, quotes several towns in which it is employed, while to my own knowledge there are several more places where it has been adopted. Therefore I would urge that the question should be considered much more fully. It will be seen that the rule laid down by the Institution is 10 megohms, divided by the maximum number of amperes required. The insulation test should not be a question of points only.

Attention has been drawn to-night to the question of wiring rules and supply regulations. These two subjects differ considerably, and distinction should be made between them. Supply regulations should not be complicated, as is often the case, by mixing them up with wiring rules. They are two totally distinct matters affecting two totally distinct classes of people; and generally the risks are very different. The wiring rules affect especially the insurance companies since they relate to fire-risk on the premises, while the supply regulations are really intended to safeguard the installation *outside*. In dealing with supply regulations, therefore, we should avoid complicating them with references to questions relating to the interior, which really do not seriously affect the supply.

Mr. Crompton has referred to the compulsory use of the Institution rules. I may say that it has always been my endeavour to bring this about in supply regulations. As I foresaw, many years ago, this has been the general tendency, and I have found that it is by no means a disadvantage. Indeed, I find that to do so tends to far more harmonious working with the fire insurance companies themselves. I was very happy to hear representatives of the fire insurance companies express a wish to co-operate with this Institution with regard to their rules. Mr. Shoolbred.

I heartily agree with the suggestion which has been made to seek, on certain points, the practical opinion of makers of fittings, since they are more intimately acquainted with various minor questions of construction, which may be very well worth consideration.

Mr. Andrews has referred to the question of earthing. I think the regulation that has lately been issued by the Board of Trade, with regard to the earthing of the middle wire at the central station, will be found to have a considerable bearing, later on, in various wiring rules, and it may possibly bring about the advantage which Mr. Andrews speaks of.

Mr. A. A. C. SWINTON : In what I had intended to say I have been anticipated by Mr. Raworth and Mr. Atkinson. I have given some attention to the subject under discussion, and it appears to me that, excepting in the case of Manchester, where, a Bill having been smuggled through Parliament, the municipality is apparently trying to enforce atrocious conditions of management which infringe the liberty of the subject, nearly all the rules that are made by municipalities and supply companies are *ultra vires*. They would not stand in a court of law for a moment. So far as I understand, all the municipality can do without special powers such as they have in Manchester, is to enforce such regulations as will ensure that the arrangements on the consumer's premises do not interfere with the supply. So long as that is so, I do not think the supply company or the municipal authorities can have anything to say in the matter. Of course, a different question is raised in the case of the insurance companies. My experience coincides with that of Mr. Mavor and Mr. Atkinson, for I have usually found that when an insurance company raises an objection to any particular point, one has only to mention that there are other insurance companies, to make them agree to what is wanted, provided, of course, it is reasonable. Mr. Swinton.

Mr. CHARLES BRIGHT, F.R.S.E. : With regard to the futility or otherwise of a high insulation resistance for electric light mains, there is one point which has not, I think, been referred to in the course of the discussion, and which has, I venture to suggest, a considerable bearing on the matter ; I refer to the problem of durability and maintenance. In submarine telegraphy we are in the custom of stipulating for a dielectric resistance enormously higher than that which is necessitated for the purposes of the case.[1] This is so, owing to the fact that the two Mr. Bright.

[1] Indeed, a much lower resistance would—if it could be relied upon—more closely realise the theoretical requirements of high-speed signalling on a long cable.

Mr. Bright. materials which have given any measure of success as insulators for submarine work are known to have such a resistance. These materials —gutta percha and vulcanised indiarubber—are the only two insulators that meet the requirements in the way of durability, they alone withstanding the water and the enormous pressure entailed. Now, though it is very difficult to detect variations in these gums when made up into core, by mere sight—and chemical analysis is also unsatisfactory in this instance—we know that an electrical test will reveal them; and thus the resistance specified in submarine cable contracts is always based on the above circumstances—*i.e.*, in order to ensure that the material is the material specified, and not something else. Similarly, it is principally on the same grounds, I presume, that electric light supply companies—in order to ensure their mains standing the test of time—stipulate for a fixed insulation resistance such as the material decided on is known to possess if of a durable character. It is surely far more on this account than from considerations of limiting the leakage; though I venture to think that even this, from the general public point of view—considering the vast lengths of neighbouring telegraph and telephone lines—is of more importance than Mr. Atkinson seems to hint when speaking of it in the same breath with leakage in the supply of water. So far as concerns the actual number of megohms to be determined on for the specific value of the dielectric—with a given thickness to meet the mechanical requirements—I would suggest that this is a question which should be governed almost entirely by what the material decided on—a really durable material, as a *sine quâ non*—is found to afford; and that this standard should in fact be only given as a means of assurance that the material specified is used. I would go further and suggest that, before drawing up any set rules or any specification, tests should be made from time to time on samples of the most durable materials in the market, and that the specific resistance stipulation should be based on these tests. It should be remembered that almost any specific resistance can be obtained—temporarily, at any rate—by expert manufacturers with all manner of mixtures and compounds; thus, very often an abnormally high resistance is as bad a sign as an abnormally low one.

The fact is that specific resistance *per se* serves but poorly as an indication of good and lasting qualities such as are required for a serviceable dielectric; moreover, tests on short lengths are often, at the best, unsatisfactory. What is of infinitely greater value is a prolonged insulation test by direct deflection, such as shows whether the rate of absorption (or electrification) is regular, or otherwise. A sound and durable material invariably electrifies regularly, and the reverse is the case with a material that is not homogeneous or durable—no matter how high its specific resistance may be; indeed, very frequently the latter will not stand the electric strain and breaks down altogether, after a comparatively short period of battery application. Mr. Cottam has quoted Sir Henry Mance in regard to this subject; and I have no doubt when Sir Henry warned us to keep an eye on our electric light mains he pointed to the importance of selecting something that would last and not give endless trouble in the way of maintenance and replacement. These remarks are mainly directed to the discussion; but the

papers are, of course, full of interest, and it appears to me that Mr. Wordingham's suggestions demand very full consideration. Any measures that will bring about a general and uniform understanding should meet with approval. Mr. Fright.

Mr. A. WRIGHT: I should like to say a few words from the point of view of the central station engineer, who does not have rules, and does not want them. We can quite appreciate the work that has been done by this Institution and by Mr. Wordingham in drawing up rules, which we can recommend when called upon to do so. But a great many of us do not consider it to be in our province to enforce rules which belong entirely to the fire offices. We have certain things to look after, and certain regulations to make to see that our conditions of supply are not interfered with. For instance, we have to see that the pressure remains fairly constant; that no user shall so take current intermittently as to interfere with our pressure; that the general leakage should not be very great—although the permissible amount of such leakage is very difficult to define. But here I think our province ends, and that of the fire inspector commences. It is not our province to make fire rules; and I hope that it will not be imagined that many central station engineers have these autocratic notions of our duties. I can assure you a great many of us think it our duty to get as much business as we can, to help the wiring contractors to the utmost, and to keep our pressure constant. Why should we take up the duties of fire inspectors? They are very highly-paid officials; we are the reverse. They send down a form to be filled up; why should we ask our staff to go into that business and help them? They draw up a form of rules to satisfy old ladies, but they do not consider it necessary to be very strict. It is useful to have rules to present to old ladies, but it is not necessary for us to insist on their adoption; indeed I think such rules are apt to interfere with our business and to frighten the public. The public may think that electricity must be very dangerous if it requires thirty-seven or thirty-eight pages of rules to make it safer than gas. I really think that if central station engineers have their plant, and sufficient of it, they had better devote their time to keeping up their pressure; and if they have not the plant, they should devote their time to getting it. Fire rules can be recommended, but not enforced, by central station engineers. To enforce them would be a suicidal policy. It is not their province to do so, and it is a form of autocracy which ought to be stamped out as soon as possible. Mr. Wright.

We work under severe restrictions. Suppose that for every drop in pressure below 4 % we were fined £10, and for every shut down we were fined the £100 named in the Act, where should we be? Imagine us working under the Board of Trade rules, rigidly enforced; we should not live another ten minutes.

I will not now discuss the very important matter of earthing, as I hope some day this session to get a paper on that, but also because I think it might lead the discussion to-night on to a side track. The most important point of this discussion, to my mind, is the unwisdom of the supply companies' rules.

Mr. W. M. MORDEY: I want to ask Mr. Wordingham one question. Mr. Mordey.

Mr. Mordey. I believe Manchester has a municipal gas supply; it also has a municipal electric supply. Gas is dangerous. Electricity is safe. Why should Manchester have all these electric supply rules? I understand that the only restriction in Manchester in the case of the gas supply is that people must employ an authorised plumber. Well, we all know what an authorised plumber is.

Mr. Evershed. Mr. S. EVERSHED: I want to endorse what I take to be the common-sense views of the matter under discussion put forward by Mr. Raworth, Mr. Atkinson, and other speakers this evening. I scarcely hope we shall convince any of the younger central station engineers that we are right, but we may be able to do so by a parable. A few months ago I was asked to re-hang a picture in a house. The house was an old one, and I presume the nail on which the picture was hanging had been there for some years. I pulled out the nail to drive it in elsewhere, but was nearly blown away by the gas that came out of the hole. The nail had been for many years in the composition pipe in the plaster. Well, that house is insured in the Phœnix Fire Office, and was passed by an official who is, I suppose, the Mr. Musgrave Heaphy of the gas department. I put the nail in again, and I got a new nail and put it in further up. I believe that that house is still insured in the Phœnix Fire Office, and that any fire office would accept the risk.

Gas was introduced ninety years ago into London, and other large towns. If there had then been an Institution of Gas Engineers, and if there had been rules analogous to our wiring rules and our station engineers' rules, I do not think there would now be a single prosperous gas company in this country.

Mr. Berry. Mr. H. H. BERRY: I would like to say a few words from the manufacturer's point of view. It was with great pleasure that I contemplated the probability of some concerted action being taken by the resident and central station engineers in the matter of rules. But other views which have been expressed have opened up to me that the matter should be dropped by central station engineers altogether. Professor Ayrton, in his criticism, has thrown down the gauntlet to fire insurance companies with regard to the inconsistencies in their wiring regulations, and I suggest that he would have had the full sympathy of at least the manufacturing contingent present at this meeting if he had embodied in his challenge a great many of our central station engineers as well. In this connection I wish particularly to refer to the conflicting ideas prevailing at the present moment with regard to the efficient design of all electrical accessories, from the switchboard to the ceiling rose. It has been my painful privilege during the last twelve months to visit almost every city in this country that can boast a supply of electrical energy, and I have met with a very different reception in almost every town. The views of the resident engineers with regard to the efficient design of these accessories are very different, and of many examples I should like to take just one, namely, the fuse-boards which are now becoming so extensively used throughout this country. Many engineers in the extreme north specify that the length of break of the fuse should be 2½ inches; others specify that it should be 2 inches; others, again,

that it should be 1½ inches, and still others specify that it must be not less than ¾ inch. Some engineers specify that these boards must have the opposite poles in separate boxes placed 3 or 4 inches apart; others specify that they may be placed in the same box, provided the poles are divided by some suitable insulating material. I suggest that the manufacturer, as matters stand at present, is between the devil and the deep sea. He cannot possibly stock the whole of the apparatus that is required to conform with these many different regulations. There is, no doubt, some mean which would answer the purpose, and which, if generally adopted by the manufacturer, would bring down the cost very materially. Now, it occurred to me, when I intended to speak, to support Mr. Wordingham very strongly, and to advocate that this Institution should permit some electrical testing at the Laboratory with a view to having consistent views upon this very important subject from the manufacturers' point of view; but since hearing Mr. Raworth's speech my views have altered. It appears to me that the same object might be attained if the resident engineers dropped this matter. I believe the British manufacturer is capable of guiding his interests, and would see that the proper article is placed upon the market. If the matter were left to him, patterns would be standardised, the cost of production lessened, and the progress of the whole electrical industry materially accelerated. Mr. Berry.

Mr. J. SHEPPARD [*communicated*]: In response to the request of the Chairman and Mr. Crompton at the meeting of the Institution on the 26th of January, that Fire Insurance Surveyors should offer suggestions with regard to the above rules, I submit the following remarks, as an unofficial expression of my own views on the subject. Mr. Sheppard.

The Institution Rules are only applicable in the "ordinary cases of dwelling-houses, offices, and business premises." This should be prominently stated on the cover and title-page, a note being added to the effect that in premises where trade processes or manufacture is carried on, additional precautions may be necessary, especially for motors which in many instances, where they are not placed in a fire-resisting enclosure, should be entirely enclosed with a metal casing forming a part of the construction of the motor, all driving pullies being outside such casing.

Section 2 of the Rules. The rule requiring the outer conductor of concentric wiring to be insulated throughout is somewhat stringent, and a modification of this requirement appears desirable.

The use of unprotected metal staples, so frequently adopted with disastrous consequences, for the fixing of twin flexible cord conductors, between a wall plug and a fitting or portable lamp should be forbidden. Plug connectors with their flexible conductors form weak points in many installations that are otherwise satisfactory.

Section 4. The framers of the rules appear to despise wood casing, and they are no doubt right; but in a great number of installations wood casings are used, and their use is likely to be indefinitely continued.

A rule defining an approved wood casing and capping, and the mode of fixing the same, should be added.

Section 5. Rule 5 states that conductors should not be placed near gas-pipes; to this should be added, "or immediately below water-pipes,"

Mr. Sheppard. the condensation on which is frequently sufficient to cause a drip, and the falling of this on conductors or their casing would be very objectionable.

Section 7. Frames of resistances, in addition to being of incombustible material, should also be at least 12 inches distant from combustible material.

Some regulation applicable to heating appliances should be inserted.

Section 8. Main switches are not used as frequently as is desirable, this is probably owing to some of the lamps controlled by such switches being required during the night.

The provision of a separate pair of leads for the supply of any lamps that may be needed during some part of each night, with separate main switches and fuses, should be recommended. This would allow of the chief main switch being used every night, so that the leads throughout a greater part of the installation would not be constantly charged.

Section 10. The minimum distance allowable between the terminals of fuses and switches for currents at different pressures should be stated.

The percentage above the normal current at which fuses must break the circuit should also be stated.

Section 13. It is desirable to make provision for the cutting off of current from the outside of fire-proof enclosures containing transformers and other appliances.

Section 14. Incandescent lamps and their holders should be kept free from shades, curtains or other combustible material. The omission to observe this precaution has been the cause of many serious fires. A rule with regard to this is necessary.

The use of celluloid about electric lamps and fittings should be forbidden. The extensive use of this material (the inflammable nature of which is either not suspected or not kept in mind) for numerous purposes in all kinds of positions may partly account for the large increase in the number of fires notwithstanding the introduction of electric light.

The maximum current allowable on conductors of different size given in column 4 of the table is understood to apply only to cases where the external temperature never reaches more than 100° Fahr.; but this is not made quite clear in the explanation of the table.

The absence of an Index to the rules may have had some influence in preventing their more general adoption. Even with the grouping described on page 2 it takes time to find the exact item required; a full and complete Index should therefore be added. The supply companies and fire offices freely give copies of their rules to all who ask for them. The Institution of Electrical Engineers should do so likewise.

The general adoption of one Code of Rules would be a great convenience to all interested in electrical energy, including the fire offices. When the majority of electrical engineers come to a definite agreement on this point, a majority of the fire offices will no doubt quickly follow. In any case there would be no difficulty with the fire insurance on premises installed in accordance with the Institution Rules with the foregoing modifications and additions.

During the discussion on electric wiring in 1896, the registration of reliable wiremen was suggested, but a list of electrical engineers and wiring contractors undertaking to follow the Institution Rules would be equally beneficial and most useful to the fire offices. Mr. Sheppard.

The confident expectation that the use of electric light would greatly reduce the number of fires has not been realised ; this is as disappointing to the fire offices as to electrical engineers. It is possible that conditions distinct from the use of electrical energy have contributed to this increase, but the fact remains that the number of fires has increased, especially in London, in spite of, if not in consequence of, the introduction of electric light.

Electrical fires are more frequent than is generally admitted, and no doubt a full proportion of the numerous fires the cause of which is not discovered, result from defective electrical installation.

Some particulars of fires in the County of London, compiled from the official reports of the Metropolitan Fire Brigade, are annexed, showing the increase in the number of fires referred to, together with the proportion of fires attributed to different causes connected with artificial light ; to which is added the average number of fires per annum over different periods, in streets where electric light is used in almost every building.

Total Number of Fires per Annum Included in the Reports of the Metropolitan Fire Brigade.

Years ...	1866-70	1871-5.	1876-80.	1881-5.	1886-90.	1891-5.	1896-8.
Total average number of fires per annum during each of the periods named ...	1,584	1,507	1,683	2,124	2,278	3,228	3,567
Proportion of above totals ascribed to the undermentioned causes connected with artificial light :—	%	%	%	%	%	%	%
Candles	13·3	12·9	8·0	6·2	6·0	5·9	6·7
Gas	8·6	8·8	9·2	8·8	8·7	7·7	8·1
Lamps, oil and spirit only ...	1·6	3·9	5·6	8·78	10·74	13·2	11·5
Electric light	—	—	—	0·02	0·06	0·1	0·2
	23·5	25·6	22·8	23·8	25·5	26·9	26·5
Lighted taper, light thrown down, lucifers	7·3	10·8	11·3	12·2	11·5	12·5	14·5
Other causes { Known ...	38·9	45·2	43·6	36·9	33·0	31·8	30·8
Other causes { Unknown ...	30·3	18·4	22·3	27·1	30·0	28·8	28·2
	100·	100·	100·	100·	100·	100·	100·

A continuation of the Statement of the total number of fires in the

Mr. Sheppard.

following streets, where electric light is more extensively used than in other parts of London, given in Vol. xxv. of this Journal, p. 158, is annexed :—

Cheapside, St. Paul's Churchyard, Ludgate Hill, Fleet Street, Strand, Charing Cross, Cockspur Street, Pall Mall, Waterloo Place, Regent Street, Oxford Street, New Oxford Street, High Holborn, Holborn, Holborn Viaduct, Newgate Street.

Average total number of fires per annum in buildings on the streets above named		... 1881–85	... 37
Ditto	ditto ...	... 1886–90	... 37
Ditto	ditto ...	... 1891–95	... 46
Ditto	ditto ...	... 1896–98	... 47

Mr. Andrews.

Mr. O. M. ANDREWS [*communicated*] : As a central station engineer, I should like to support the views expressed by Mr. Wright. I would submit that it is no part of the duty of central station engineers to lay down rules for the wiring of buildings, other than those necessary for the safeguarding of the supply, and that it is inexpedient that they should do so. They have all found it necessary to repudiate responsibility for anything on the consumer's premises beyond the supply terminals, but, if they lay down the manner in which the internal work is to be done, such repudiation avails them very little. As a matter of fact, every one concerned in the wiring holds them responsible, for it is assumed that, having laid down rules, they will see them carried out. The contractor pleads the "passing" of the installation to secure payment for his work ; the fire insurance inspector, in many cases, takes the same "passing" as evidence that the work is satisfactory from his point of view ; and the consumer concludes that he has a good installation. But most central station engineers, not having the means of looking into the details of the wiring, and protected by their repudiation of responsibility, do not trouble to see that their rules are carried out ; and so a false sense of security is produced by the mere fact of the issuing of the rules.

If, however, the plan advocated by Mr Wordingham is adopted, and statutory powers are obtained to enforce the rules, then the responsibility of the central station engineer is considerably increased, for I take it that such powers involve statutory obligations to see that all work connected to the mains is in accordance with the rules. To do this properly will involve an increase in the work of the central station staff, which must be paid for, and the cost of which will ultimately fall on the consumer ; while the inspection in what is, after all, a domestic matter, cannot fail to be a source of annoyance to the public. But the most serious objection to this plan is that, if all this inspection is necessary—if, that is to say, the supply of electricity to the public is so dangerous that it must be safeguarded by elaborate regulations as to how the wiring is to be carried out—then central station engineers are not the proper persons to have the enforcing of the rules entrusted to them. Their interest as *suppliers* must sooner or later clash with their duty as inspectors, and they will be tempted to let important consumers down lightly.

As Mr. Wright has well said, central station engineers have plenty to do in looking after their own business, and seeing that the public has what it has a right to demand of them, namely, an efficient supply which it can use when and how it pleases. They have ample power to prevent the improper use of the supply, and I cordially endorse the protests which have been made against any attempt on their part to dictate how wiring work shall be carried out, and especially against their obtaining statutory powers to enforce their rules. Mr. Andrews.

Mr. J. J. STEINITZ [*communicated*] : I think the papers on this subject admirable ; the only unsatisfactory point being that, in my mind, it is questionable if they are wanted. It may be perfectly satisfactory in towns like Manchester, where the demand for current is assured, to lay down laws, and, if I may say so, place stumbling-blocks in the way of intending consumers ; but other towns find each and every fresh rule add to their difficulty in obtaining customers. I do not see why we should care what happens in premises on the consumer's side of the meter—that is, so far as it concerns the electrical engineers who are undertakers. They can protect themselves by carrying out pressure testing to house installations, put double, or any pressure thought necessary, on the installation for a suitable time ; if it stands, pass it ; if it fails, condemn it. As for the fire insurance rules, I wonder who works to them. It is usual to put a clause in wiring specifications that the work is to be done to the satisfaction of the fire insurance companies concerned in the risks. Who looks after this ? Certainly, in my experience, not the insurance company. There is an advantage to be derived if Mr. Crompton's suggestion is adopted ; namely, if one set of rules is adopted, there will be so many the less in existence to awe the intending consumer and to harass the wiring contractor. Mr. Steinitz.

In this instance we might well copy the gas- and water-companies, who seem to leave (and rightly so) the internal fittings to the householder and his plumber. When one thinks of some of the wiring that has been in existence for so many years without any bad or even unfavourable results, and when one also knows that accidents that have happened have been due to causes that no obedience to rules would have obviated, I, for one, think the time has come to do away with rules that frighten the timid (who naturally think, if all these precautions are necessary, what a very dreadful mistake it would be to introduce electricity into one's house) and so help to hamper a rising industry.

Mr. F. G. BAILY [*communicated*] : Wiring rules and specifications betray such an astounding diversity, as has been clearly shown by Mr. Pigg's paper, that a thorough discussion cannot but have a good effect upon current practice even if it be found impossible to embody all the desirable conditions in a set of regulations, and I venture to bring forward a few points which seem to me to require revision. Mr. Baily

In most cases the official insulation test by the supply company or corporation is the only check on the wiring contractors, and hence it is a matter of great importance to the consumer as well as to the supplier of electricity. But I do not think that the present form of test is satisfactory, and it does not act fairly with different methods of wiring.

Mr. Baily. The points at which leakage takes place in an electric lighting installation are :—

1. Lengths of unbroken cable.
2. Joints insulated with rubber tape.
3. Cut ends of cables.
4. Switches, lamp-holders, ceiling-roses, fuse-boards, and distributing boxes.

Leakage through unbroken cables even with the lowest class of insulation is altogether negligible if the cable is not rotten. Giving the liberal allowance of twenty yards of cable per lamp, the insulation resistance per lamp runs to many thousand megohms. Leakage at joints means an inevitable breakdown in the future, and is extremely serious. Leakage over exposed ends back along the outside of the cable may be serious, as the material is combustible and the evil is likely to increase owing to decomposition. All these should be very small if the work is good.

Leakage over the porcelain, in heading 4, makes up by far the greater portion of the leakage of a good installation when it is first tested. It is impossible to prevent, but is really harmless. The insulation test should, if possible, differentiate between the first three headings and the last, as they are of greatly differing importance. But the present test, in my opinion, frequently fails to discover the first three, while it takes the worst conditions for the last, for buildings are usually tested soon after the wiring is completed, when defects in the cables and joints have not had time to develop, and when the plaster is often hardly dry and the porcelain insulators are damp. After the pressure has been on the building for even a few hours, the moisture on the porcelain is driven off and the improvement is most marked. I would recommend, therefore, that the pressure be applied to the installation for a day before the final test is made, all switches being on and no lamps inserted. This would show the porcelain in its true working condition, and would help to increase faults due to bad insulating material. Again, a variation of pressure does not, in my experience, affect the insulation resistance of porcelain, while faulty cables and joints show a rapid deterioration under increasing pressure. Hence I would advocate a testing pressure of 500 volts or even more. Such a test will give a far more satisfactory proof of the lasting insulation of the building, and this is what is really required ; while there is no part of a good installation which is not able to withstand this pressure without damage.

The unreliable nature of the present test is shown by the great effect of the weather on the result obtained, and by the effect of the presence or absence of heating in the building. On a damp day, in a new unoccupied building, where porcelain has been extensively used as an insulator, it is difficult for a contractor to produce a satisfactory result, however sound and reliable his work may be.

Another matter which is unsatisfactory is the pure rubber insulated joint. Its goodness depends entirely upon the good faith and skill of the individual workman, and subsequent examination is impossible. When kept perfectly dry it may be safe, but if any moisture comes on

it, its ruin is only a matter of time. The layers of rubber do not join together. I have opened joints that have been made for years, and the layers have peeled off with ease. A still more fatal joint is the one frequently made in 3- and 5-light fittings. There it is kept at a fairly high temperature by its proximity to the lamps, and the material after a few months becomes completely rotten. If the Institution were to give a strong expression of opinion about the taped joint, it would help to remove one of the chief sources of breakdown in electric light installations. Mr. Baily.

One other point I wish to mention, and that is the bunching of small wires. This is frequently condemned where wood casing is used, while it is freely used and even enjoined with iron pipes. We are told that it is dangerous to put wires of different potential in the same groove. What is the sense of this? Either the insulation of the cable is satisfactory, in which case there is no danger; or it is not, in which case there is certain to be danger. The half-inch wood fillet is surely not supposed to be a material assistance to the double thickness of vulcanised rubber! The most elementary test will soon dispel that notion. The rule is always or almost always disregarded in crossing, or in passing through the back blocks of switches and fittings. Moreover, if there is to be a leak, it is better to have a dead short which blows the little fuse than to have a slow leak which does not blow the fuse, but heats the wood to the smouldering point. There are, I allow, reasons for keeping circuits apart in view of subsequent additions or alterations, but the present superstition causes walls to be covered with unnecessary casings to the despair of the architect and decorator.

I will only add that I have found the rules of the Institution of very great convenience in drawing up specifications, as they save the enumeration of the matters they deal with by a simple reference to their requirements, and I am sure that all consulting engineers will welcome their extension and confirmation.

Mr. H. L. Leach [*communicated*]: We are indebted to the authors of the three papers on "Wiring Rules" for bringing this important subject before us, and any steps which may be taken to secure uniformity in the rules and regulations for wiring will be welcomed by all persons engaged in practical work. Mr. Leach.

Some twenty years have now elapsed since Mr. Crompton and a few other English engineers demonstrated, at the Paris Exhibition of 1878, the practicability of the distribution of the electric current, and from that date the use of the electric light may be said to have commenced; and it is somewhat surprising that the present state of confusion has been allowed to exist for so many years.

It would be difficult to imagine such a state of affairs existing in any other trade. The idea of a separate set of rules being in force at each port for the construction of ships, in addition to Lloyd's rules and those of the Board of Trade, would be considered ridiculous, and would not be tolerated by mechanical engineers and shipbuilders.

If it is possible to have one set of rules, as in the case of Lloyd's Rules for the Use of Electric Light on Steam Ships, I feel sure that, if engineers will sink their prejudices, it will be quite possible to frame a

Mr. Leach. set of rules for universal adoption in the United Kingdom, to meet all requirements as regards efficiency and safety and to give satisfaction to all parties concerned.

Owing to the divergence in the opinions as to the necessary minimum insulation resistance required for installations and on other points, it would appear at first sight extremely difficult to obtain the sanction of the various central station engineers and other parties interested, to adhere to a uniform set of rules for use in the United Kingdom. It therefore remains to be seen, if a set of rules could be devised to meet the views of the various authorities, and what steps, if any, could be taken to enforce the use of such a set of rules.

On reference to the papers, it will be seen that the insulation resistance required for 25 lights at Oxford is only ½ megohm, while for a similar number of lamps on the Westminster Company it is 2½ megohms or five times as great; and at Edinburgh, where the working pressure for the lamps is doubled, the insulation resistance is also 2½ megohms. To take another example, the figures at Bradford and St. Pancras are $\frac{10 \text{ megohms}}{A}$, whilst at Glasgow $\frac{60 \text{ meghoms}}{A}$ are demanded, or six times that at Bradford and St. Pancras.

It appears to me, however, that although the reading of these papers will doubtless arouse the attention of engineers to this subject, it would be impossible to deal satisfactorily with the various points at one or two meetings of the members of this Institution, and that the only practical method of dealing with the question would be at a Conference where the prejudices of the various persons could be thoroughly thrashed out before a committee.

I would, therefore, venture to suggest that a Conference might be held at some convenient centre under the auspices of the Institution of Electrical Engineers, with the assistance of the Municipal Electrical Association, the Northern Society of Electrical Engineers, &c., and representatives of the Fire Insurance Companies, to enter thoroughly into this very important matter, and to secure the consent of all parties interested.

There are many other points which deserve attention in the papers before us, but these will doubtless be discussed by the members present at the meeting.

In conclusion I may add, that I think the Institution Rules might be taken as a pattern with perhaps a few modifications to meet modern requirements, and I feel sure that a uniform set of rules for any voltage up to, say, 230 volts would be welcomed alike by engineers and contractors, and I hope to see a uniform set of rules adopted as a result of the reading of the three papers before the meeting.

Mr. Cockburn. Mr. ARTHUR C. COCKBURN [*communicated*]: The papers before the Institution afford a much needed opportunity for discussing a most important question. All present must wish for the advancement of electric lighting, and one of the most important steps in that direction will be a complete understanding between the consulting electrical engineer, the fire insurance companies, and the electrical contractor, to adopt and *work to* a set of agreed rules. At the present time it is

almost impossible for a contractor to meet the varied requirements; I say "work to" because, unless the rules are enforced, they will be worse than useless. I quite agree with Mr. Hirst in his remarks concerning club life; if a person joins a club he has to abide by the rules. After all, the Institution is very like a large club for the advancement of electrical industry. If a set of rules be agreed upon as a basis, all members ought, in honour to the Institution, to support them, even if they are not made compulsory. If such were the case, a much better state of things would rule all round. Mr. Cockburn.

Attention was drawn by Mr. Brown to the form or set of questions sent out by many of the fire insurances. I quite agree with him that, as sent out, they are of little use; they appear to be sent out indiscriminately to well-known contractors who do know their work, and equally to others who are allowed to gull the public by calling themselves electrical engineers, and who hardly know a switch from a cut-out. It is needless for me to ask what is the value of the latter's answers. If an inspection were made (and the record checked) of even say one-half the installations which these forms represent, there might be a little more respect paid to the form; as it is, I think I am right in saying that very few of these forms are checked by inspection. The state of things that would be found would, I feel sure, be a great surprise even to an insurance inspector. As it is, many of the forms are filled up regardless of proper description—in some cases in ignorance, and in others because they are fairly sure that no official inspection will be made. I do not quite agree upon the wording of some of the questions, but that is a matter of detail. While upon the matter of insurance, I would like to know this: If a building is burnt out through the bad work of a contractor who has filled up a form showing the work as good, and if the risk has been accepted by the fire insurance office without a personal inspection by their surveyor, will the insurance money be paid? If not, who is liable? It does not appear just that the assured should be so. Most contracts are subject to final acceptance by the fire office interested, and upon that acceptance the assured pays the bill, and intrusts his safety and, maybe, his life. I trust my remarks will not offend any of the fire insurance offices, as they are made as much in their interest as in that of the engineer and the assured.

A great deal has been said about insulation tests. A basis of agreement as to this will be a great point gained, but the fact must not be lost sight of that it is possible for a very bad installation from a fire-risk point of view to give a very good insulation test. On some of the installations inspected I have found switch- and fuse-boards of wood, at the back of which twenty to thirty wires are crossing and recrossing in all directions, quite regardless of polarity; frequently a lot of short ends tailed on to the main and insulated (if it can be called insulation) with a few laps of loose rubber, without any waterproof tape or other protection from pressure of one wire upon another when the board is forced home by screws, &c. Often the joint is covered with powdered resin, and the outer covering is not bared back, so that the new covering can be rubber to rubber. What is the use of such work if by any chance this partial covering shifts or damp gets in? Now that pressures

Mr. Cockburn. of 200 volts are in common use, this is a serious question. Again, how often will you not find a ceiling-rose fused with a fuse large enough to carry current for twenty or thirty lamps, the screw hardly biting at all, and the whole space filled with the commonest inflammable flexible. Take, again, shop windows in which flexible wire is largely used for side-lights (which, if wired with good vulcanised flexible and properly protected, are quite safe); the flexible is too often of the poorest quality, such as that instanced by Professor Ayrton, stapled up with sometimes as many as six or ten twin cords passing under one uninsulated staple, and with the wire exposed to all the condensation of the window and to damage in other ways. Lamps are also fixed straight out without any protection, ready for goods to fall upon them, and are often made supports for his wares by the unsuspecting shopkeeper. Within the past few weeks I found on one new installation of about forty lights a large proportion of the lamp-holders held together only by one turn of the thread, not to mention switches and cut-outs with threadless screws. Low prices may be very nice, but safety must be considered as well. Were further details necessary, I could mention a large number of similar risks. It may be said there are very few electrical fires, but I think there are more than we hear about. It does not follow that, because a lighted candle is placed upon a barrel of gunpowder, an explosion must take place; but if the candle is not there, the risk will be less. In like manner, if we do not permit bad work the risk of fire will be less, and that is what we have to aim at. Stamp out bad work by a good and efficient set of rules, and insist upon their being worked up to. An electrical fire will be then all but impossible, except from an accident.

Mr. Lundberg. Mr. G. C. LUNDBERG [*communicated*]: I quite agree with Mr. Wordingham's suggestions respecting the testing and registration of fittings, as such a process would do much to stamp out the inferior material which is at present flooding the market.

In order to be acceptable to all parties concerned, the tests and registration should be carried out under the auspices of the Institution, as certificates granted by that body would be very valuable to manufacturers, and, in order to obtain the certificates, the quality of electric light fittings would rise to meet the conditions. If registration be left to separate authorities, there comes of course the question of the possibility of favouritism, and some articles might be unfairly dealt with, although of equal merit to those approved; again, there is the question of a manufacturer requiring to stock several patterns (perhaps differing in but the smallest details) to suit individual requirements.

By all means let the tests be stringent, as the reliability of articles in common everyday use does much towards influencing a consumer in favour of electric lighting and heating, his recommendation following as a natural result; whereas, if his experience has been with unreliable fittings, condemnation is the result instead, which means a loss to all branches of the profession.

I think Mr. Drake's proposal that manufacturers should have the right of being represented when the tests of their productions are made, is a very fair one. At present, the question of stocking several patterns

to suit various requirements, comes more hardly on the manufacturer than on the installation contractor, as the latter can practically order just what he requires. I think registration easily applicable to small fittings, such as branch switches, ceiling-roses, wall connections, cut-outs, &c., but hardly as suitable for larger and more expensive articles on account of cost, as if duplicate samples of these have to be supplied to several districts, prices must necessarily go up to compensate for such loss. Mr. Lundberg.

Wiring rules, I should imagine, are not made for contractors of repute, but as a means of checking the shoddy work of competitors, and in so doing they protect good work, otherwise the cutters, if not held in check, would have it all their own way, and bring the trade into bad repute.

Professor Ayrton's examples of wires, and the results he obtained from testing them, are excellent object lessons which emphatically show the need of strict supervision.

Mr. M. P. O'Gorman [*communicated*]: While supporting the vote of the Institution for unifying Fire Office Wiring Rules, I think that the I. E. E. regulations, if adopted, should be reconsidered from this new standpoint. If the Institution Rules are accepted by all the fire offices and the words "should be" are changed to "must be," as suggested by Mr. Crompton (p. 175), they will be enforced with the weight and inertia of the fire office interests. Can they bear to be thus enforced and to be taken with extreme literalness by officials who would not make exceptions merely with their own knowledge of electricity, but would regard the rules as the final decision of the electricians? We must remember that it would be difficult to appeal from an inspector who is too literal, either to expert opinion, as it would have little avail against the ruling of the whole Institution, or to any competing fire office with broader views, as the standpoint of all the offices would be identical. Indeed, fire offices often enough accept each other's verdicts on the riskiness of a risk, and they would be the more dogmatic on electrical matters if they were unanimous. Mr. O'Gorman.

To frame rules so that they shall not impede development by the words which are only intended to accept the present standards and forbid a return to errors, is a difficult task the consideration of which leads me to differ from Mr. Wordingham who, as a member of the Committee, complacently writes (p. 168) that "The I. E. E. Rules are as good as can be drawn up at the present time." The use of materials found to be serviceable should not be stereotyped to the exclusion of others, and new methods should not be hampered. The I. E. E. Rules when put to the test of literal interpretation commit both these faults and others.

I can quote two instances in which an electrical fire might occur in a house complying with the recommendations of the I. E. E.

On p. 6 of the Rules, a risk with the use of conduit is not provided against.

On p. 7, a test of little value is specified and a useful and easy test is neglected.

On p. 15, column 7, a fallacy is perpetuated.

Mr. O'Gorman.

On p. 2, as Mr. Raphael has pointed out, there is an inaccuracy in using the hard instead of the soft standard of conductivity.

On p. 7, under precautions as to switches and fuses and other appliances, a risk with glow lamps is not provided against.

I think that those who have adopted or invented the following systems of wiring have had scant consideration.

(1) The Factory method of using wires insulated throughout and held at a distance from each other and the ceilings, &c., by porcelain insulators, deserves more than the bare condemnation.[1]

(2) Mavor and Coulson's Conamor System, Sharpe and Kent's method, and others in which the outer concentric conductor is earthed and its conductivity assisted by the lead sheath and armouring wires.[2]

We find no recognition of the economy, waterproof character, and flexibility of these methods.

(3) Andrew's System, see p. 2: "All conductors having larger area than 14 S.W.B. must be stranded;" also pp. 4 and 2 as below.

(4) Twin wire half round conductors.

It is almost unfair to exclude these from "all ordinary methods" in the terms of line 3, p. 1: indeed many of the claims on p. 1 are excessive, as for example:—

The rules are stated (p. 1) to embody "the precautions which must be strictly enforced if a user of electrical energy wishes to have his premises supplied in such a manner that he may be as free as possible from risk of fire, &c. &c., with due regard for economy in first cost and maintenance." The claim to have considered economy in a pamphlet on wiring which does not mention twin wire systems, armoured return systems, or concentric earthed return methods is out of place. It is also unnecessary because there is no engineering which does not include an appreciation of eventual advantages as against the total expenditure to be required.

For the purpose of testing the rules, it is fair to apply the case of an extremely literal official, just as the rules themselves apply a double voltage to a completed installation. Such an inspector would raise difficulties over the four systems of wiring above mentioned. He would condemn conductors of any other metal than copper, such as aluminium or zinc[3] (patented by Mr. J. Swinburne). He would restrict lead-covered wires in casing to the same current density as rubber-covered wires, notwithstanding their greater over-all dimensions, heat conductivity and consequent emissivity (p. 15).

[1] See p. 4: "Must be further protected by casing or pipes;" and p. 6: ". . . must not be permitted unless mechanically protected throughout their entire length."

[2] See p. 4: "Concentric conductors must in all respects conform to the requirements laid down for single conductors.

"The insulation of the outer dieletric must be . . . that in the table. . . ;" and p. 2: "Conductors must be of H.C. copper. . . ."

[3] P. 2, line 20: "The conductors should be of H.C. *copper*, &c.," and p. 5, line 9: "The dielectric (in flexibles) should be *either pure* or *vulcanised rubber*. . . ."

This wording fails to recognise that copper at its present price (£70 a ton), is nearing the margin at which it becomes cheaper to use other metals, and that the supply of rubber is limited, its price rising, and its substitutes receiving deeper study and daily improvement.

He would allow concentric conductors (if the outer were insulated) to be run at the same current densities as a pair of equivalent "singles" lead covered, notwithstanding the difference of their capabilities for emitting heat. Mr. O'Gorman.

He would pass an installation without testing between the wires. Thus two leads in casing abraded within a foot of each other and subject to local moisture (a most risky combination) would be considered good provided the local moisture did not extend to the earth employed to test from p. 13, line 5.

He would condemn the use of wires covered with a rubber substitute, however excellent, say with cellulose, bitumen, asbestos, gelatine, flexible glass, or any of the alternatives to which we look for relief from the weight of lead or the expense and rapid decay of rubber unless they obtain an insulation admitted to be unnecessary (p. 5, line 9, and p. 15). To give practical proof of an improvement in dielectrics requires superhuman energy and perseverance, without there being added to the inventor's labours the toil of passing fresh resolutions before the I. E. E. and without awaiting the results of representations from the fire offices.

He would insist (p. 9, lines 6 and 7) that the main switch should be placed *at* the transformers or *at* the point of entrance of cables (*see* "Elect. Rev.," vol. 14, p. 231, for the peculiar interpretation to which this is open).

He would make the rules retrospective as there is no provision to the contrary.

He would cast out the arc lamps in which both terminals are not insulated (and few would escape). He would, in fact, be so obnoxious that the unamended I. E. E. rules may be prophesied to break down under the test of literal interpretation.

Reverting to the faults enumerated above: the I. E. E. rules unmistakably recommend—and with good reason—pipes or conduit in preference to casing, yet they draw no attention to the necessity for an insulating bush between every lamp-holder and the conduit to which it is screwed. In an installation with regard to which I was recently consulted, where the inflammable property is worth £200,000, it was found that, although no expense was spared, the insulation between conduit and mains was about 200 ohms, this leakage was almost entirely in the plaster of the lamps, which were the best make of the best known English maker. On introducing ebonite bushes between some two hundred lamps and the conduit, the measurement improved 100,000 fold. The rules should have foreseen the fault.

Stephenson remarked that "man lives not by bread alone but principally by catch-words;" here is one of them born in the early gutta-percha age and rampant yet (p. 4, line 6): "At 60° F. after one minute's electrification and after the test piece has been immersed in water 24 hours" a wire shall measure 1,200 million ohms.

It is not generally known that rubber is capable of making excellent and durable cables, for its reputation has been undermined by the above unfortunate test, which proves nothing, and which can be complied with by untrustworthy rubber mixings. For example: an 800

Mr. O'Gorman. yard length of 7/16 measured 200 megohms and was condemned; it remained immersed for three months and still measured 200 megohms. On being cut in halves it measured at the same rate per mile, viz., 100 megohms. These tests, far from condemning the length, are good evidence of its excellence. The rules condemned it!

Another (bad) length measured 4,000 megohms after 24 hours and was passed. It was left in the tank by mistake, and after a week in water, tested 100 megohms. Having been sealed "accepted" by the engineer, he took delivery. Although I agree with the inspector that 100 megohms is enough insulation resistance, I would condemn the cable for the fact "*that it fell* from 4,000 to 100 megohms." The rules accept it!

Cable makers will, I believe, agree that a good mechanical test with a low uniform and permanent electrical test is far superior to a poor mechanical result with high temporary megohms. A severe but valuable mechanical test of good quality rubber is to stretch the covering to twice its length for 24 hours, and pass it if it is unbroken and recovers its original length within 20 per cent. in an hour's time. The War Office sometimes specifies the stretching test, and shows that it wants rubber, not French chalk. If it were to try the rate of falling off of insulation resistance with a week or a month's immersion, it would frequently be astonished with the results.

Similar remarks apply to lead-covered cables; with these, especially, the test in megohms is of little importance; the permanency is everything. It is only the large pinholes in the lead covering that will be revealed by 24 hours' immersion. Although it is obviously impossible to test for a month in more than a few cases, there is no hardship to makers or users in a specification which may be enforced on occasion and requires a standard of good work. It is usually admitted that the life of lead-sheathed cable depends on the permanency of the sheath, and also that for various sizes of cable the minimum safe thickness of lead is (after eight years' experiment) well known, not to the purchasers, nor indeed to most consulting engineers, but to those to whom the Institution has easy access for evidence.

A table of the minimum advisable radial depths of lead which may be used with dielectrics that will not stand a month in water without deterioration, would be a safeguard against the cutting effect of competitive tenders. There, as elsewhere, the wording should be such as not to specify that lead alone should be used, or that the table of thicknesses applies to other metals.

I am not alone in hoping that the compulsory megohms per mile of wire may be considerably lowered, and this is reasonable both from the leakage and fire-risk points of view. It is an error to enforce a standard easily attained by rubber and rosin oil, to the detriment of new processes, which are arduously making their way to public notice, or on insulating materials yet to be discovered. It is notorious that the surfaces of fittings measure so low that the high insulation on the wires is wasted, when installed.

The Board of Trade is satisfied with 6 megohms per lamp wires, *i.e.*, the insulation of a central station with half a million lamps would be 12 ohms to earth, or, say, a leakage at 100 volts of 8 amperes.

This is a reasonably lenient standard, and if we allow 20 yards of small-sized wire per lamp and make the insulation of the cable equal to that of the fittings, fuses, switches, &c., we find that 20 yards of cable should measure 6 megohms, or, say, 500 megohms per mile *installed*—*i.e.*, about 100 megohms after immersion in water for a month. This appears to establish that one-twelfth of the I. E. E. megohms for small wires, and one-quarter for larger sizes, is all that safety requires. On the other hand the "long submersion" test ensures that these results are permanent. Mr. O'Gorman.

It is not for me to extol the valuable work which has been expended upon the I. E. E. Rules or the excellence of many of the results. The determination of a suitable test for flexibles, the public recognition of tube and conduit wiring, the abolition of the 1,000 amperes density rule, drawing attention to such details as "that the covers of switches shall be kept clear of all internal mechanism" and other things too numerous to name, all deserve the high praise and appreciation which they have received without stint from all quarters.

Various members of the Committee have asked for criticism, doubtless with a wish to be warned in time of any defects that might be thrust home by future experience.

In view of this, I put forward the following :—

On p. 3 an effect of temperature on cables containing oils, pitches, wax, &c., is the *flowing* of the viscous part of a dielectric.

Where conductors run vertically on the sunny side of a house, or a pit-shaft, or even down a hill, the result may be a pool of oil in the switch or junction box, and an air-space cable above; no precaution against this is suggested, though it would be easy to hang a yard of cable in a warm place and watch it.

On p. 4 it is stated that a test piece should be cut from the conductor and a high pressure applied for ten minutes. This excellent rule may be rendered valueless because the length of the test piece has not been specified, and the time has been restricted to ten minutes. For instance, a low tension cable which in lengths of 200 yards always breaks down at just about 3,000 volts, will, if tested on a short length of three or four feet, easily withstand 80,000 volts for many hours.

A ten minutes pressure test is insufficient to discriminate between good and bad cable. The theory of the breakdown of a dielectric which we will suppose is not electrolysed by the direct or alternating pressure applied, is illustrated by the following :

An air-space between two brass balls which is at the point of breakdown, but which will nevertheless resist the strain indefinitely, will be caused to fail by introducing a material such as ebonite, or glass, which has a very much higher dielectric disruptive resistance than air.

The reason of this is that the ebonite or glass has a high specific capacity, and as the voltage divides itself up in the dielectric under stress inversely as the capacity of its parts, there will be an increased voltage gradient in the air, which will fail, the ebonite will thereupon get hot and also break down. From this we see that the *non*-uniform specific capacity of a dielectric (which invariably occurs in practice) is a cause of weakness, that weakness results in heating, and that heating takes

Mr. O'Gorman.

time to accumulate enough to raise the temperature and increase the leakage sufficiently to produce breakdown.

Many experiments have shown me that this time element for materials at present laid up as cables exceeds ten minutes. I have not found, however, that if a material fails to break down during the first hour of test, it will break down after, and I would suggest that an hour is a more reasonable time for which to apply the high pressure.

On p. 5 a bending test is specified which is met by the cable makers but which is extremely severe inasmuch as it does not occur in practice for the larger sizes. A one-inch diameter cable is rarely bent round a curve of six inches radius. The treatment of bending three times in one direction and three times in the opposite is exactly such as will produce minute vesicles or vacuous spaces in the dielectric under the lead. This results in a non-homogeneous inductive capacity and consequently in the break down of a cable which would stand the bending, if effected extremely slowly and with an interval between the bendings and the pressure test.

On p. 5, line 11, flexibles are ordained to have rubber laid on in "two laps so as to lap-joint." I would suggest "break-joint" instead of the new word and "*layers*" instead of laps, unless there is a special merit in applying the rubber spirally, as that word implies.

On p. 7, line 26, we are told when jointing wires to switches to cut back the lead for a distance of three-quarters of an inch from the end of the insulating material : where this is done, we ought to be told to cover again the exposed hygroscopic material with a waterproof tape, such as rubber.

On p. 7, paragraph 7, the damp from a conduit which sweats into the base of a switch should be avoided by other means than "an additional porcelain base for the switch" as recommended.

On p. 10, line 15, there occurs, inappropriately, under the heading of "Fuses and Portable Fittings" the only mention throughout the rules of one conductor being connected to earth. This is, as I have pointed out before, a casual way of dealing with an important branch of wiring which, but for this phrase, is apparently condemned.

Under the heading of "Fuses and Fittings," &c., the danger of conical paper shades on glow lamps should be mentioned, especially when a resistance frame is on p. 12 forbidden to have wood within six inches horizontally. Three months ago, at Norwich, a three-candle power frosted glow lamp set fire to a paper shade, which fell on a child in bed beneath it, with terrible results. The glow lamp itself partially collapsed under the warmth of the burning paper, but was not otherwise damaged.

On p. 12, line 25, I would suggest that resistances for arc lamps "should have metallic ventilating guards, and should be so placed that no *unprotected* woodwork is within six inches" of them. The introduction of the word "unprotected" is of advantage.

On p. 13, line 8. &c., . . . provision should be made for testing with corridor and other switches in both their alternative positions, otherwise a length of cable will escape measurement.

With regard to the wording of the rules generally, they will gain

power, if warnings, prohibitions, or items of advice be enunciated in the simplest possible terms. For example: "transformers should not be allowed to heat," is better, because it is shorter than "under no circumstances should transformers be allowed to heat." Or, again, "conductors must be encased," is better than, "in all cases conductors must be encased;" the emphasis of "in all cases" is quite unnecessary to one speaking with authority. Mr. O'Gorman.

Applying this rule of straightforward diction, on p. 7, par. 6, I would eliminate thirteen unnecessary words from eight and a half lines.[1] Probably similar simplification is possible throughout.

Giving thanks once more to the able Committee who drew up our rules, I commend to them as a warning the effect of our old-fashioned traction engine legislation, which, though it protected the public, severely damaged the motor-car industry of this country through lack of foresight and careful wording.

The CHAIRMAN: Before I call on the authors of the papers to reply, I would like to add one small remark to the discussion. Some time ago, when fire rules were being discussed in another place, I heard it asked how it was that here in London there could be a large hotel wired on a plan that violated all fire rules, without any insulation whatever of a solid nature on its conductors, and that it could be allowed to go on from year to year without alteration. The answer given by a past president of the Institution, who, I believe, was responsible for the wiring of the hotel, was that when the hotel was wired it was wired in a way that was considered safe by a most eminent firm of contractors, and the insurance company was calmly, but clearly, informed that if they did not accept the installation as it was carried out the insurance would be placed elsewhere. I know several other cases where installations were allowed, and are going on in safety, which are not under any existing fire rules; and some of them I believe to be far safer than those in many buildings which are usually supposed to comply with every rule under the sun. I can conceive no more likely source of danger from fire than the wood casing which at present is, unhappily, too prevalent. If I had to draw up fire rules, I would not allow an inch of wooden casing anywhere. I have none in my own house. The Chairman.

Mr. J. PIGG, in reply [*communicated*]: General Webber has referred to the inference that may be drawn from the first paragraph of my paper. Need I say that I had no intention of conveying the impression that the interest in this subject is of recent growth only? Mr. Pigg.

What I intended to convey was that public attention had been given, more recently, to methods of protecting wiring; and I had in mind more particularly the papers read at this Institution, one of which was by Mr. Bathurst.

[1] Where conductors are connected to switches, fuses, or other appliances, care must be taken that all the separate wires forming the stranded conductor are twisted together and clamped into the terminal, so that no loose wire or strand projects; the insulation should only be bared back sufficiently to allow of the conductor entering properly into the terminals. The ends of the insulation should be thoroughly sealed, to prevent moisture creeping along the copper beneath.

Mr. Pigg. To Mr. Raphael's indictment I have also to plead guilty, and I desire to thank him for the localisation of the defect.

With reference to the want of uniformity in the rules for the regulation of wiring, I take it that no one wishes to dwell too much on the past, or to inquire too closely into the causes of the present impossible position.

Something has been said of the influence of self-love, and of the desirability of eliminating it in future. I beg to hazard the opinion that it will not be eliminated by this or any other discussion ; and it is by no means certain that it would be desirable to eliminate it entirely.

Something has also been said respecting the enforcement of a standard set of rules which, when coupled with the suggestion that the Institution Rules should constitute the standard, resolves itself into a suggestion that the Institution should obtain the means of enforcing compliance with its rules.

I trust that this Institution will never endeavour to *enforce* more upon its members than the payment of their subscriptions. That way disruption lies ; and it may be observed that a note of this kind has already been sounded. The question of the enforcement of rules is a very delicate one, and I will not refer to it further than to observe that no one likes more coercion than he is able to avoid. I will go further, and express the hope that the Institution will not *issue* any rules of its own, either with or without the means of enforcement ; and I would ask, Is the suggestion to work to one unvarying code of rules well thought out?

One aspect of the question connected with the issue of a single set of rules is, that however applicable they may be to the methods obtaining at the time of their issue, they will, no doubt, at some time become out of date. It is inconceivable that finality has been reached in wiring practice. Whence and when is to come the initiative for reform when it is felt that they no longer represent the best practice ? and how is the experimental work necessary for the confirmation, or otherwise, of new conceptions to be done under rules which may have been drawn up without reference to them ? (The removal of Mr. Wordingham's objection to the earthing of the middle wire, for instance.)

The adoption of one rigid code of rules will certainly tend to stifle individual effort, and prevent the application from the outside of that gentle stimulus which it is the duty of all to apply to representative bodies when, from the arduous character of their labours, they exhibit an inclination to fold their hands.

There is inherent in every representative body a disinclination to believe that its work is in need of revision, and it is from the man outside who, speaking figuratively, is continually breaking his shins against inconvenient obstacles, that the initiative for alteration must come.

There is, moreover, the question of responsibility, and a most important matter it is. If a standard set of rules were brought into force to-morrow, the same persons would be responsible as are to-day under their own rules. Always excepting, of course, that the framers of the rules are willing to accept the responsibility that naturally follows the assumption of the power of direction.

Mr. Wordingham has, in his paper, drawn attention to the fact that rules will vary, in different localities, with the system of distribution adopted by the various undertakers; and, whilst he advocates the adoption of the Institution Rules for the regulation of wiring proper, he thinks they should be supplemented by others more applicable to local conditions. Hence we are confronted with another objection to the use of a single set of rules; and it is an objection which will be difficult to set aside. Mr. Pigg.

For these reasons, and for others that may be adduced, I beg to suggest that the position to be taken up by this Institution—which is, of course, a voluntary association—towards those who are called upon to prepare specifications of the nature of those under consideration, should be advisatory and consultative, rather than coercive.

There seems to be nothing in the Articles of Association of this Institution which can be made to cover any attempt to direct, with authority, the manner in which ordinary commercial undertakings shall be carried out; but there is reason to expect that the Institution will be made the depository of the experience gained by its members and others; and it is common knowledge that its officers are drawn from the most eminent in the profession, and that it is unlikely that their approval will be withheld from any proposal without good and sufficient reasons.

Hence the natural way out of the present difficulty would seem to be an extension of Mr. Wordingham's suggestion with reference to the certification of fittings to rules for wiring.

If the framers of rules on behalf of guarantors and undertakers were to submit their rules in draft form to a permanent committee of this Institution, on which the two classes were adequately represented, for authority to add—"*Approved by the Institution of Electrical Engineers,*" I am, in my own humble opinion, persuaded the difficulty would be finally solved.

The self-love of no individual would suffer by the arrangement outlined, as the drafting of the rules would still be in their own hands; and they would be provided with a standard to work to such as has, obviously, been greatly wanting in the past; and it would be possible to incorporate in any set of rules the requirements necessitated by local conditions.

Besides the desirable results already referred to, the gentle stimulus aforesaid would be available, and, indeed, in constant operation. Energetic engineers (as all are) with opinions of their own (as all have) would no doubt see that the rules were not kept on too level a basis; and the Committee may be expected to see that rules approved by them are more uniform, in essentials, than existing rules are.

Mr. Wordingham, in reply, said: It is so late to-night that I must cut my reply very short; but I do hope that you will not think that I am not prepared to fight every speaker—I do not mean physically. I will try to take the different speakers *seriatim*, at any rate as regards the last meeting. Mr. Drake was the first speaker. I was glad to see that, as a wiring contractor of the very highest standing, he approved generally of the course I advocated. He seemed to think that manu- Mr. Wordingham.

Mr. Wordingham.

facturers were not allowed to see the tests performed on their apparatus. That is not so. Any manufacturer whose apparatus is being tested is at liberty, and more than welcome, to see the test made, if he likes. He also seemed to be afraid that something in the nature of corruption might arise if one had a dishonest inspector or person to make the test. This also is impossible, because the tests are perfectly definite. Nothing is left to the discretion of the inspector. Either the fitting passes the test, or it does not. Any manufacturer, as well as the inspector, can see whether the test is fulfilled. General Webber was the next to speak, and I must tender him my very hearty thanks for the kindly reference he made to the paper, and for his approval, which I value very much. He asked, among other things, whether I intended the book of Regulations I had drawn up to be a model. By no means; it is simply drawn up for the requirements of Manchester, and is not in any sense a model. Wiring rules are covered only so far as they concern the station. The remainder of the rules are purely local for a particular station. The wiring rules which are set up as a model in those regulations of Manchester are the Institution wiring rules. They are specifically referred to, and are held up as a model both to contractors and to consumers. The next speaker, Mr. Bathurst, made a long and interesting speech. He, like a good many speakers to-night, wants no rules. That is very likely; a good many people want no rules or laws either, but I do not think it would be found that any large station could continue very long without rules of some kind. He does not want testing of fittings; so far as I can see, the only reason can be that his fittings will not bear investigation. He says "Let manufacturers design proper fittings." Quite so; so do I; but who is to say they are proper? Surely not the man who makes them. He made some threat about my load curve becoming more peaky if I enforced these regulations. I have puzzled my brains to understand what he meant. I can only think he refers to the pique of the manufacturers whose fittings will not pass. He went into earthing. That is a big subject, on which I shall have to touch later on. It seems to me futile to compete in first cost with the first cost of gas-pipes; that is absolutely hopeless. You cannot put in electric wiring as cheaply as gas fittings. As regards Mr. Bathurst's guileless suggestion that a central station engineer should put down enormously heavy mains in order to save first cost of wiring to the consumer, I do not think it is likely to be adopted. With regard to the question of earthing the middle wire, without entering into the question of how many volts drop there may be per mile, it must be obvious to anybody that, no matter how carefully the installation may be balanced on paper, the domestic servant will upset the balancing, and therefore there will be local heavy balancing currents, and these must be dealt with in the question of earthing raised in my paper. I now come to Professor Ayrton's remarks. He spoke at the last meeting and also at this. The chief question which he asked was as to the basis of the figures taken for specifying the insulation resistance of an installation. I do not know what other people have done, but I can tell him exactly what I did. It is absolutely unscientific, and may not commend itself to many people.

However, what I did was this. It appeared to me that the only rational way of obtaining some average figure to which installations could conform was, to measure the results of a number of installations which you know have been well done, that have been carried out with first-class materials by first-class workmen. You take the average,[1] as I did, of fifty or sixty of these installations, and I found it happened to come out at very nearly 50 megohms divided by the number of lamps; and I settled upon that figure. I can defend it on that ground, but on no other. It seems to me a fairly practical way of doing it, but it certainly is not particularly scientific. Mr. Wallis-Jones and Mr. Brown both referred to the question of earthing the frames of motors. I think it is a very great safeguard to do that, because in my experience I find that if you do not, the insulation resistance between the winding and the frames is usually extremely low; and if you bolster that up by attempting to insulate the frame, you may get the coils practically in contact with the frame; and under certain conditions you may render a whole length of shafting, perhaps in some other shop, alive, and you may get very serious results. If the frame is earthed, I fail to see how any accident can happen. Mr. Goldsworth referred to insulation resistance improving. I quite agree that the insulation resistance goes up in damp buildings after the current has been turned on a little while; and I make it a rule, if the work seems to be well done, and the insulation resistance is not absolutely below the Board of Trade limit, to put it on conditionally that the insulation rises within a reasonable time, say a couple of months; and in all cases where work is well done it does rise. All rules must be enforced with reason; you don't want a cast-iron rule and to have no discretion whatever. I now come to Mr. Raworth's speech, and as other speakers followed Mr. Raworth on very much the same lines, I will try to deal with all their speeches together. Mr. Atkinson's speech was particularly clear, and I will try and follow that as well as the others. Mr. Evershed, Mr. Mordey, and Mr. Campbell Swinton also followed practically on the same lines. All of them will pardon me if I say one thing, and that is, that they are rather in the position of talking about other people's business; they are not central station engineers, exclusively at any rate—they may be everything, but they are not that solely. And although their remarks to some extent have been supported by Mr. Wright, and to a less extent by Mr. Cottam, I still venture to differ from their line of argument. Of course, I am aware I am tarred with the brush of being a municipal engineer. I cannot help that altogether, and I do not know that I want to help it if I could. I would remind them that, after all, some municipal engineers have been company engineers and *vice versâ*, and even a municipal engineer may be allowed to have an opinion. The first fallacy they seem to have got hold of is, that I advocated the making of rules by municipal engineers. Mr. Wordingham.

[1] It has been pointed out to me that I ought to have taken the lowest figure and not the average figure. In answer to this, however, I ought to say that the installations from which the figures were obtained were carried out some six or seven years ago, and were not all new ones; hence I thought it better to take the average, and experience shows that the modern good work easily conforms to the standard I have adopted.

Mr. Wordingham. Not at all; I advocate the making of rules by the Institution. And they all practically said, as did Mr. Bathurst, there should be no rules. I cannot believe that they seriously think there should be none at all; and I do not think there can be better evidence of the necessity for them than the fact that the Council has seen fit to draw up rules. Surely that shows there should be some rules to some extent. I do not wish to argue that question further. As regards the effect of having no rules, who do these gentlemen want to benefit? Do they think they are going to benefit the consumer by having no rules? Surely not. Do they think they will benefit the straightforward manufacturer, who wants to do good work; or the wiring contractors? Certainly not. The only effect of having no rules, or very lenient rules, would be that the market would be flooded with cheap foreign rubbish, and that the jerry wireman would have it all his own way, and drive out of the field everybody who could or would not do wiring at 5s. or 7s. 6d. a light. That would be the only result—yet not the only result; it would also bring utter discredit on electric lighting generally. Even now, if the light goes out, the average consumer says, "There goes the electric light again; always going out"; and the supply gets blamed. What you want is good wiring for the protection of the consumer for the protection of the manufacturer, for the protection of the contractor. I can see no unwarrantable interference with the liberty of the subject; I think it is all nonsense. You are protecting him against fraudulent people. I must not take up more of your time, but I felt rather strongly Mr. Wright's remarks, because, speaking as a central station engineer of such wide experience, his words must undoubtedly carry great weight. I must confess I was surprised to hear he had such an enormous amount of difficulty in keeping his pressure constant that it took up the whole of his time. I do not bother myself very much about pressure in Manchester; it keeps within two per cent, and we are only obliged to keep within four per cent., and if it does so that is all I care about. I know that for two nights we were nearly out, because we had no plant. We got the plant and we now keep to our old standard.

[*Communicated.*] My verbal reply to the discussion on my paper was necessarily so hurried that I think it well to add a few remarks to what I said.

In the first place, one cannot but be struck with the unpractical nature of the comments of many of the speakers, and with the fact that their remarks had but little to do with the subject of my paper. Some of the speakers, indeed, appeared not to have read either the paper or my book of Regulations which they so strongly condemned, as the views ascribed to me are in certain cases diametrically opposed to those set forth in my paper.

For instance, it was implied in many cases that I was endeavouring to set up my own regulations, and so was adding to the number of different codes of rules in use; whereas my paper was chiefly devoted to advocating strongly absolute uniformity by the adoption of the Institution rules, as modified after a full discussion of them by all parties interested. On the first page of my regulations it is definitely

stated that "they are additional to, and are not in substitution of, the ordinary wiring rules. The recommendations issued by the Institution of Electrical Engineers should be closely followed." So far as my own rules are concerned, only a portion deals with wiring rules, and these are not antagonistic to the rules of the Institution except in the one particular that a higher standard of insulation resistance is required. Mr. Wordingham.

The point about which most discussion centred was the question of registration. Now this registration merely requires that fittings should conform to certain tests, the tests being, for switches, those prescribed by the Institution of Electrical Engineers, and for fuses the requirement is merely that they should be capable of breaking a short-circuit, which is necessarily one of the *raisons d'être* of a fuse. The deposit of duplicate samples is not compulsory, but the arrangement is simply made to provide for the convenience of contractors and manufacturers, so as to avoid the repeated testing of identical fittings, the samples being necessarily required for the identification of fittings actually used. Registration, therefore, introduces no new requirement, but merely ensures that regulations already approved are carried out.

The point to which those speakers who made such a violent attack upon my proposition chiefly directed attention was that a municipality conducted the tests. The question of municipalisation or otherwise is doubtless one possessing very great interest, and admitting of much discussion, but it has nothing whatever to do with my paper. In the paper I strongly advocated the registration of fittings by the Institution and not by municipalities. I have merely been driven to municipal registration because there is at the present time no central body prepared to carry on the work. Not only this, I advocated equally strongly that companies should adopt the same system of registration, and should acquire powers to make regulations as well as municipalities. A large amount therefore of the discussion is wholly uncalled for by the paper, for it is merely an accident that the registration should be municipal, and, personally, I should prefer, as I stated in the paper, that the Institution should make the tests.

Several speakers were most emphatic as to there being no regulations in force as to gas fittings, the argument apparently being that, there being no regulations in connection with gas, and gas being highly dangerous, and there being many fires in consequence of the want of control, therefore there should be no regulations in connection with electric lighting. The argument appears somewhat remarkable, and the premisses are wrong. In the case of Manchester, for instance, there is an elaborate little book published containing no less than forty-two regulations corresponding to wiring rules and seventeen conditions of supply. A copy of these I have pleasure in presenting to the library of the Institution.

In conclusion, I would say that so far as the discussion related to the actual matter in my paper, I feel that I may claim that the course I have advocated has been approved, and in many cases warmly, by those immediately interested in the matter; and since my paper was read I have received a considerable amount of evidence privately which goes

Mr. Wordingham.

to show that manufacturers especially, welcome and agree with the idea of registering fittings.

Mr. Crompton.

Mr. R. E. CROMPTON, in reply, said: I am in the position of a minister in charge of a Bill. I should like to say a good deal on my own views of the matter, but at this late hour of the evening I must refrain. I have heard the discussion with considerable pleasure. I have heard one fire insurance inspector after another say he does not find any fault with the Institution Rules except that in some cases, even from the fire office point of view, they are too severe. It follows from that that there ought to be no difficulty in getting uniformity so far as concerns the fire insurance offices; that is to say, that it will require very small pressure on the part of the Institution to get the fire insurance companies to adopt our rules as the standard rules, modifying them to suit local conditions if they find it necessary. But at all events the profession, as far as wiring contractors are concerned, would then know that when they were dealing with the fire insurance companies they were always working under one set of rules, and that any modifications would be immediately pointed out to them and would be looked for when they took the contract. The other, and apparently, from what has been said, the more culpable offenders, are the municipal engineers. The extraordinary figures as regards insulation resistance demanded by some of the engineers strikes one forcibly. I am not going to say anything about it, but nearly all those municipal engineers are Members or Associate Members of this Institution, and I think that this meeting has shown—and I believe I am correct in saying has shown decisively—that it is in favour of uniformity being obtained by the adoption of our own Institution Rules as widely as possible. I will therefore confine myself to trying to give a voice to that by asking the Chairman if I am in order in moving a resolution. I have looked at the Articles, and I see that "no question shall be discussed or motion made at ordinary general meetings relative to the direction and management of the concerns of the Institution," but I take this to be not the direction or management of the concerns of the Institution, but rather the direction and management of some one else's affairs in which this Institution desires to have a hand. That being so, I am going to ask the Chairman if I am in order in proposing that we take the sense of the meeting on this resolution—"That this meeting is of opinion that the Institution should take such steps as it thinks best to secure uniformity in rules by pressing on the supply companies, municipal engineers, and fire offices the advantage of adopting the Institution Rules as a standard, with such modifications only as local conditions may necessitate." I think a strong expression of opinion from this meeting would strengthen the hands of the Institution very greatly, and I now ask you, sir, if you will put that to the Institution.

The Chairman.

The CHAIRMAN: It is unusual, but I cannot say it is out of order. It seems to me that an expression like that, where there is evidently a very strong feeling by the members present, would be quite in order. It is not actually in the form of a recommendation to the Council, but it practically does tell the Council what they wish the Institution to do; and it seems to me that, as it is not taking anything out of the power of the Council, it will be in order to have that motion put to-night.

Mr. FAWCUS: May I be allowed to second that resolution? I feel very strongly on the question of uniformity, and I foresee the great injury that would accrue to the trade generally if all corporation engineers and other authorities could have separate rules of their own in conflict with the main rules that we hope to see drawn up by this Institution. Mr. Fawcus.

Mr. O'GORMAN: May I suggest that the wording of the resolution should be slightly altered. I think Mr. Crompton's resolution recommended that the Institution should press upon the insurance companies the adoption of the rules. Many of the insurance companies have rules of their own, and it might be better if we put it in the way of "inviting" the insurance companies to discuss the question with the Institution. Mr. O'Gorman.

Mr. CROMPTON: That is one of the steps that the Institution would take. It is natural we should persuade them; we do not want to force them. Mr. Crompton.

Mr. S. MAVOR: I should like to suggest that this proposition be open to discussion, and that it should not be carried immediately, because there are several points which should be considered with regard to the rules. The rules are not inclusive enough to be so strongly forced upon the entire profession. Mr. Mavor.

General WEBBER: If this proposal of Mr. Crompton's is carried unanimously to-night, there will have to ensue a considerable correspondence, and meetings will have to be held, to which, no doubt, all those interested in the subject will be summoned, when everybody will have a voice in making suggestions, as was the case when the present rules were drafted by a committee during Mr. Crompton's own presidency. Thus every one present here to-night will be satisfied that his remarks will be heard and considered. Then, when some approach to unanimity is attained, I hope that the whole subject may be again discussed by the Institution in full meeting. General Webber.

The CHAIRMAN: Professor Ayrton proposes a verbal amendment in the motion suggested by Mr. Crompton. I do not know whether Mr. Crompton would be prepared to accept it. Mr. Crompton's resolution ran, "the advantage of adopting the Institution Rules as the standard." Professor Ayrton proposes to substitute "some rules to be drawn up by the Institution based on the present ones." The Chairman.

Professor AYRTON: I think the amendment would meet Mr. O'Gorman's suggestion. I do not think we should say that the rules of this Institution as they stand at present, but that some rules based on these, should be submitted for general acceptance. Professor Ayrton.

[Mr. Crompton intimated his acceptance of the alteration.]

The CHAIRMAN: I will read the resolution with the suggested alteration: "That this meeting is of opinion that the Institution should take such steps as it thinks best to secure uniformity in rules by pressing on supply companies, municipal engineers, and fire offices, the advantage of adopting rules to be drawn up by the Institution based on the present ones, as a standard, with such modifications only as local conditions may necessitate." The Chairman.

The resolution was put and carried unanimously.

The Chairman.

The CHAIRMAN: We have one duty more—to return a hearty vote of thanks to the readers of these three papers, which have evoked such an interesting discussion.

This was carried by acclamation.

The CHAIRMAN: I have to announce that the scrutineers report the following candidates to have been duly elected:—

Member:

Frederick Henry Jackson.

Associate Members:

Edwin E. Craig.
Edward Geipel.
Walter Frederick Jones.
Alfred William Marshall.
Dennis Grenville Shephard.
Francis Lovel Todd.
Charles E. Wilson.

Associates:

Frederick Rains Batty.
Edwin Seneirratne Dissanaike.
J. P. Gregory, Jun.
William Edward Milns.
John Henry Matthew Wakefield.

Students:

Charles Noel Moberly.
Percy G. Moore.

The Three Hundred and Twenty-Seventh Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, Great George Street, Westminster, on Thursday evening, February 16th, 1899, Professor SILVANUS P. THOMPSON, F.R.S., Vice-President, in the Chair.

The Minutes of the Ordinary General Meeting held on February 9th, 1899, were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Associates to that of Members:—

John Richard Bradford. | Frank Christy.
George Stephen Corlett.

Messrs. E. J. Brothers and W. Harling were appointed scrutineers of the ballot for the election of new members.

A donation to the Library was announced as having been received since the last meeting, from Mr. E. Highton, to whom the thanks of the Meeting were duly accorded.

The CHAIRMAN: I have now to announce one or two matters on behalf of the Council. The vote which was passed on the subject of "wiring rules" last week, practically a recommendation to the Council to take such steps as might secure uniformity among Municipal Supply, and Fire Office, and Consulting Engineers, has been before the Council at its meeting this evening, and the Council has re-appointed its former Committee with certain instructions, in order that the wishes conveyed to it by your resolution of last week may be duly carried out.

I have also to inform the Members present that after many months, perhaps I might almost say years, of consideration of the subject, the Council has adopted a seal and a form of diploma for Members and Associate Members, in order that it may conform to Article 10 of the new Articles of Association. These diplomas will forthwith be prepared, and in a short time will be available to those Members and

Associate Members who choose to apply for the same. It will be no use to apply, however, for some time yet to come, for two reasons; firstly, because the printing will take time, and secondly, because we shall certainly await—as every recipient, I think, would wish that we should await—the return to this country of our President, in order that he may sign them. There is one detail with regard to the diplomas which I have not mentioned. The diplomas will be printed, unless otherwise ordered, upon a special kind of Japanese vellum paper, and no charge will be made to those who are entitled to apply for them; but the Council has determined that there shall be a number of full Members' diplomas struck off on vellum, and for each of these a fee of one guinea will be charged to such Members as may choose to apply for that form of diploma, and to pay for it. It has been determined that any profits arising under this head shall be devoted to that most laudable object, which naturally is uppermost in the minds of every one of us when we think of the future of this Institution, the Building Fund.

The following paper was then read:—

ELECTRIC TRACTION BY SURFACE CONTACTS.

By MILES WALKER, Associate.

We cannot put forward any surface-contact system as a practical substitute for the overhead wire in electric traction unless we can answer in the affirmative the following three questions. (1) Is it possible to lay surface contacts in a roadway so that they do not cause any obstruction to traffic? (2) Is the method of picking up current from studs by means of a skate feasible under practical working conditions? (3) Can the studs be made perfectly safe? If affirmative answers to these three questions can be established, the future of surface contacts for town traction is a great one. If the answer to any question is unsatisfactory, there may be grave objections to this system of conveying electrical power to a moving car.

Nothing but a long trial can give us conclusive answers, but an Institution like that of the Electrical Engineers can form a very fair judgment upon the probabilities of a scheme on being supplied with experimental data, and it can moreover give a diversity of criticism and suggestion which will

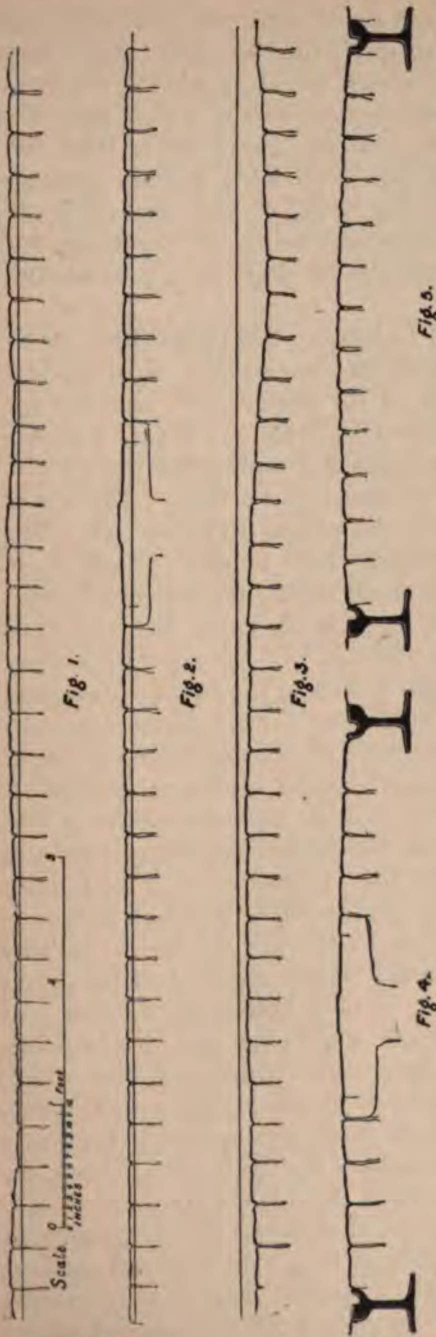


Fig. 1. Fig. 2. Fig. 3. Fig. 4. Fig. 5.

be of the highest value to a scheme in its infancy. It is to obtain that judgment and to profit by that criticism and variety of suggestion that I have brought forward the details and experimental data of a surface contact system, upon which Mr. C. E. Holland and I have been at work under the direction of Professor Silvanus Thompson.

I. To arrive at an answer to the question as to the obstruction in the roadway we must find by what amount the studs ought to project and also what amount of inequality is ordinarily found and is admissible in a roadway.

During the last few months I have been making observations of roadways in city streets and along tramway tracks, and I have been surprised to find how great the depth of inequalities in pavement really are when we come to measure them. Hollows $\frac{1}{2}$ inch or even $\frac{3}{4}$ inch deep are hardly noticeable if of two or three feet in

extent. Hollows an inch in depth, and with gently sloping sides extending four or five feet from the centre are of very common occurrence in roads which a casual observer would call good. The track between tram rails is generally to be found much more uniform as to levels than the other parts of the road, but even here sudden changes of ½ inch or ¾ inch in the level of the crowning are so common that we never notice them. It is difficult for the eye to detect slight changes of slope in a groundwork of rough granite setts.

In order to give an idea of the sort of inequalities which commonly occur unnoticed in roadways, I have made a drawing (Fig. 1) to scale of the section of a fair average piece of well-worn roadway in the Holloway Road. The section is taken parallel to the rails and midway between them. The straight line gives the level of the rail. The upper line gives the inequality of the paving-stone surface. The average hollow that one meets with is about ½ inch deep. In places the stones have sunk until they are nearly level with the rails, while the original crowning between rails seems to have been about 1⅛ inch. Of course it looks very much worse in section than it does in the street. For comparison with this piece of paving I have given a section (Fig. 2) drawn to the same scale of our experimental line at Willesden Junction. This, of course, represents a piece of track just newly laid and without the inequalities which are sure to come with time and traffic, but one can see at a glance that if we impress the inequalities of Fig. 1 upon those of Fig. 2 we do not get anything which is substantially worse than either. In order to show the sort of hollows that one meets with in roads which are in bad condition I have given a section (Fig. 3) of a hollow in a rather bad piece of roadway. I do not know that the thoroughfare from which this was taken (and which contains many hundreds of similar hollows) bears at all a bad name. People seem to put up with them very quietly. Figures 4 and 5 show sections of roadway at right angles to the rails, one with a surface contact and the other without.

Now the projection of the stud shown in Fig. 2, innocent though it may seem in comparison with ordinary paving-stone inequalities, is amply sufficient to give the required clearance to the skate. Its top is on the average 1⅛ inches

above the level of the rail, and the clearance between the skate and the crowning of the pavement between two studs is $\frac{3}{4}$ inch. This is sufficient to clear all but very deep mud. The effects of mud and water are considered at length below. If a paving-stone happened to project some way above the others it would do no harm until it projected above the level of the stud, and a rare occurrence of that kind could be very easily dealt with.

II. This brings us to our second question as to the feasibility of picking up current from studs. It should be noticed in the first place that a row of studs is a very much better thing than a sectional rail. A stud is much easier to insulate than a rail. The leakage from a rail in wet weather would be twenty times as great as from a stud. A rail is much more difficult to renew, and though it gives greater facility upon sharp curves, its advantage in this respect loses weight as soon as we show that the studs can operate on the sharpest curves met with in practice.

When studs are used it is of course necessary to carry below the car a long metal skate which will maintain its contact with one stud while it passes on to the next. And the question naturally arises, Can a skate be made to run smoothly over a row of studs ? That is a question upon which we had considerable doubt until we tried it. The answer (so far as an answer can be obtained from an experimental line) is that it can be made to run at the highest speed smoothly, in fact almost noiselessly. We have tried the effect of various obstacles on the line. Anything big, like a brick or a tin canister, is simply knocked out of the way without even causing a spark. Stones standing about an inch high generally suffer the same fate. Sometimes, however, if a stone is wedged carefully into a hollow in the road the skate will glide over it and be prevented from touching the stud. The effect of this is that the car is deprived of power from that stud, but running on by its impetus contact is again established with the main. This is an accident of no moment, and would, in fact, very seldom occur in a paved street. A shovelful of gravel thrown upon and round the stud has the effect of causing considerable sparking, but does no great harm. Gravel upon the track will cause sparking in any electric traction system in which the rails are used for a return.

One method of supporting the skate (in the case of a four-wheeled car) is shown in Fig. 6. A frame (part of which is shown in the figure and marked F) is supported on bearings running on the axles of the car. To this is bolted a pair of stout brackets of the shape seen in the figure. A vertical pin, 1¼ inch in diameter, with a thickened base pivotted to the skate, slides freely in each bracket, and carries two pairs of lock nuts. The lower pair press upon a spring and can be adjusted to take any required fraction of the weight of the skate, while the upper pair act as a back-stop and prevent the skate from falling below a certain fixed limit. In practice the back-stop should be adjusted so that it allows the skate to fall as low as the lowest stud on the line (say about an inch above the level of the rail), while the spring is adjusted so that when running on an average stud the

FIG. 6.

pressure is about 10 lbs. The spring being a long one and much compressed changes its forces very little with a lift of ¼ or ½ inch. The little friction that occurs on the pin is just sufficient to deaden any tendency to dance when running at a high speed. The skate is composed of a U-shaped steel girder 4⅛ by 2½ inches, and is 18 feet long.

The side webs of this girder are cut off for a length of three feet at each end of the skate, so as to give the point a certain amount of springiness. The springy portions are then bent up at a very gentle slope (about 1 in 100), and the tips are still further curved upwards. By supporting the skate on springs at two points about four feet from its ends a very useful pitching motion is given to it as it slides over the studs. This is shown in Figs. 7 and 8, in which the vertical scale is exaggerated so as to make a small drawing sufficiently clear.

The greater part of the weight of the skate being borne by the springs, so long as a stud is under the central part the ends are kept well above the ground. But as the stud gets nearer and nearer the hind end more and more weight is thrown upon the front end, until just before the skate reaches the next stud, the front end is down upon its back stop, as seen in Fig. 8. This stop is about four feet from the tip, and the slight tilt of the skate throws the tip a little lower than it otherwise would do and at the same time presents the striking surface at an exceedingly small angle (about $0° 30'$) to the horizontal. Thus the contact with the studs can be made almost silently even at the highest

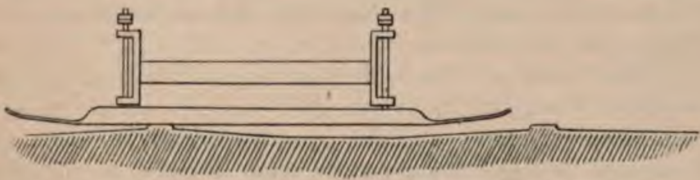


FIG. 7.—Vertical scale exaggerated, showing how skate rests on stud.

FIG. 8.—Vertical scale exaggerated, showing the dipping action of skate.

speeds. The tip of the skate, which is ordinarily well above the ground, dips down to accommodate itself to a worn stud just at the point where it is required. Having once touched the stud and excited the magnet under it, the attraction of the magnet helps to stop any jumping. The ends of the skate being very springy accommodate themselves very well to studs differing considerably in height. In practice it will be possible, by setting the boxes directly upon ties between the rails, to ensure that the studs do not in the ordinary course vary by more than $\frac{1}{4}$ inch.

The method by which one stud is made alive before the car leaves the stud behind will be considered in the next

section, but while considering the skate it is important to note what amount of overlap must be allowed to it. In designing an experimental line we wanted to be on the safe side, so we allowed a $3\frac{1}{2}$ foot overlap, knowing that the switches took about $\frac{1}{10}$ second to close. Thus the skate was made 18 feet long with $14\frac{1}{2}$ feet between the studs. This overlap is more than sufficient. We have run at a measured speed of 16 miles an hour without experiencing trouble from the non-closing of switches, and could probably go quicker but for the inconveniences of our short track. Now as the Board of Trade regulations limit the speed at which cars in towns may run far below this, we might have a much shorter overlap or a longer distance between the studs. The Board of Trade Regulation for Dublin runs as follows:—

"The speed at which the carriages shall be driven or propelled along the tramway shall not exceed the rate of eight miles an hour, and the speed at which the carriages shall pass through facing points, whether fixed or movable, shall not exceed the rate of four miles per hour."

Further, in the regulations relating to the motor carriage it is provided—

"It shall be fitted with a governor which cannot be tampered with by the driver and which shall operate so as to cut off all electric current from the motors whenever the speed exceeds 10 miles an hour."

While it is possible to go at the speed of 15 to 20 miles an hour with a moderate overlap, we can by shortening the overlap, positively prevent the car from going at a speed greater than a certain fixed limit.

We find that we can increase the length of skate to 22 feet for a 25-foot car and have 18 feet 6 inches between the studs. Lines on which bogie cars are employed would allow of still greater length of skate and as much as 30 feet between the studs.

In wet weather a certain amount of leakage occurs from the stud which is for the time supplying current to the car, and it is important to inquire how much this leakage is. It is easily ascertained by actual measurement with street mud and water placed around the stud and skate. The following data were obtained by experiment. The pressure on the line was 500 volts.

PARTICULARS OF LEAKAGE FROM STUDS.

Condition of Road.	Current leaking in amperes.
Track covered with very wet mud and water ½ to ¾ inch deep so as to make the very worst street condition. The skate resting on two studs at 500 volts above earth. The mud between the studs touching the skate at intervals along its length. Total leakage from the *two studs*	3·9
The same after standing five minutes	4·5
After one stroke of a brush so as to reduce the depth of mud and water to ⅛ inch	2·3
One stud with short skate resting on it. Wet mud about ¼ inch deep...	1·2
The same with mud deeper. Very bad road conditions...	2·5
The same with thin mud and water as it would be on an ordinary wet day	0·4
The same dry	Practically none.

It must be remembered that the sloping asphalt round the studs tends to become cleaned in very wet weather. After a heavy shower, the bulk of the mud having been removed, the leakage will be very little indeed. It is probable that the average leakage in wet weather will be about half an ampere per car; while under exceptionally bad road conditions it may approach five amperes. By carrying a brush in front of some of the cars to sweep away the deepest mud it can be kept at about one ampere even under the worst conditions. Taking only five fine days to one wet one, the average leakage will be under a quarter of an ampere per car, or about 2 per cent. of the total power of the station. It is difficult to say what effect this will have upon the coal bill, but it is certain it cannot increase it by 2 per cent. Of course on a dry year like the one we have just had the average leakage would be almost negligible.

Ease in going round curves and over crossings is a matter of great importance in any system of electric traction in towns. When a long skate is used on a simple four-wheeled car it is necessary to put the studs nearer together on a sharp curve, because the ends of the skate do not keep to the centre of the track. On a 34-foot radius curve it is desirable to have studs every 8 feet. This, of course, adds to the cost of construction, but as the greater part of a tram-

way company's rails are straight or nearly straight it is not a serious matter. The length of the skate is really limited by the radius of the sharpest curve on the line. If a line has many 34-foot radius curves, a suitable length for the skate is about 18 feet for a four-wheeled car.

It is a great advantage in a surface contact system to have only one row of studs in the centre of the track. It is also an advantage to have each stud self-dependent; that is to say, capable of being exerted without interconnections from other studs. In systems having two rows of studs it must be very difficult to keep the right skate on the right stud when going round a curve, unless the skates are quite short and the studs very near together in each row. At crossings the difficulties would become greater. We have found that by employing a single row of studs in the centre of the track (where full advantage can be taken of the crowning of the paving-stones) there is no difficulty in going round sharp curves and over crossings.

In electric traction systems it is not usual to provide a means of bringing the car off the rails and taking it round an obstruction on the line. If it is thought desirable to do this, it can be done very easily on a surface contact system by carrying with the cars properly protected flexibles which can in an emergency be attached to the studs and to the rail.

III. We now come to the most important question—the question of public safety. The automatic switches must be designed so that they can only be closed when a car is over the stud, and the provision for guarding against the accident of a stud being left alive in the street must be absolutely without a flaw if the surface contact system is to be permitted for a moment in England.

It is exceedingly difficult to form an estimate of the safety of any particular device. Things that we are accustomed to use every day without injury to ourselves we come to regard as harmless, often not so much because they have no element of danger in them as because in common experience accident does not happen. Who seeing for the first time a railway train steaming at 60 miles an hour would care to trust himself on board it? The more he might know of the laws of motion, the attraction of gravitation, and the strength of materials, the less would he be

inclined to listen to arguments to prove that it could not come off the rails. The reason that we dare go railway journeys is not because we are satisfied with the proof, it is because in common experience trains do not run off the rails. Upon the introduction of anything to which we are unaccustomed, anything that contains a conceivable element of danger, we are at once afraid, and it is exceedingly difficult for any one to satisfy us that it is perfectly safe. It is so with surface contacts. The more we know about mechanism, the less we feel inclined to trust in the safety of a device which is proved to be safe by unanswerable arguments. There is only one way of bringing conviction to the mind of the timid, and that is to simplify the issue upon which safety depends until it becomes identical with something with which we are perfectly familiar and in which we have perfect confidence. It is in this way that we have tried to bring home the safety of surface contacts.

Take something that we are sure about. Can a magnet attract the air? It cannot. If we are sufficiently sure of that, it will do for a principle upon which to base a safety switch. But we want something more than a principle, we want a moving part to make a switch. Then let us take one of the simplest and surest of all moving things, a heavy cylinder sliding in a very loose vertical tube. Is it possible for it to remain suspended in the tube when the only force upon it is gravity. If it is impossible, then that cylinder will do for our moving part. The problem now is to make a switch whose safety depends, and depends only, upon a magnet not attracting air, and a cylinder which will not stick. If we can do so without introducing other issues of a more complicated character and with which we are less familiar, then we have a switch upon which we can rely. If we take an iron plunger with expanded ends, like that numbered 2 in Fig. 9, and surround it by a solenoid of wire, carrying a current, it is possible to choose the size of the heads so that the tendency of the plunger to assume a central position is balanced by the attraction of the solenoid on the iron heads. With properly chosen heads the solenoid exerts no force whatever on the plunger. Gravity being the only force upon it, it will, of course, take up the position No. 2. If now a piece of iron is placed over it, as shown in No. 3, the plunger may be raised by the attraction

between the two. In case a very heavy plunger is used it is possible to support part of its weight by merely increasing the size of the lower head by a small amount. Thus we have a simple piece of mechanism whose movement is entirely dependent upon the presence of a mass of iron in its neighbourhood, and which cannot be moved by the solenoid itself, even though the magnetising current carries the iron up to the saturation limit. This has been converted into the operating part of the switch, in the manner described below. Now, though the force of attraction is sufficient to lift a fairly heavy plunger and operate a switch, it is desirable to have something more than this. We want a switch which will, notwithstanding its massive moving parts, close

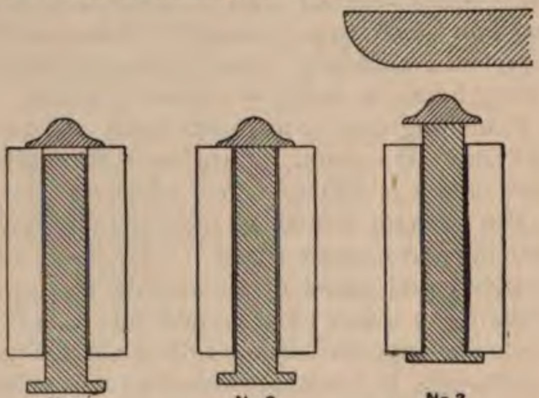


FIG. 9.

in an exceedingly short space of time. We want a very great acceleration upon the moving parts during the first part of their motion. This is easily obtained by merely separating the upper head from the plunger by a short air-gap, as shown in position No. 1 of Figure 9, and arranging matters so that the mass of iron comes over the switch before current is put through the magnetising coil. As soon as current passes, the attraction upon the head is so great that the plunger gets up a high velocity before the two come together, with the result that the switch operates as though it had been hit with a hammer. A very simple device, a mere film of oil acting as a cushion, can be employed to prevent the switch from being injured by the concussion. The connection of this operating part to the contact points

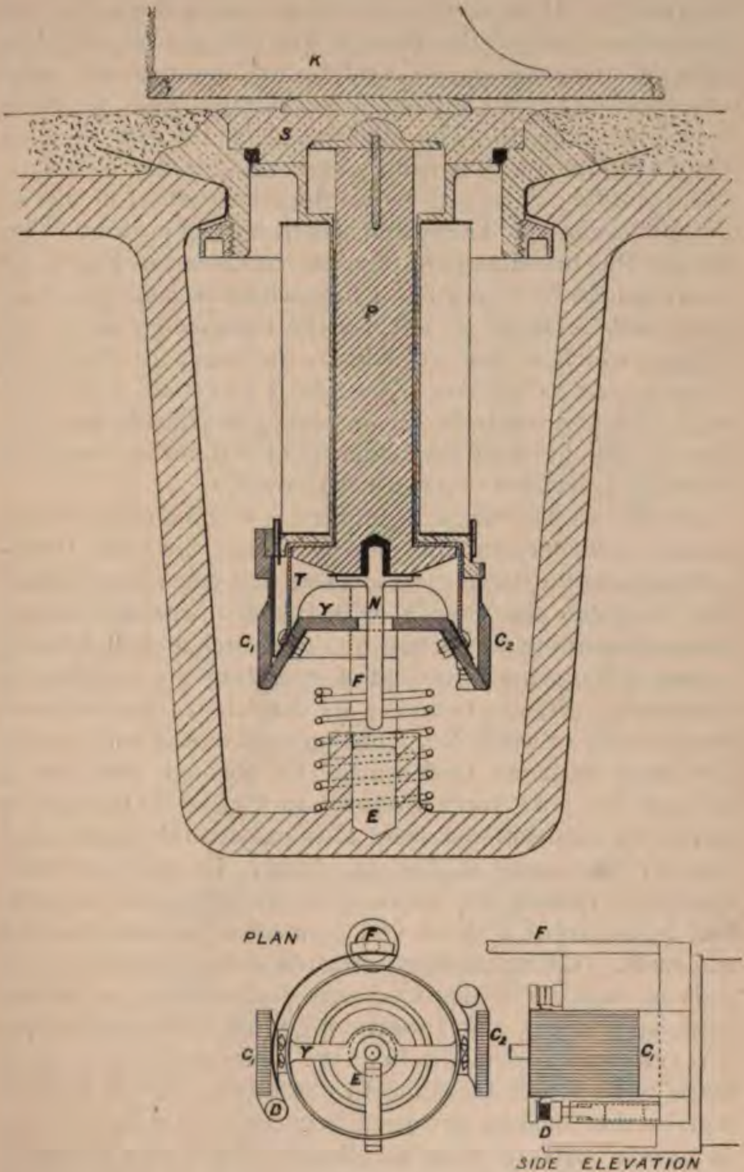


FIG. 10.

of the switch has been the subject of some very varied experiments. If we are to preserve our original simple issue we must not hamper this plunger with any gear at all. The safety of street passengers must be made dependent upon nothing else than the falling of the plunger when the mass of iron is removed. The attachment of any gear at once introduces the risk of friction whose amount may be a variable quantity. Figure 10 shows the construction of one of our safety switches. The switch is in the closed position. The plunger P is the same in design as that shown in Fig. 9. It is surrounded by a very loose tube which is enlarged at its lower end, T, so as to enclose the expanded end of the plunger, which is free to slide in the tube, the clearance being as great as we like. The tube T can itself slide in an outer tube, and is attached by insulating brushes to the yoke piece Y, which, when the switch is closed, makes electrical connection between two brushes C and C2.

As the whole switch is immersed in oil a considerable suction exists between the plunger P and the tube T notwithstanding the large clearance, so that when the plunger falls it carries the yoke Y with it and opens the switch. Before describing the details of this switch it will be best to look at the general method of operating the switches in succession. Figures 11 and 12 are diagrams of the electrical connections. Switch No. 1 of Fig. 11 is closed and current is flowing from the feeder main FF through the fuse *f*, through the yoke piece from C_1 to C_2 and to the stud S, thence by means of the skate K it passes to the motor and then to the frame of the car truck. In this particular method of making the connections the plunger is magnetised by means of a shunt coil connected between the stud and earth. The comparative advantages of shunt and series coils in switches of this kind was discussed in our British Association paper, and I need not go into that matter again. It is sufficient to describe here the method which we have found to be simplest and most satisfactory. It will be seen that in the diagram the yoke between C_1 and C_2 is drawn as a piece separate from the plunger, but it is to be understood that in the ordinary course they rise and fall together as mentioned above. Thus in No. 1, Fig. 11, the plunger is up and the yoke is up; in No. 2 they are both down. Fig. 12 shows the car in the act of passing from one stud

to the next. As soon as the skate K touches the stud of No. 2, current passes from it round the shunt coil, magnetising the plunger, which rises and is held up by its attraction upon the iron skate K. A moment later the skate K leaves switch No. 1, and as the plunger has no longer anything

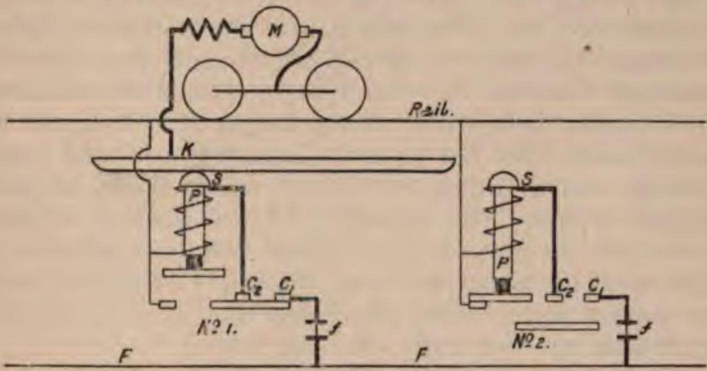


FIG. 11.

which it can attract it falls and opens the switch. The forces required to open the switch are very small, and the three pounds weight thrown upon the yoke piece upon the removal of the iron of the skate gives a very ample factor

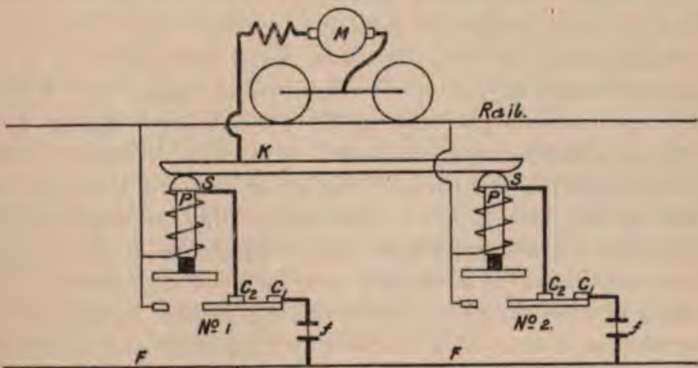


FIG. 12

of safety. But the safety of street passengers does not depend upon the opening of the switch in this way. If for any reason whatever the yoke piece did stick up (a thing which it practically never will do), the plunger will slide down the tube T and make connection between the yoke

piece and the earth in the manner shown in Fig. 13. This will connect the main to earth through the fuse *f*. The fuse will, of course, be blown immediately and the switch entirely cut off from the main.

The switch shown in Fig. 10 can now be described in greater detail. In the first place it is contained in a round cast-iron box, the lid of which is of gun metal and forms the stud. The lid is fixed into a gun-metal ring which is insulated from the cast-iron box by means of micanite. The whole is designed to bear a weight of 15 tons and to take a heavy blow having a considerable horizontal component, such as would be received from a heavy waggon jogging on to it. The space round about the stud is filled in with asphalt up to the level of the road, but the micanite is designed to insulate the stud sufficiently by itself even if the asphalt were broken away and the space filled up with water. In still later designs we are insulating the studs by supporting them entirely upon a block of granite. The stud is faced with a plate of hard phosphor-bronze to take the wear, and this plate can be replaced when worn down.

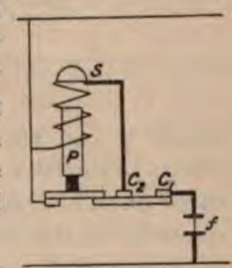


FIG. 13.

One very important matter in connection with surface contacts is the water-tighting of the switch-boxes. Our method is to keep the opening of the switch-box as small as possible, and to provide a very solid lid screwed down hard against a rubber washer placed in a suitable recess. This gives a very perfect joint. The lid of the switch-box is the stud itself. The switch is attached to the underside of the stud and comes away with it as a single piece. To renew a switch, all that is necessary is to take out the three screws which hold it down, withdraw it by a holder made for the purpose, and drop in a new switch and stud and screw the whole down. The operation can be performed in less than two minutes. On the form of the switch contacts we have experimented a good deal. We have found that a particular form of mercury contact is very satisfactory, but there is a prejudice against mercury contacts and so I show here a brush contact which answers its purpose admirably. The precautions to be adopted to keep a metal contact in good

condition permanently are as follows. In the first place provision must be made absolutely to prevent sparking upon the contact-surface. Secondly, a certain amount of friction must be introduced between the surfaces to keep them bright. The surfaces should be arranged to touch on as many points as possible, and yet there should be a substantial bulk of metal in the vicinity of the touching surfaces. A material should be employed which has good elastic properties, and in the operation of opening and closing all strains upon it must be far below the elastic limit. The cross-section of the material must, of course, be amply sufficient to carry the required current, and all causes of warming must be eliminated. If these precautions are adopted, a switch contact can be made to operate thousands of millions of times without injury. The switch has, of course, never to break the main current going to the car. It cannot open until the skate has been removed. It has, however, to break the current leaking from the stud and also the current taken by the magnetising coil, which is nearly half an ampere. To take the spark caused by this current we provide a contact between two arc carbons, D (in parallel with the main contacts), which does not break until the main contact has opened $\frac{1}{4}$ inch. This ensures the small spark always occurring on the carbon points. The switch being immersed in oil, this small spark is not pulled out any perceptible distance, and by providing a $2\frac{1}{2}$ inch break the switch is enabled to break currents of ten times the amount if it were necessary to do so.

The method of earthing the switch in the event of the yoke piece Y failing to open is seen at once from Fig. 10. The rod N is of copper, and has a collar on its upper end which will come in contact with the yoke piece Y whenever P moves down relatively to T. If Y and T should for any cause stick and keep the switch closed after the car has gone past, N in falling will connect the earthed mercury cup E with the yoke Y and blow the cut out. The mercury cup E is merely a hole drilled in the cast-iron switch box. The box is permanently earthed by being connected to the rails by a stout conductor. It is found that a mercury cup of this kind remains unchanged for years, but still it is well to put a check upon its efficiency as a safeguard. This is done in the following way. It will be seen from the diagram of

connections that one end of the shunt coil is connected to earth. The method of making the earth connection is to bring down a stiff conductor from the switch and make it dip into the upper portion of the mercury cup E. So long as that cup remains efficient as a connection to earth the switch will operate, but if for any reason (as for instance the box being cracked and the mercury spilt) the connection to earth is broken, then the shunt coil becomes inoperative and the switch cannot be closed at all. If, therefore, the switch will close at all, we are sure that the mercury cup E is full. In this way the risk of accident is reduced to the one risk of the plunger sticking in its tube.

It is therefore necessary to consider somewhat carefully what risk there is in the plunger sticking up after the car has passed. It will be noted that there are absolutely no side forces upon the plunger to cause friction against the inside of the tube. The clearance is so great that even the introduction of grit does not have any effect. As a matter of fact, under the worst conditions the force due to friction is less than $\frac{1}{4}$ oz. When the plunger and tube are polished and working in oil as in practice, we may safely take the force of friction as less than 0·01 of a lb. Now we can make the downward force upon the plunger when the iron skate is removed almost what we please; how great we shall make it is merely a question of economy in copper. We consider that a factor of safety of 200 is more than sufficient. We have adjusted the sizes of the iron heads of the plunger so that the downward force after the removal of the skate is $2\frac{1}{2}$ lbs. This of course becomes less as the plunger falls, but it still has a considerable value after the plunger has fallen $\frac{3}{8}$-inch and earths the switch. Thus we see that the chance of the plunger sticking is absolutely nil.

There is one other risk to guard against, and that is the breaking down of the insulation between the main and the stud. This is most effectually done by surrounding the switch by a sheath of metal which is permanently earthed and supporting all parts connected to the main on the earthed sheath, so that if their insulation breaks down there is an arc to earth and the fuse is blown.

Although the passage of the car from stud to stud is a continuous process, each switch being actuated by current derived from the last switch, it is necessary to have on the

car some means of making a stud alive if by any means connection with the main should be lost. For this purpose we carry on the car a small accumulator battery capable of yielding a current sufficient to work a switch at a pressure of 500 volts. Its capacity need only be small, so it can be very solidly and cheaply constructed, and as it can be charged direct from the line it is always ready for use. In practice we find that it is very convenient in use. The ordinary car controller can be adapted with an additional contact on it, so that when the handle is swung round to a certain position the accumulator is connected to the skate. By this means the driver can in an instant pick up again, if through any obstruction the skate loses contact with the main. It is a distinct advantage in a surface contact system for the current which operates the switches to have a high pressure behind it because it is able to overcome little obstacles such as mud and paper on the studs. For this reason switches requiring 500 volts pressure to operate them are preferable to switches operated by a low voltage battery carried on the car.

Throughout the whole of our work on surface contacts we have, of course, been aware that they could never be as simple in operation as the overhead wire, and that where the overhead wire is permissible, it is to be preferred on account of its cheapness if for nothing else. We have merely directed our efforts to see how far surface contacts could be perfected. We believe that future experience will show that the studs can be laid so as to cause no obstruction to traffic; that the method of picking up current from studs in the road is much more feasible than it at first sight appears, and that the safety can be made as great as in any other system of electric traction.

One thing that augurs well for surface contacts is the ease with which they can be worked in conjunction with the trolley wire. It will be possible to run a car from one system to the other without any more delay than is required to hitch the trolley on to its line. Throughout the experimental work of the last two years we have had the assistance of Mr. C. E. Holland, and some of the most valuable suggestions have been due to him. Had it not been for his continual efforts the experimental line at Willesden could not have been made such a great success.

Professor Carus-Wilson.

Professor CARUS-WILSON: I am sure that I am only expressing the general feeling of the meeting in congratulating the author of this paper, and those who have been associated with him, on the successful issue of the application of the principles that have been brought before us this evening to the difficult but important problem of electric traction by surface contacts. I should have hesitated to criticise the interesting contrivance which we have been shown, as it is at present in an experimental state; but since the author submits this paper with the object of getting expressions of opinion on the subject, he will understand that criticisms are not directed with any other feelings than those of goodwill and appreciation of the work that has been done already.

The author has not made as good a point as he might have done of some of the characteristics of his system. A serious objection to the majority of existing systems of surface contacts is that the leakage current is liable to close the circuit, and so to make the stud, or contact section, alive. But although this point is not alluded to in the paper, reference to the diagram on page 253 makes it quite apparent that leakage current cannot have any effect in keeping or making the stud alive. This objection is, therefore, entirely absent in this system.

As to the very important question of safety, I cannot conceive of anything that could be more safe than the magneto-mechanical contrivance which has been exhibited to-night. The demonstration of the author as to the practical impossibility of the plunger remaining up when the car is away from the stud, seems to be conclusive; and as far as one can see without having submitted the arrangement to the test of prolonged experience, I think that, on the score of safety, the author, is to be most heartily congratulated on the success of his efforts.

There are other elements besides safety which have to be borne in mind, some of which have been dealt with by the author. It is possible, I think, to sacrifice too much for the sake even of safety. I would allude particularly to the element of accessibility. Any electro-magnetic contrivance should be easily accessible for the purpose of inspection and repair, without interfering with the traffic. It is rather a drawback in this system that the studs can be opened only when the traffic is not passing, and that, as the author states in his paper, it takes about two minutes to open one of them and get at the switch.

As to the question of reliability. I cannot imagine anything getting on the stud to interfere with the picking up by the skate except ice; but in this country, that is not a very important objection.

The author has given up mercury contacts, on the value of which great stress was laid in the paper read at the British Association last year. I hoped that he would have had the strength of his convictions and would have continued the use of the mercury contacts in spite of the prejudice that exists against them.

With reference to the question of sparking, the author is quite right in stating (p. 255) that—"In the first place provision must be made absolutely to prevent sparking upon the contact surface." The success of any system of surface contacts will largely depend upon being able to get rid of sparking at the contacts. Given a conductor passing over

Professor Carus-Wilson.

successive studs connected to the main cable by conductors containing switches and electro-magnetic contrivances; the problem is to collect the current from each one of these studs, without getting sparks at the switches. This problem is peculiarly like that of commutation, and if we cannot get rid of the sparking at the switches, I agree with the author in thinking that there is a very serious defect in the system.

The author states on p. 255 that "the switch has, of course, never to break the main current going to the car." I do not want to dispute this statement altogether, because I have not had the opportunity of examining closely the working of the model before us; but there is no explanation in the paper to show why this should be so. Suppose that the skate is resting equally upon two studs, we shall not get the same current passing up from each. There is always a certain amount of self-induction in the switch, and the result is that there will be a larger proportion of current passing from the rear stud to the skate, than from the forward stud. Hence there will be a considerable current to be broken, either at the surface of the stud or at the switch, and the question is at which will the break take place first? It seems to me that the answer to this question depends entirely upon the rate at which the plunger falls, and I should like to ask the author to experiment with the model before us, by drawing away the skate in a horizontal direction, to show how far off the stud the skate must be before the plunger will fall. If the plunger falls before the skate has left the stud the circuit will be broken at the switch. No doubt the author will be able to give us information which will settle the question.

Mr. Sharp.

Mr. S. SHARP: I have, during the course of some years, had experience in experimental work in regard to tramways. I assisted in the examination and report upon the Lineff system, and since then I have been working experimentally on the Gordon and the Pringle systems. But of all the surface contact systems we have yet seen, the one under discussion strikes me as being by far the best.

With regard to the last point raised by Professor Carus-Wilson, that of the break of the current when the skate passes from one stud to another, I think his fears are misfounded, because, as I understand the explanation, the plunger cannot fall until the skate has quite broken the contact with the stud. If I interpret it rightly, the current is still passing through the shunt, and holding up the plunger until the skate has quite left the stud, and therefore necessarily the spark must take place on the surface of the latter, and not in the switch itself. I do not believe the question of sparking to be a very important one, occurring where it does, and especially as then there is another stud in circuit.

Having seen this system in operation at Willesden about three or four months ago, and having spent a considerable time there, examining the working of the switches and seeing the ease with which they were removed and renewed if required, I think the switches are among the simplest and most easily accessible of those used in the systems of surface contact which have yet been made public. In a very few seconds an attendant, by simply removing three screws from the surface of the stud, can take out the switch and put in a new one; the switch can then be taken away to be examined at leisure. Even

Mr. Sharp. throughout a long system, if the switches should require examination or renewal, say every month, it could be done without very great loss of time, or without hindrance to the traffic. Indeed, the system might be kept in good working order without interruption of traffic, and without delays and breakdowns.

The principal danger of surface contacts, hitherto, has been the possibility of studs or rails being left alive; but this is, I think, entirely obviated in Mr. Walker's arrangement. A break dependent entirely on gravity is hardly likely ever to fail; there being no mechanism or springs to get out of order, the weight exercises its effect and the switch breaks.

Of course these things will have to be proved by actual experience in hard daily work; but what I have seen on the experimental line appears convincing, and I believe that when the system gets into every-day work with heavy traffic it will still be able to hold its own.

There are many places in which this system could be used, where a conduit system would be impossible. I may quote Argyle Street, Glasgow, as an instance. For about two miles the Caledonian railway passes under this street and line of streets with only about 10 inches between the surface of the road and the top of the railway-arch. That is one of the reasons why the Glasgow Corporation were unable to adopt a conduit system, and had recourse to the overhead system with its attendant disfigurement of the streets. I believe Mr. Walker's system would ordinarily require more than 10 inches beneath the surface of the road, but no doubt he could so modify his arrangements as to be able to supply what was needed in Glasgow, if that city should ever wish to adopt his system. I must conclude by congratulating Mr. Walker and those who have been associated with him in what I think may some day be a great success; and, for the sake of the surface contact systems in preference to overhead lines, I very much hope it will be successful. I have a very great sentiment against towns being disfigured, I will not say by trolley wires when they run on poles and parallel to the line, but against towns being disfigured by span wires, especially where they guide the trolley wires round corners.

Professor Ayrton. Professor AYRTON: As Mr. Walker has intimated, there are, in addition to the well-known trolley and conduit systems, two systems of electric traction, namely a sub-divided conductor system in which the road conductor is divided into a number of sections, each fairly well insulated from the next and from the ground, and the so-called surface contact system, with studs along the road which are stationary except for an up and down motion and with which contact is made by means of a piece of metal now called a skate and carried by the car. The two latter systems, the sub-divided conductor and the surface contact system, although not called by that name, form the subject of the patent taken out by Professor Perry and myself eighteen years ago. It did not attract very much attention at the time, although I think it is generally admitted that it started the idea. According to the English Patent law, the idea of a surface contact or the sub-divided conductor is, after eighteen years, open to every one, and all that can be done at the present day is to make it successful. It is a question, therefore, of

detail, not of fundamental principle, that has to be considered to-night. Now, I do not want, in saying a few words about Dr. Thompson's and Mr. Walker's system, to be led into either of two errors: I do not wish, on the one hand, to take too rosy a view, as I might be led to do, because nobody longs more than I, as one of the originators of the system, to see a surface contact system of electric traction really successful; nor, on the other hand, do I wish to be so English as to imagine that anything which has not been done cannot possibly be done.

Professor Ayrton.

We were delighted when we heard, or read, the account given by Professor Thompson of this perfected system at the British Association Meeting in the autumn of last year at Bristol. Now, the first thing, I am sorry to say, that struck me was that it was not as perfect as Dr. Thompson had hoped, and as the world might have imagined on reading the published account. My first criticism of the system as described in the full paper is that if through dirt, or a piece of paper, or whatever it might be lying on top of the stud, a skate failed to make contact, it was impossible not only for the car to move on, but for any other car behind to come after it. The whole line behind would become absolutely blocked mechanically by the failure of the skate on any one car to make contact with any stud. Also if, in the generating station, the main fuse went for an instant, even if, in some automatic way, it was rapidly replaced by another fuse, the whole line would become dead; and it would be absolutely impossible for any car to regain current when the central station sent it out, because all the plungers would have fallen and there was nothing on the car, and no possibility in the system, whatever the central station might do and whatever the cars might do, as described in the paper, to make them alive again, so that the studs should obtain current and transmit it to the cars. I do not know who pointed this out at the time—I certainly did not publicly—but at any rate we were then answered that the car carried a battery of accumulatorsof 500 volts. Of course that gets over the difficulty; but they were not referred to in Dr. Thompson's paper. It is to my mind a distinct disadvantage to the system, that it is not completely automatic in itself, but requires each car to carry a battery of accumulators giving 500 volts, so that if from dirt or ice, or a piece of paper, or whatever it may be, the skate fails to make contact, then this contact-box has to be made alive again by the car using its battery of accumulators.

The next thing that struck me in examining into the system was the existence of, to my mind, certain serious defects in the mechanical part of the contact-box; but to-night we are shown a contact-box which differs in many particulars from that described in the British Association paper. In the first place, as Professor Carus-Wilson has said, the mercury contacts, on which such stress was laid in the paper, have been entirely abandoned in only a few months, although the system was supposed to be perfected. Secondly, the way in which the contact-maker is moved is materially altered in the arrangement put before us to-night. Why should a system which was so perfect and so good a few months ago require to be entirely changed if the success of the

Professor Ayrton. thing is the detail? I have not had time to study the new contact-box described in the paper of to-night, or to give sufficient consideration to it to know what its defects are.

The question of leakage seems to me to be rather a serious matter if we are to have the possibility of a leakage of five amperes at 500 volts per car; for it is stated on page 247 of the paper that, with bad road conditions, the leakage may possibly approach 5 amperes—it is 4·5 amperes in the table. It may be, therefore, about 5 amperes, and this current at 500 volts is 2½ kilowatts. I wonder what the Board of Trade would say to 2½ kilowatts per car leaking to earth. The second part of the same paragraph (on p. 247) enables one to calculate how much power is supposed to be given out at the central station. It says: "Taking only five fine days to one wet one, the average leakage will be under a quarter of an ampere per car, or about 2 per cent. of the total power of the station." So that with 5 amperes leakage per car, something like 40 per cent. of the whole output of the station per car may be leaking to earth. Even if we disregard the question of the corrosion of pipes, this waste of power is alone a very serious matter, and I think requires most careful consideration before we can say that the system is one that is ready to put on the roads which, as we hope, the County Council are shortly going to run electrically on the south side of the Thames. I feel very ungrateful in what I am saying, as Professor Thompson spoke so generously of my colleague and myself in his paper at Bristol. I have, however, another feeling, namely, that I should not like the County Council or any other public body in this country to adopt a system under the idea that every detail had been worked out and that success was guaranteed (a system which really, however, as the last six months has shown us, is simply in the beginning of an experimental stage), and then for that body to have to say later on that Ayrton & Perry's old contact system is a failure after all.

Mr. Lawson. Mr. A. J. LAWSON: I should like to ask for information with regard to the width of the skates and the amount of curve permissible with a system of this kind, without putting the contact studs very closely together. It is more than possible that, with a very sharp curve, even if the studs were close together, the skate might make contact with the rail. Then with regard to the amount of leakage. Mr. Walker states that we might take it on the average at one half an ampere per car. This with forty cars would be 10 kilowatts. I should be very glad indeed to supply such a system as this with current, metering the units going out at the central station, for there would be 58,400 units leakage per year with the forty cars supposed. On an extended system such as the South London Tramways, the loss would be very considerable indeed.

Now with regard to capital expenditure on a system of this kind, I have some experience of the streets of London in putting down lighting mains, and I know that there are almost as many difficulties to be met with through want of room as there are in the case of Argyle Street, Glasgow, instanced by Mr. Sharp. I should like to know what would be the cost of the removal of the 48, 36, 24 and 16

inch pipes of the gas and water companies, in order to make room for these plungers between the rails. I wish here to protest against the sentimental rubbish so often stated with regard to the disfigurement of the streets of London by overhead wires, especially when the sentiment practically defeats the application of electricity to the propulsion of cars on the tramways in London. Mr. Lawson.

Mr. EUSTACE THOMAS: With regard to this system of surface contacts the matter is less a question of the general principle adopted, which is very pretty indeed, than of the way in which the details are worked out, so as to make it a practical success. I cannot help doubting whether the details have yet reached an approximately final stage. Thus, for one thing, a very long distance is taken between the studs; this is a small matter and could easily be remedied, but it serves to illustrate my point. Mr. Thomas.

With regard to the question of crossings, in going over a crossing with a large amount of mud on the road there would be enough leakage at first to start an arc which would blow the fuses in the studs. This would not stop the line, perhaps; but it would be a serious objection in practical working. It might to a great extent be avoided by using a second set of studs connected to a negative distributor. The suggestion of insulating the portions of the rail lying between the main rails on the track on which the car is travelling has been made; but that would be difficult to do in practice efficiently from both mechanical and electrical points of view.

There are many details in the switch which do not appear to be quite practical at this stage. In the first place, it has been decided, as far as we can judge from the paper, to work with the switches underneath the stud. Those who have used surface contact systems have, I think, come almost universally to the conclusion, that it is necessary to remove the switch from the stud and put it in a special vault, and I cannot help thinking that perhaps the next stage will be that the author will modify the system so as to remove the switch from the stud. As illustrated, there are two joints, one of which is made between two pieces of metal with micanite between, it being necessary to insulate the two pieces of metal from one another. When we remember that cart wheels are liable in crossing to strike heavy blows on these studs, and that this may occur very frequently; and when we also remember that there is, to a smaller extent, a hammering of the plunger inside, it is to be expected that trouble will occur with the micanite. There must, at any rate, be always a difficulty in trying to keep a material of this sort in good order over a long period, even although it may be able to take a steady pressure of 15 tons without damage. Then, in consequence of the hammering of micanite or of any other insulating material that can be put in, the tops would be likely to get loose in time, and constant supervision would be required to prevent water getting into the switch. Every precaution appears to have been taken to prevent any water that may get in doing damage; but it is certain that, if it does gain admission, the precautions taken will not eliminate trouble. Then

Mr. Thomas. taking the switch as it stands, the yoke is as likely to stick to the lower portion of the coil, as the plunger to stick to the yoke. If that were to happen, the fuses would frequently require renewal.

One very great difficulty, which it may be difficult to surmount in this system, is that a shunt coil is used ; and it is necessary after every contact with a stud, that the current should be broken. Those who have worked with electric motors, with all the experience that has been gained in connection with the insulation of the magnet coils, know what a great difficulty there is, even when one is working with fairly large coils and is not hampered for space, in managing to break the circuits without getting breakdowns. That has to be guarded against in all shunt coils, and it will be necessary to take great precautions to ensure safety in working.

I repeat, that the whole thing comes to this : it is a very pretty system in principle, but it has to be proved whether or not under practical conditions, working on ordinary roads, it is possible for it to become a commercial success.

Mr. Cozens-Hardy. Mr. E. H. Cozens-Hardy : The author has shown such ingenuity in getting over the ordinary difficulties in surface contact systems due to leakage current magnetising the switch, that under ordinary conditions it is difficult to say how the plunger could be raised ; but the safety of such a system is determined by extraordinary, rather than by ordinary, occurrences, and it seems to me that there are certain conditions under which the magnet might hold up the plunger. In the first place, alluding to Mr. Thomas' remarks as to the insulation of the coil, if part of the coil became short-circuited it is conceivable that when the plunger was up it might be out of balance, due to disarrangement of the coil, and so be held up although there might not be enough to draw it up initially. With regard to leakage, the amount depends to a very large extent on the behaviour of the horses on the particular part of the road in question. As a result of actual experience of surface contact systems with lengths of rail in stone setts, the leakage at 500 volts from a 2-ft. length of rail has, under certain conditions, risen to 20 or 30 amperes. The conditions under which I measured the current were that the liquid from a stable drain was poured out over the rail in the centre. Such conditions, though not of ordinary occurrence, are always liable to arise, and the trouble from such a source would last for some considerable time. The vestries take care of the solid form of street refuse, but they cannot remove the liquid, and the result is that a leakage of 20 amperes may go on for some considerable time from a stud.

With regard to the studs, I do not know how long the plunger takes to fall, but it seems to me that if it requires an appreciable time, say a little less than a second, to do so, a heavy dray with a large iron tyre crossing the track immediately behind the car might provide the necessary piece of iron for the plunger to attract, and so keep the stud alive. Then, in Paris, they have had considerable difficulty with the wear of brass studs, and have found that their ordinary life is less than six months ; but, if they are of iron, very considerable difficulty will be experienced, I imagine, on account of the plunger sticking.

The question of the insulation of the rail is another very important matter, but no mention has been made of the manner in which it is proposed to carry it out. Mr. Cozens-Hardy.

Mr. G. C. Sillar: I can confirm the last speaker's statement with regard to the excessive leakage that takes place in connection with the surface contact system he has spoken about. I think he underestimated the amount, when he said that from a 2-ft. rail there was rather more than 20 amperes leakage with 500 volts pressure, under the conditions he mentioned. Mr. Sillar.

I agree with Professor Ayrton that it is by the practical application of the principle of the surface contact, that the success or otherwise of the system will be determined. If the author comes to consider the details of a car running, under practical working conditions, and taking ordinary curves and ordinary gradients over bridges, he will find very great difficulties in dealing with the skate. I have had experience in connection with this, and, I may say that, in ordinary car-building work, it is customary to allow not less than 3½ to 4 inches from the roadway to anything on the underside of the car to clear obstructions; and in electric tramway work even that amount is not always found quite sufficient to clear the bottom of the motor when travelling over bridges of short, sharp gradients, or when turning corners and sharp curves. On a car with a skate of about 16 feet from point to point of support, I have found that there is a side-swing of from about 2 feet to 2 feet 4 inches. I am quoting entirely from memory and cannot be quite certain of my figures; but, practically speaking, in order to cover a surface contact stud throughout the length of the car, on a curve of something like 30 feet radius, it would be necessary to have the skate about 4 feet wide. I do not think that would be practicable, and in the case that I have in mind, we had to introduce some fanciful arrangements in order to make the thing practicable at all.

Professor R. H. Smith: May I ask Mr. Cozens-Hardy if it is not a fact that the studs in Paris to which he referred, and which he said lasted about six months, stand nearly an inch above the ground? Professor Smith.

Mr. Cozens-Hardy: No; they project half an inch. Mr. Cozens-Hardy.

Professor Smith: I do not think ordinary quarter-inch studs can possibly last six months, and on that line, I believe, there are no sharp angles. Professor Smith.

Mr. E. Tremlett Carter: The question of the mechanical difficulty with regard to the collision of the skate with the projections in the roadway, could easily be determined by putting a skate on an ordinary tramcar and trailing it about. This would very soon show whether the humps and hollows in the Holloway Road would affect the skate. Mr. Carter.

I can testify to the rapidity with which the studs can be removed, for inspection or repairs, because I have timed the removal of one of them on the experimental line, and found it to be a matter of a very few seconds. But if the stud should happen to be under slush or water at the time of the removal, the cavity, even in these few seconds, would be filled with water and mud and stones, and I should like to ask how they are to be removed? It seems to me, looking at the latest pattern

Mr. Carter. of box, that a lot of water will run in unless it is quite filled up with oil, and even then stones or some mud would sink down, and there can be no provision for trapping those into the drains because the box is filled with a fluid oil. I think it is the most practical form of surface contact that has yet been devised; and my impression, after seeing it at work at Willesden, was that it is a system which ought to have a fair trial on a short length of public highway.

Professor Wilson. Professor E. WILSON [*communicated*]: In offering the following remarks, I should like to compare the Thompson-Walker system with two other surface contact systems. The first is that in which an external magnet carried by the car is the only means employed to lift an underground iron bolt and thus establish a contact between a surface-button and the supply feeder. I will refer to Diatto's system in which an iron bolt is guided vertically and floated in mercury. The rubbing skate or shoe on the car carries a series of electro-magnets, and these act upon the bolt, causing it to rise and establish a contact. Diatto's system resembles the Thompson-Walker in that it has only one row of studs, but the two systems are totally distinct in this respect, that in Diatto's only the external magnet is required to operate, whereas in the Thompson-Walker the presence of an external mass of iron as well as a magnetising current in the underground coil are required. The Thompson-Walker system has, then, the advantage that when the current is stopped and the iron skate removed by the passage of the car, the whole force of gravity acts upon the bolt for the purpose of severing the contract; whereas in Diatto's system the force on the bolt is the difference between the forces due to gravity and the upward pressure of the mercury; moreover, Diatto's system has not the ingenious short-circuiting device possessed by the Thompson - Walker system. Suppose it be demonstrated that these two systems are equally good with regard to public safety, reliability of working, and power taken to work the switch; then the questions of first cost and maintenance have to be considered. It must be remembered that in a large system a certain cheapening per switch underground means that percentage gained on a large proportion of capital expended—whereas the doubling even of the cost of an apparatus in the car is not serious, since the cars are few compared with the number of underground switches. For instance, in the Thompson-Walker system there is a magnetising coil underground, whereas in Diatto's there is not. Would this make much difference in the first cost of the switch?

The other system I wish to mention is that in which only a magnetising current is employed to lift the underground bolt and establish a contact. No external magnet is required. The Wheless and Johnson-Lundell systems may be mentioned. In each of these, two rows of surface studs, or their equivalent, were employed, although I am informed that Wheless now works with one row. In these systems the public safety is reduced to the falling of a bolt under the force of gravity, but only the one agent—namely the magnetising current—is required to actuate the switch. Further, two magnetising coils are wound on each switch in these systems, but neither of them employs the very ingenious method adopted in the Thompson-Walker

system of severing connection from the underground feeder by melting a fuse in the event of the bolt falling and the switch not severing contact. Professor Wilson.

With regard to the adoption of surface contact systems in practice. In places where the authorities will not have an overhead wire, for instance, in the middle of a large town, there is the choice of using (1) a slotted conduit system, (2) a surface contact system, (3) batteries. In such a position as this, reliability of working is of the utmost importance, since a breakdown would be very serious indeed. Public safety and reliability are the important points, and the authority would be badly advised to put in a system on account of its being cheaper, unless it was equally as good from the other points of view. On the other hand, if the systems are equally good as regards public safety and reliability, the question resolves itself into one of cost, by which is meant first cost, cost of supervision, maintenance, &c., and cost of power to work, and leakage. The change over from slotted conduit to overhead is easily and quickly effected, and batteries are favoured in places. It would have added to the interest of the paper if Mr. Walker had given the cost, say, per mile, so that a comparison with the slotted conduit and battery systems could have been made.

The CHAIRMAN: I will now call on Mr. Walker to reply to the criticisms that have been made; and perhaps I may take this opportunity of expressing very briefly that, while it is known to everybody that I am deeply interested in the particular matter which has been brought before the Institution to-night, I have left to my colleague, Mr. Walker, the entire responsibility of preparing this paper, and of placing it before you, and I leave entirely to him the responsibility for any reply. I have listened with intense interest to the discussion, to all the terrible things that ought to happen when we get to work, the skates that must be 4 feet wide, and the leakage that must be 50 per cent. of the total output of the station. And I listened with some amusement, because it has fallen to my lot to spend time and money on actually trying whether any of these dreadful things do, in fact, take place. The results of the experiments you have had narrated to-night by Mr. Walker. The Chairman.

Mr. M. WALKER, in reply, said: I wish to thank those who have spoken, for their comments and criticism. I think that I shall be able to give satisfactory answers to the objections that have been raised, though the time does not permit my entering into some of them as fully as I should like. Mr. Walker.

Professor Carus-Wilson thought that the leakage might affect the current supplied to the shunt. But as the stud (connected direct to the main) is maintained at 500 volts above earth, no amount of leakage can affect the shunt current. The liability of the switch to open just as the skate is being removed and before it breaks connection with the stud is obviated by bending up the end of the skate as shown in Fig. 6. The electrical connection is broken before the iron is moved away far enough for the switch to open.

I can heartily congratulate Professors Ayrton and Perry on being the first in the field with sectional conductors. It is their misfortune that they were so much before the times. Professor Ayrton need not feel

Mr. Walker. that his remarks are ungenerous, because we are glad to have his criticism. I think he is mistaken as to the question of accumulators on the car. The desirability of carrying on the car a small accumulator for the purpose of picking up was certainly mentioned at Bristol.[1] We did not state what the voltage of the accumulators should be, because it was at that time (and, in fact, still is) uncertain what voltage is required. A 500-volt accumulator certainly has drawbacks.

But in some of our designs the switch only requires 100 volts to pick up, and there is no great objection to a 100-volt accumulator, especially as it is common to carry a considerable battery for the purpose of lighting. Besides, there are well-known methods of transforming which would only require a few cells, and even these can be dispensed with if we prefer to carry a small generator somewhat larger than that used in the Evershed testing set. The closing of the switch is so quick that even under the worst conditions of leakage the amount of energy required to operate it is too small to present any practical difficulty. It is not true that we have abandoned mercury contacts. On the contrary, we find them more reliable than any other contact we know of. But there is an enormous prejudice against mercury contacts. This is mainly in the minds of those who have employed them for purposes for which they were unsuited. We wish to show that the mercury contact is not, by any means, an essential of our system, and for that reason I show here to-night a switch without mercury contacts which does its duty perfectly. I am content for the moment to leave a debatable question behind. Nevertheless, the switches on our experimental line have mercury contacts, and as an answer to the statement that our designs have been changed, I may say that, with the exception of two boxes that were reserved for experimental work, our switches at Willesden were designed more than eighteen months ago, and have been doing their duty well ever since they were first put in their boxes. Of course we have had dozens and dozens of designs, and several of them have been made up and tried, but it cannot be said for that reason that we are not certain of our ground. One might as well say that the dynamo was an unreliable machine because engineers have been improving it for the last twenty years. There is the switch upon the table : we ask you for your criticism of it as it stands.

With regard to the question of leakage, I have only to say that the figures given in the paper were taken for the purpose of ascertaining what the leakage would be. We had actual mud taken from a road, and the current was measured with a Weston ammeter. It is unfair to say that the figures are low and that seven or eight amperes would be nearer the mark, unless that statement is based upon experiment. The case cited of a 10-ampere leakage from a 2-ft. rail is a corroboration of my figures. The leakage depends mainly upon the perimeter of the conductor. Our studs have a perimeter of 15 inches ; the rail had, say, 54 inches. My measurements point to a leakage of 8

[1] On referring to the printed British Association paper, I find that the method of picking up is not described, so that Professor Ayrton was right. The use of accumulators was, however, described at Bristol as well as in other accounts of system published previously.

or 9 amperes from the rail, and it is admitted that the conditions were very bad indeed. It does not give one a clear idea of the importance of leakage to sum up the amount of kilowatt-hours wasted in a year in a large tramway system. The right way to evaluate it is as a percentage upon the whole power. In electric lighting accounts one usually notices in separate columns two items, "total units generated" and "total units sold." My recollection is that these generally differ by much more than two per cent. Mr. Lawson says that he would like to be paid for all the power wasted by our surface contacts. We should have no objection to pay him for that power; it would come to much less than the power wasted in carrying heavy accumulators. Our scheme would then have a feature to commend it to him, at any rate, more highly than schemes involving "total units unsold." The question of injury to pipes hardly arises, because the leakage that occurs is only from the stud under the car, not from all the studs, and being a surface leakage goes direct to the rail. Mr. Walker.

Mr. Thomas's remarks were very much to the point, and I will proceed to answer the objections he has raised. It is not right to assume that our system is defective because we have a great distance between the studs, while other experimenters on surface contacts have employed shorter distances. Other experimenters have been hampered with the difficulties of interconnections and the necessity of covering a stud in advance. The very object we had in view in our magnetic contrivance was to have a perfectly free hand with regard to the method of connecting with the main and the spacing of studs. We are enabled by the use of shunt coils to place the studs far wider apart than is possible in any series system. So far from thinking that 15 feet is too great a span, we propose to increase the distance between studs for bogie cars to 25 feet or more. As Mr. Thomas says, the risk of a "short" to rail is one that can be met. I do not think that an arc will start through an inch of wet mud, but if in practice it was found occasionally to happen, it would be an easy thing to place a brush, which could be depressed at will, in front of the skate so as to remove any deep mud. The question whether it is better to put each switch in a separate box or group several switches in the same box is a debatable one. I have not time to enter into all the pros and cons. The matter is only of importance from the point of view of economy. The permanent insulation of the stud is a matter of vital importance. It is only by long experiment in the street that we shall be able to find out what will last and what will not. We believe that the micanite rings screwed down with great pressure between broad metal surfaces will stand the street traffic. We, however, suggest an alternative method, namely, the encasing of the studs in solid blocks of granite. We have been obtaining estimates of the cost of this, and find it does not add greatly to the cost of the boxes. The possibility of the yoke sticking is one which does not arise, because there is not as much fluid damping between the yoke and the frame as there is between the plunger and the yoke. There is no great rise of voltage upon the breaking of the shunt current, because the mutual induction with the solid brass bobbin upon which the shunt is wound nearly wipes out the self-induction of

Messrs. R. P. Brousson and J. P. Milne were appointed scrutineers of the ballot for new members.

Donations to the Library were announced as having been received since the last meeting from Messrs. Alabaster Galehouse and Company, and the Electrician Printing and Publishing Company ; to whom the thanks of the Meeting were duly accorded.

The CHAIRMAN : It is with great regret that I have to announce the death of Mr. Peter Christian Dresing, engineer of the Great Northern Telegraph Company, Limited, who has, since 1890, acted as our local honorary secretary and treasurer for Denmark.

The Committee appointed by the Council to consider and report as to the desirability of securing uniformity in certain branches of Electrical Engineering, has made an interim report which I will call upon the secretary to read.

The SECRETARY read the following interim report of the Uniformity Committee :—"This Committee has given very careful consideration to the question of the adoption of a uniform standard of frequency or periodicity in alternate current working. It has collected information and has ascertained the opinions of a number of manufacturers and of consulting engineers on the Continent and in America, as well as in this country. It is unanimous in recommending the adoption, for general purposes, of a frequency of 50 periods per second, as on the whole the best suited to ordinary cases of mixed distribution of alternating currents for glow-lamp, arc-lamp, and power supply. For the exceptional purpose of transmission of power on the large scale, it recommends the standard frequency of 25 periods per second. It also suggests for the exceptional cases of house-to-house tranformer supply as preferred in districts where the houses are scattered, a standard frequency of 100 periods per second."

The CHAIRMAN : I will now ask Mr. Marconi to read his paper on Wireless Telegraphy.

WIRELESS TELEGRAPHY.

By G. MARCONI, Member.

"WIRELESS TELEGRAPHY," or telegraphy through space without connecting wires, is a subject which has attracted considerable attention since the results of the first experiments I carried out in this country became known. It is not my intention this evening to give my views on or discuss the theory of the system, with which I have carried out so many experiments, and by means of which I have worked various installations, but I hope to put before you some exact information of what has been done by myself and my

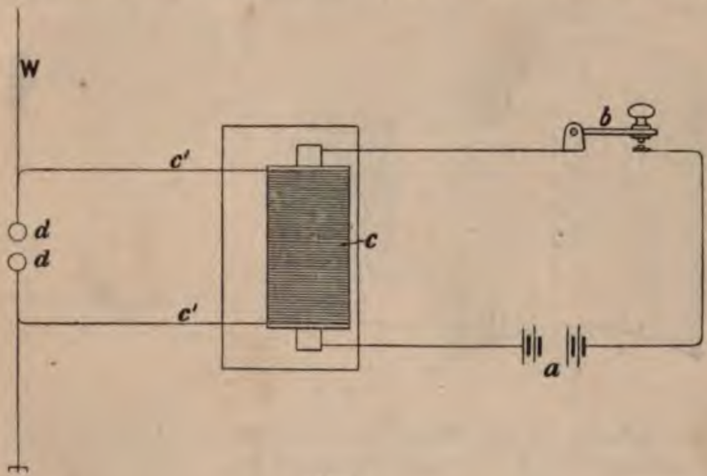


FIG. I.

assistants during the last twelve months, and also some reliable data as to the means employed to obtain such results. Much has been published on the subject, I must say with varying accuracy, and there can hardly be any one here altogether ignorant of the general characteristics of the system.

Before I go into this subject further I wish to state that any success I have met with in the practical application of wireless telegraphy has been in a large measure due to the efficient co-operation which has been rendered by my assistants.

I think it will not be out of place if I give a brief description of the apparatus.

TRANSMITTER.—When long distances are to be bridged over and it is not necessary that the signals should be sent in one definite direction, I employ as transmitter an arrangement as shown in Fig. 1, in which two small spheres connected to the terminals of the secondary winding of an induction coil *c* are connected, one to earth and the other to a vertical conductor *W*, which I will call the aërial conductor.

Should it be necessary to direct a beam of rays in one given direction I prefer to use an arrangement similar to a Righi oscillator placed in the focal line of a suitable cylindrical parabolic reflector, *f* Fig. 2. The transmitter works as follows:—When the key *b* is pressed, the current of the battery is allowed to actuate the spark coil *c* which charges

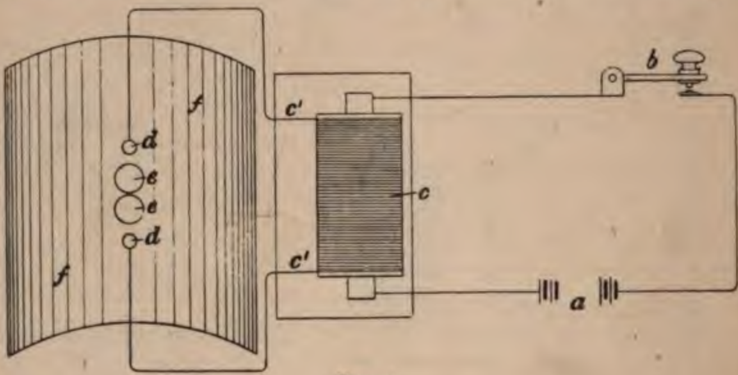


FIG. 2.

the spheres of the Righi oscillator or the vertical wire *W* which discharges through the spark gap.

This discharge is an oscillating one, and the system of spheres and insulated conductor becomes a radiator of electric waves. It is easy to understand how, by pressing the key for long or short intervals, it is possible to emit a long or short succession of waves, which, when they influence the receiver, reproduce on it a long or short effect, according to their duration, in this way reproducing the Morse or other signals transmitted from the sending station.

RECEIVER.—One of the principal parts in my receiver is the sensitive tube or coherer or radio-conductor, which was discovered, I think I am right in saying, by Professor Calzecchi Onesti, of Fermo,[1] and was improved by Branly,

[1] See *Nuovo Cimento*, series 3, vol. xvii., Jan.–Feb., 1885; and ditto, Jan.–Feb., 1896.

and modified by Professor Lodge and others. The only form of coherer I have found to be trustworthy and reliable for long distance work is one designed by myself as shown in Fig. 3. It consists of a small glass tube, four centimetres long, into which two metal pole pieces, j^1 j^2, are tightly fitted. They are separated from each other by a small gap,

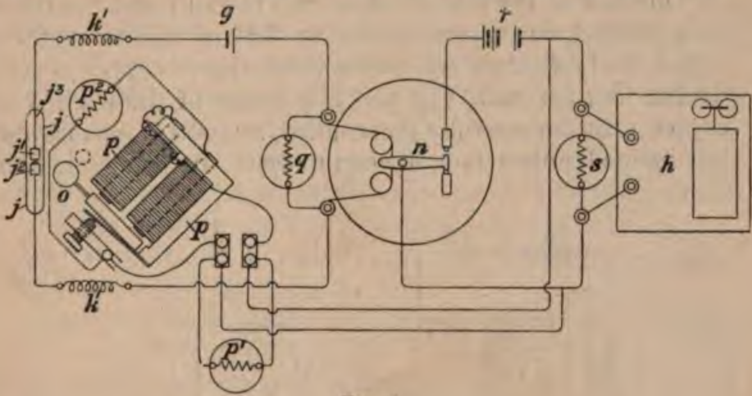


FIG. 3.

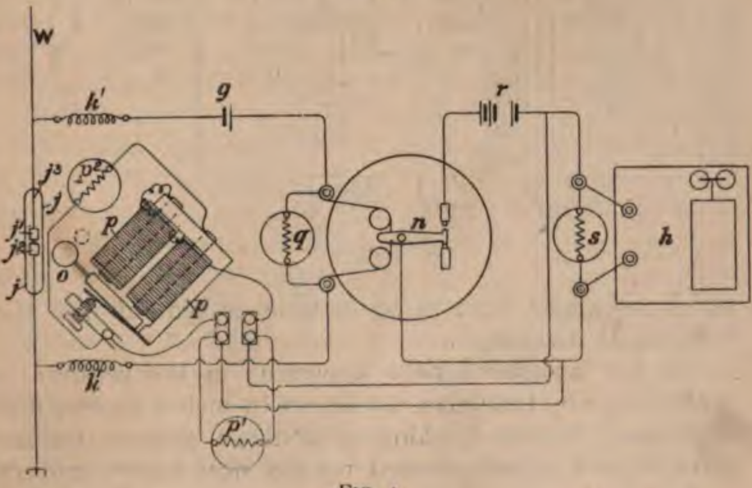


FIG. 4.

which is partly filled with a mixture of nickel and silver filings. This coherer forms part of a circuit containing the local cell and a sensitive telegraph relay actuating another circuit, which circuit works a trembler *p* or decoherer and a recording instrument *h*.

In its normal condition the resistance of the filings in the

tube j is infinite, or at least very great, but when the filings are influenced by electric waves or surgings, cohesion instantly takes place, and the tube becomes a comparatively good conductor, its resistance falling to between 100 and 500 ohms. This allows the current from the local cell g to actuate the relay n.

One end of the tube is connected to earth and the other to a vertical conductor similar to that of the transmitter Fig. 1, or if reflectors are used a short strip of copper is connected to each end, Fig. 5. The length of these strips of copper must be carefully determined, as good results cannot be obtained unless they happen to be of the proper length,

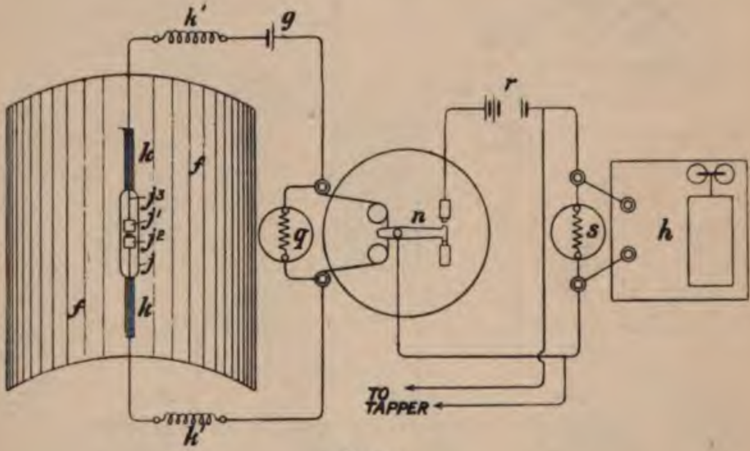


FIG. 5.

which will cause them to be in tune or syntony with the transmitted oscillations.

All the electro-magnetic apparatus in the receiver is shunted by non-inductive resistances in such a manner that there may be no sparking at contacts and no sudden perturbations or jerks caused by the local battery current near the coherer.

I find that the relay tapper and telegraphic instrument, if not properly shunted, produce disturbing effects, the result of which is to prevent the coherer from regaining its sensitive condition after the receipt of electrical oscillations.

No such trouble is experienced when suitable shunts are used, and I attribute to their action in very great

measure the success which has been attained with this system.

Small choking coils *k'* *k'* are introduced between the coherer and the relay. They compel the oscillating current due to the electric waves to traverse the coherer rather than waste its energy in the alternative path afforded by the relay.

The oscillations induced on the strips *k k* or aërial conductor *W*, which acts as resonator, by the radiation from the oscillator affect the sensitive tube. This effect on the tube consists, as we have said, in a great increase of its conductivity, thus completing the circuit and allowing the current from the cell to actuate the relay. The relay in its turn causes a larger battery *r* to pass a current through the tapper or interrupter *p*, and also through the electro-magnets of the recording instrument *h*.

The tapper or trembler is so adjusted as to tap the tube and shake the filings in it. If in the instant during which these various actions take place, the electrical oscillations had died out in the resonator, the shake or tap given to the tube by the hammer *o* would have restored it to its normal high-resistance condition, and the Morse instrument or recorder would have marked a dot on the tape, but if the oscillations continue at very brief intervals, the acquired conductivity of the tube *j* is destroyed only for an instant by the tap of the trembler, and immediately re-established by the electrical surgings, and therefore the relay tapper and telegraph instrument are again actuated, and so on until the oscillations from the radiator have ceased.

The practical result is that the receiver is actuated for a time equal to that during which the key is pressed at the transmitting station. For each signal, however short, the armatures of the relay and tapper perform some very rapid vibrations dependent on each other. For it is the action of the relay which starts the tapper, but the tapper by its action interrupts the relay.

The armature of the Morse recording instrument being rather heavy, and possessing a comparatively large inertia, cannot follow the very rapid vibrations of the tongue of the relay, but remains down all the time, during which the rapidly intermittent action of the receiver lasts. In this way the armature of the inker gives a practically exact reproduc-

tion of the movements of the key at the transmitting end, dashes coming out as dashes, and dots as dots.

Much has been said and written about coherers being very unreliable and untrustworthy in their action, but I must confess that this has not been in any way my experience. Provided a coherer is properly constructed and used on a suitable receiver, it is just as certain in its action as any other electrical apparatus, such as an electro-magnet or an incandescent lamp. I have coherers which were made three years ago that are now quite as good if not better than they were at that time, and we have had tubes working for months in most important installations without ever giving trouble. At the installation my Company have erected at the South Foreland Lighthouse, which, as you probably know, is working to the East Goodwin Lightship, the coherer was mounted on the receiver when we first started in December of last year, and has done its work in a most satisfactory manner ever since.

I must call your attention to the object and function of the vertical wire *W*. It has been by means of this addition to the apparatus, that we have been able to telegraph over distances which have been so far unattained, I think I am right in saying, by any other method of space telegraphy. The way I came to appreciate the great importance of the addition of the conductor *W* and earth connection *E* to the apparatus was as follows :—

(I take this data from a copy of a letter I wrote to Mr. Preece in November, 1896.)

When carrying out some experiments in Italy in 1895, I was using an oscillator having one pole earthed and the other connected to an insulated capacity, the receiver also earthed and connected to a similar capacity. The capacities were in this case cubes of tinned iron of 30 centimetres side, and I found that when these were placed on the top of a pole 2 metres high, signals could be obtained at 30 metres from the transmitter. With the same cubes on poles 4 metres high, signals were obtained at 100 metres, and with the same cubes at a height of 8 metres, other conditions being equal, Morse signals were easily obtained at 400 metres. With larger cubes of 100 centimetres side, fixed at a height of 8 metres, reliable signals could be obtained at 2,400 metres all round, equal to about one mile and a half.

These results seemed to point out that a system of transmitter and receiver designed according to the lines on Fig. 1, *i.e.*, a radiator of the Hertzian type having one pole earthed and the other connected to a vertical, or almost vertical, conductor, or to a lofty capacity area, and a resonator consisting of a suitable receiver having similarly one terminal connected to earth and the other to an insulated vertical conductor, constitute a system of transmitter and receiver capable of giving effects at far greater distances than the ordinary systems of Hertzian radiators and resonators.

The results I have referred to also show that the distance at which signals could be obtained varied approximately with the square of the distance of the capacities from earth, or perhaps with the square of the length of the vertical conductors. This law has since been verified by a careful series of experiments and found correct, and has furnished us with a sure and safe means of calculating what length the vertical wire should have in order to obtain results at a given distance. It is well to know that the said law has never failed to give the expected results across clear space in any installation or experiment I have carried out, although it usually seems that the distance obtained is slightly in excess of what one might expect. I find that with parity of other conditions a vertical wire 20 feet long at the transmitter and receiver is sufficient for communicating one mile, 40 feet at each end for 4 miles, and 80 feet for 16 miles and so on. An installation is now working over a distance of 18 miles with a vertical wire 80 feet high at each installation station.

Professor Ascoli[1] has confirmed this law and demonstrated mathematically, using Neumann's formula, that the inductive action is proportional to the square of the length of one of the two conductors if the two are vertical and of equal length, and in simple inverse proportion of the distance between them. Therefore, the intensity of the induced oscillation does not diminish with the increase of distance if the length of the vertical conductors is increased in proportion with the square root of the distance. That is, if the height of the wire is double, the possible distance becomes quadrupled.

Should it be necessary to rig up an installation at a distance of say 32 miles, such as is about the distance

[1] See *Elettricista*, August number, 1897. (Rowe.)

between Folkestone and Boulogne, it is easy to find that a vertical wire 114 feet long would be quite sufficient for that purpose.

Such laws are applicable only when apparatus properly constructed is employed. With apparatus in which some or several improved details are omitted I find it quite impossible to obtain anything like the results above mentioned. If, say, the impedance coils $k' k'$ are omitted the distance (other conditions being equal) is reduced to almost half its original value.

I must also call your attention to such cases as when obstacles like hills or mountains, or large metallic objects, happen to intervene between the places between which it is desired to establish communication. With all other forms of Hertzian transmitters and receivers with which I have experimented I find it to be quite impossible to obtain any results if a hill, mountain, or large metallic object intervenes in any way between the two stations. I am not aware whether any satisfactory results have been obtained by others where such obstacles have intervened, but when the vertical wire system is employed it becomes easy to telegraph between positions screened from each other by hills or by the curvature of the earth. In such cases it seems to be a marked advantage if the aërial conductor is thick or if a capacity area is placed at the top of it.

I am rather doubtful as to the correct explanation that can be given to this effect. I think there can be very little doubt as to the complete opacity, to electric waves, of a hill three miles thick, or of, say, several miles of sea-water. A solution of this difficulty might be given by attributing the results to the effect of the diffraction of such long waves as those radiated by a conductor 100 ft. long, but in that case it is difficult to explain why other forms of Hertzian transmitters and receivers, also giving long waves, do not act when such obstacles intervene. A way out of the difficulty may be arrived at if we suppose that the electrical oscillations are transmitted to the earth by the earth wire E of the transmitter and travel in all directions along the surface of the earth till they reach the earth wire of the receiving instrument, and by travelling up the said wire to the coherer thus bring about its action. This was the first explanation I came to during my early experiments. I, however, do not wish

to say that I hold entirely to this view at present, although I have not yet found any other perfectly satisfactory explanation of the phenomena.

It is well, also, to note that a horizontal wire, even if supported at a considerable height from earth, seems to be of little or no practical utility in increasing the range of signals. If, say, a vertical wire 30 ft. long is employed at both stations, and to the top of this is added a horizontal length of 300 ft., as shown in Fig. 6, the distance obtained is greater with the vertical wire without the horizontal length than it would be if both were employed. These results show that with this system it is not sufficient to use a horizontal radiating or collecting wire, as such a wire would be of no utility for long-distance signalling.

I believe that the exceedingly marked advance made by

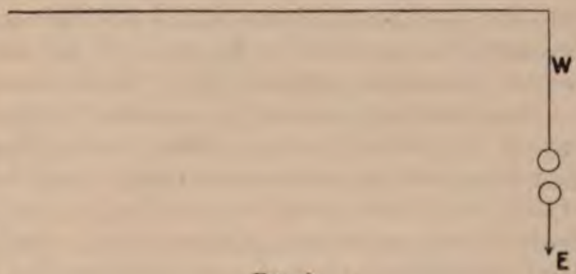


FIG. 6.

the adoption of the vertical conductor is due to the fact that the plane of polarisation of the rays radiated is vertical, and that therefore they are not absorbed by the surface of the earth, which acts as a receiving conductor placed horizontally. As the maximum effect is obtainable when the conductors of the transmitter and receiver are parallel, this makes it necessary to have a vertical conductor connected to one pole of the coherer.

Before proceeding to describe the results obtained under various conditions by means of what we may call the vertical wire system, I think it desirable to bring before you some observations and results I have obtained with a system of Hertzian wave telegraphy, which was the first with which I worked, and in which parabolic reflectors are used to control the propagation and intensify the effects obtained when comparatively short electric waves are

employed for signalling. As in ordinary optics, so also in the optics of electro-magnetic oscillations, it is possible, as has been shown by Hertz, to reflect the waves radiated from the oscillator in one definite direction only. This can be done, as you know, by using convenient reflectors, similar to those used for projectors, but preferably, for economical reasons, made of copper or zinc, instead of silver amalgam or silver. Except when very small radiators of the Righi or Lebedew type are employed, it is desirable to use cylindrical parabolic reflectors, and it is with reflectors such as I here exhibit that the trials to which I am alluding have been carried out. The advantages obtainable by their use are obvious.

In any other system intended for the transmission of telegraphic signals by means of electric waves through space, the waves have been allowed to radiate in all directions, and would affect all suitable receivers within a certain radius, which of course is dependent on the power of the radiator or transmitter and on the sensitiveness of the resonator or receiver. It is, however, possible, by means of syntonising arrangements, to prevent, to a certain extent, messages affecting instruments or receivers for which they are not intended, and therefore to select any receiver by altering the wave length of the transmitter. By means of reflectors it is possible to project the waves in one almost parallel beam which will not affect any receiver placed out of its line of propagation, whether the said receiver is or is not in tune or syntony with the oscillation transmitted. This would enable several forts, or hill-tops, or islands to communicate with each other without any fear of the enemy tapping or interfering with the signals, for if the forts are on small heights the beam of rays would pass above the positions which might be occupied by the enemy. An illustration of the possibility of directing these waves can be shown by the action of the receiver, which in this case rings a bell only when the radiator in the reflector is directed towards it. These results are much more marked in an open space than in a lecture theatre, as the walls, gilt hangings, &c., tend to reflect the rays in all directions and may alter the results.

In experiments carried out over a distance of $1\frac{3}{4}$ miles, I noticed that only a very small movement of the transmitting reflector was sufficient to stop the signals at the

receiving end, which could be only obtained within a latitude of 50 ft. to the right or left of what was believed to be the centre of the beam of reflected radiations.

There exists a most important case to which the reflector system is applicable, namely to enable ships to be warned by lighthouses, light-vessels, or other ships, not only of their proximity to danger, but also of the direction from which the warning comes. If we imagine that *A* is a lighthouse provided with a transmitter of electric waves, constantly giving a series of intermittent impulses or flashes, and *B* a ship provided with a receiving apparatus placed in the focal line of a reflector, it is plain that when the receiver is within range of the oscillator the bell will be rung only when the reflector is directed towards the transmitter, and will not ring when the reflector is not directed towards it. If the reflector is caused to revolve by clockwork or by hand, it will therefore give warning only when occupying a certain sector of the circle in which it revolves. It is therefore easy for a ship in a fog to make out the exact direction of point *A*, whereby, by the conventional number of taps or rings, she will be able to discern either a dangerous point to be avoided or the port or harbour for which she is endeavouring to steer.

I have not up to the present attempted to signal any greater distance than about two miles with reflectors, but I am of opinion that across clear space it will be quite possible to obtain satisfactory results at far greater distances, especially if the reflectors are accurately made any larger than those I have used. By means of the same apparatus exhibited here I have succeeded in signalling over a distance of 2½ miles, without of course the use of any real "base" lines, which were supposed to be essential for any distance greater than a few feet.

It was by means of reflectors I obtained the results over 1¾ miles mentioned by Mr. Preece at the British Association meeting of 1896.

I have, however, dedicated more time to the other system *i.e.*, the vertical wire system.

A station at Alum Bay, Isle of Wight, and another at Bournemouth, the distance between them being 14½ miles, were erected at the beginning of last year in order to test the practicability of the system under all conditions of

weather, and also to afford an opportunity of proving that "Wireless Telegraphy" was not a myth but a working reality. I believe some details of the special conditions of these stations would be of interest. The installation at Alum Bay is in the Needles Hotel, and the Bournemouth station (which has lately been transferred to the Haven Hotel, Poole, thereby increasing the distance to 18 miles), was at Madeira House, South Cliff. At each station a pole 120 feet high was used, which supported the aërial conductor, usually a stranded conductor of 7/20 copper wire insulated with rubber and tape. A 10″ induction coil is used at each station, worked by a battery of 100 Obach cells "M" size, the current taken by the coil being at 14 volts from 6 to 9 amperes. The spark discharge takes place between two small spheres about 1″ in diameter, this form of transmitter having been found more simple and more effective than the Righi oscillator I had previously used. The length of spark is adjusted to about 1 centimetre; this, being a much shorter spark than the coil can give, allows a good margin over for any irregularity that might be caused by the break. No care is ever taken to polish the spheres *d d* at the place where the spark occurs, as the results seem decidedly better with dull spheres than with polished ones.

The first tests were made between the Isle of Wight and a steamer, the height of the mast on the boat being about 60 ft. Readable signals were obtained up to a distance of 18 miles from Alum Bay. During the course of these experiments, I had the pleasure of the company and assistance of Captain Kennedy, R.E., who was good enough to draw a map showing the course of the steamer. It has apparently been thought that weather or varying conditions of atmospheric electricity may interfere with or stop the signals transmitted by this system, but experience of over fourteen months of continual everyday work has brought me to the conclusion that there is no kind of weather which can stop or seriously interfere with the working of such an installation. We have given demonstrations to several eminent scientists, who came down and wanted a show, often when we did not expect them, but on no occasion have they found any difficulty in the work of transmitting and receiving messages between the two stations.

In September of last year, in consequence of the expira-

tion of our lease at Madeira House, Bournemouth, we transferred that station, as I have said, to the Haven Hotel, Poole, thereby increasing the distance to 18 miles. Experiments and tests are carried out daily between the two stations, the improvement in apparatus having allowed us to reduce the height to 80 ft. at each end. An average of fully 1,000 words are daily transmitted through the ether each way.

In the spring of last year Lord Kelvin inspected our station at Alum Bay, and he was kind enough to express himself as highly pleased with what he saw. He sent several telegrams to his friends, including Mr. Preece and Sir George Stokes, and insisted on paying 1s. royalty on each message, wishing in this way to show his appreciation of what was done and to illustrate its fitness at that time for commercial use.

We are now working at experiments directed towards still further reducing the height necessary for a given distance, and also a good deal on syntonic systems.

In May of last year Lloyds desired to have an illustration of the possibility of signalling between Ballycastle and Rathlin Island in the north of Ireland. My assistants, Mr. Kemp and the late Mr. Glanville, installed the instruments at Ballycastle and at Rathlin Island. The distance between the two positions is 7½ miles, of which about 4 are overland and the remainder across the sea, a high cliff also intervening between the two positions. At Ballycastle a pole 70 ft. high was used to support the wire, and at Rathlin a vertical conductor was supported by the lighthouse 80 ft. high. Signalling was found quite possible between the two points, but it was thought desirable to bring the height of the pole at Ballycastle to 100 ft., as the proximity of the lighthouse to the wire at Rathlin seemed to diminish the effectiveness of that station. At Rathlin we found that the lighthouse-keepers were not long in learning how to work the instruments, and after the sad accident which happened to poor Mr. Glanville that installation was worked by them alone, there being no expert on the island at the time.

Following this, in July we were requested by a Dublin paper, the *Daily Express*, to report from the high seas the results and incidents of the Kingstown Regatta. In order to do this we erected a land station, by the kind permission of the

harbour-master at Kingstown, in his grounds, where a pole 110 ft. high was placed. A steamer, the *Flying Huntress*, was chartered to follow the racing yachts, the instruments being placed in the cabin. The height of the vertical wire attainable by the mast was 75 ft. A telephone was fixed from our land station at Kingstown to the *Express* office in Dublin, and as the messages came from the ship they were telephoned to Dublin, and published in succeeding editions of the evening papers.

The relative positions of the various yachts were thus wirelessly signalled while the races were in progress, sometimes over a distance of ten miles, and were published long before the yachts had returned to harbour. During the several days the system was in use between the tug and the land station, over seven hundred messages were sent and received, none requiring to be repeated. On trying longer distances it was found that with a height of 80 ft. on the ship and the same height as already stated on land, it was possible to communicate up to a distance of 25 miles, and it is worthy of note in this case that the curvature of the earth intervened very considerably at such a distance between the two positions. On one occasion, on a regatta day, I had the pleasure of the company of Professor G. F. Fitzgerald, of Trinity College, Dublin, on the ship, who, as would be expected, took a very great interest in the proceedings.

Immediately after finishing at Kingstown I had the honour of being asked to install wireless telegraph communication between the Royal yacht *Osborne* and Osborne House, Isle of Wight, in order that her Majesty might communicate with H.R.H. the Prince of Wales, from Osborne House to the Royal yacht in Cowes Bay, and during the trips His Royal Highness frequently took. The working of this installation was a very pleasant experience for me, and it afforded also an opportunity of more thoroughly studying the effect of intervening hills.

In this installation induction coils capable of giving a 10-inch spark were used at both stations. The height of the pole supporting the vertical conductor was 100 feet at Osborne House.

On the Royal yacht *Osborne* the top of our conductor was suspended to the main mast at a height of 83 ft.

from the deck, the conductor being very near one of the funnels, and in the proximity of a great number of wire stays. The vertical conductor consisted of a 7/20 stranded wire at each station.

The Royal yacht was moored in Cowes Bay at a distance of 1¾ miles from Osborne House, the two positions not being in sight of each other, the hills behind East Cowes intervening. This circumstance would have rendered direct signalling between the two positions impossible by means of any flag, semaphore, or heliograph system. Constant and uninterrupted communication was maintained between the Royal yacht and Osborne House during the sixteen days the system was in use, no hitch whatever occurring.

One hundred and fifty messages were sent, being chiefly private communications between the Queen and the Prince. Many of these messages contained over a hundred and fifty words, and the average speed of transmission was about fifteen words per minute.

By kind permission of the Prince of Wales I will now read to you some of the telegrams which passed between the Royal yacht and Osborne House.

August 4th.

From DR. FRIPP *to* SIR JAMES REID.

H.R.H. the Prince of Wales has passed another excellent night, and is in very good spirits and health. The knee is most satisfactory.

August 5th.

From DR. FRIPP *to* SIR JAMES REID.

H.R.H. the Prince of Wales has passed another excellent night, and the knee is in good condition.

The following telegram was sent during a cruise, and while the Royal yacht was under way, as you will see from the context.

August 10th.

From H.R.H. THE PRINCE OF WALES *to* DUKE OF CONNAUGHT.

Will be very pleased to see you on board any time this afternoon when the *Osborne* returns.

This telegram was sent when the yacht was off Bembridge, at a distance of about seven or eight miles from Osborne.

On August 12th the *Osborne* steamed to the Needles, and communication was kept up with Osborne House until off

Newton Bay, a distance of seven miles, the two positions being completely screened from each other (even to the tops of the masts) by the hills lying between. At the same position we found it quite possible to speak with our station at Alum Bay, although Headon Hill, Golden Hill, and over five miles of land lay directly between. The positions were eight and a half miles apart. Headon Hill was 45 ft. higher than the top of our conductor at Alum Bay station, and 314 ft. higher than the vertical wire on the *Osborne*.

The yacht on the same trip proceeded till about three miles past the Needles, communication having been maintained during the whole trip. Another day, when I did not happen to be on board, the yacht went on a cruise round Bembridge and Sandown, communication being maintained with Osborne House, although more than eight miles of land lay between the two positions. The Prince of Wales and other members of the Royal Family, especially the Duke of York, made much use of the system, and expressed themselves as highly satisfied with its practicability.

I consider these results rather interesting, as doubts have been expressed by some as to whether it would be possible by this system to telegraph over long stretches of land.

Results across hills were also obtained near Spezia by officers of the Italian Navy, using my system.

In December of last year my Company thought it desirable to demonstrate that the system was quite practical and available for enabling telegraphic communication to be established and maintained between lightships and the shore. This, as you are probably aware, is a matter of much importance, as all other systems tried so far have failed, and the cables by which some three or four ships are sometimes connected are exceedingly expensive, and require special moorings and fittings, which are troublesome to maintain and liable to break in storms.

The officials of Trinity House offered us the opportunity of demonstrating to them the utility of the system between the South Foreland Lighthouse, and one of the following light-vessels, viz., the *Gull*, the *South Goodwin*, and the *East Goodwin*. We naturally chose the one furthest away—the *East Goodwin*—which is just 12 miles from the South Foreland Lighthouse.

The apparatus was taken on board in an open boat, and

rigged up in one afternoon. The installation started working from the very first without the slightest difficulty. The system has continued to work admirably through all the storms, which during this year have been remarkable for their continuance and severity.

On one occasion during a big gale in January, a very heavy sea struck the ship, carrying part of her bulwarks away. The report of this mishap was promptly telegraphed to the Superintendent of Trinity House, with all details of the damage sustained.

The height of the wire on board the ship is 80 ft., the mast being for 60 ft. of its length of iron, and the remainder of wood. The aërial wire is let down among a great number of metal stays and chains, which do not appear to have any detrimental effect on the strength of the signals. The instruments are placed in the aft-cabin, and the aërial wire comes through the framework of a skylight, from which it is insulated by means of a rubber pipe. As usual, a 10-inch coil is used, worked by a battery of dry cells, the current taken being about 6 to 8 amperes at 14 volts.

Various members of the crew learned in two days how to send and receive, and in fact how to run the station, and owing to the assistant on board not being as good a sailor as the instruments have proved to be, nearly all the messages during very bad weather are sent and received by these men, who, previous to our visit to the ship, had probably scarcely heard of wireless telegraphy, and were certainly unacquainted with even the rudiments of electricity. It is remarkable that wireless telegraphy, which had been considered by some as rather uncertain, or that might work one day and not the next, has proved in this case to be more reliable, even under such unfavourable conditions, than the ordinary land wires, very many of which were broken down in the storms of last month.

The instruments at the South Foreland Lighthouse are similar to those used on the ship, but as we contemplate making some long distance tests from the South Foreland to the coast of France, the height of the pole is much greater than would be necessary for the lightship installation.

We found that 80 ft. of height is quite sufficient for speaking to the ship, but I am of opinion that the height available on the ship and on shore would be ample even if

the distance to which messages had to be sent were more than double what it is at present.

Service messages are constantly passing between the ship and the lighthouse, and the officials of Trinity House have been good enough to give expression of their entire satisfaction with the result of this installation. The men on board send numerous messages almost daily on their own private affairs; and this naturally tends to make their isolated life less irksome.

My Company has been anxious for some time to establish wireless communication between England and France across the Channel, in order that our French neighbours might also have an opportunity of testing for themselves the practicability of the system, but the promised official consent of the French Government has only been received this evening. Otherwise this communication would have been established long ago. The positions for the stations chosen were situated at Folkestone and Boulogne, the distance between them being 32 miles. I prefer these positions to Calais and Dover, as the latter are only separated by a distance of about 20 miles, which is only slightly more than we are doing every day at Poole and Alum Bay, and as we find that distance so easy we would naturally prefer further tests to be made at much greater distances.

We did ask for permission to erect a station at Cherbourg, the corresponding station to be at the Isle of Wight, but the French authorities stated that they would prefer us to have our station in that country in some other position on the north coast.

My system has been in use in the Italian Navy for more than a year, but I am not at liberty to give many details of what is done there. Various installations have been erected and are working along the coast, two of these being at Spezia.

Distances of 19 miles have been bridged over in communicating with war vessels, although 10 miles have been found quite sufficient for the ordinary fleet requirements.

Other installations are now contemplated in this country for commercial and military purposes, and I am confident that in a few months many more wireless telegraph stations will be established both here and abroad.

Supplementary Note, added March 30th, 1899.

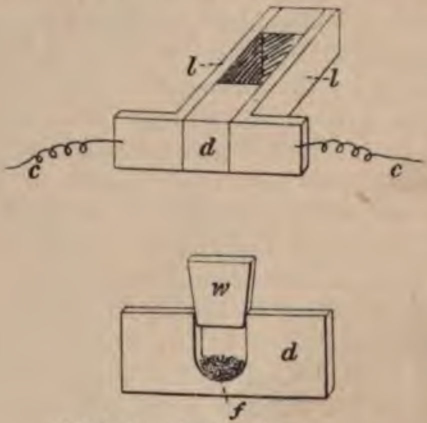
As the installation in the neighbourhood of Boulogne has been started since I read my paper, if I may I would like to add that France and England were successfully connected on Monday, the 27th of March. The station on the English side is situated at the South Foreland Lighthouse, near Dover, and that on the French side at the Châlet L'Artois, Wimereux, near Boulogne. The instruments were sent over from London the previous Monday in charge of two assistants, a house having been taken to serve as a station. A suitable pole was then erected, and at 5 o'clock on the 27th, within a week from the time the instruments left London, perfect telegraphic communication was established between the two points. The first messages passed in the presence of the Committee appointed by the French Government, viz., Colonel Comte du Pontavice de Heussey, Captain Ferrié, and M. Voisenet. The first message was sent from France to England, and the reply was promptly returned by my assistant-in-charge at the South Foreland Lighthouse. There has not been the slightest hitch in the communication since, and it will no doubt be interesting to know that yesterday, the 29th, operations were conducted by two French officers, Captain Ferrié, of the French Engineers, sending from the English side to M. Voisenet, French Telegraph Engineer, on the French side. These gentlemen kept up a telegraphic correspondence for several hours, and they and numerous others have expressed themselves as highly satisfied with the successful start and working of the installation.

The CHAIRMAN: I have the pleasure to announce that, as a great number of people have been turned away from the door this evening owing to there being no room for them, Mr. Marconi has very kindly consented to give his paper again at a date which we hope to be able to fix very soon. The Chairman.

Dr. J. A. FLEMING: I am sure, sir, we shall all desire to present to Mr. Marconi our hearty congratulations on the magnificent success he has obtained in carrying out his most interesting experiments. Dr. Fleming.

He is in such complete possession of the field that there is very little any of us can add in the discussion of his paper except by way of confirmation or questions to elicit more information. There are one or two points, however, on which I should like to make a remark or two. First, as to the transmitter. I believe in his earlier experiments Mr. Marconi made use of a Righi transmitter in which the central spark

Dr. Fleming. balls were immersed in oil, but I think he has since abandoned the use of oil. In many experiments tried last summer with various transmitters, I found that the use of oil did not result in much advantage. In the spark transmitter the creation of the wave depends essentially upon being able to produce a great difference of potential between two balls or rods, and then this difference of potential is made suddenly to disappear by the breaking down of the insulation of the dielectric. Although oil, by its dielectric strength, may enable us to create a greater difference of potential between the balls before the spark passes than would be the case with air, yet after a few sparks have passed the oil becomes full of particles of carbon resulting from the decomposition of the oil, and it seems to cease to give way electrically with the necessary suddenness. Hence what we gain in one way we lose in another. Practically, therefore, the use of oil between the spark balls does not



l l. L-shaped metal pieces.
d. Distance piece of celluloid.
w. Wedge, closing cavity.
f. Metallic filings.
c c. Connecting wires.

FIG. A.

seem to result in any increased wave energy for a given spark gap and spark coil. In the next place as regards the coherer. I think Mr. Marconi still finds it advantageous to employ a glass tube which is exhausted of its air as the container. For purely experimental purposes I have found it convenient to construct the coherer so that one can easily change the metallic filings in nature or quantity. With that aim it is constructed as follows :—

Two pieces of silver or platinoid are bent into the shape of the letter L and placed back to back (see Fig. A). Between them is laid a distance piece of celluloid or ivory cut out as shown in the diagram. The whole is bound together with silk thread, and a small wedge put into the top as a lid. It will be seen that the result is to construct a small closed box with metallic sides. In this box are placed the metallic filings. I *generally* use nickel with 5 per cent of finely powdered uranium. The

good performance o this coherer depends very much upon the exact quantity and degree of fineness of the filings. This has to be discovered by experiment. I can confirm Mr. Marconi's statement that when once well made the coherer is by no means the uncertain instrument it has been often stated to be. Dr. Fleming.

Passing on next to the question of distance transmission, there is, I think, no doubt that Mr. Marconi's splendid results have been due to his invention of the long vertical wire or rod as an adjunct. I can also confirm what he says about the use of a vertical as against a horizontal wire.

In the course of many experiments I made last year on a small scale, I had abundant opportunity to notice the far greater distances to which we can signal by the use of a vertical wire than by an equal length of receiving wire used horizontally.

I believe also that an important element in his success is due to the earth connection. In his system the spark balls and the coherer are inserted between the earth and the long vertical receiving and transmitting wires.

If the earth acts as a perfect conductor the system may then perhaps be regarded as one conductor in which electrical oscillations are set up. There is, however, much yet to be learnt as to the true function of the earth in these experiments. Mr. Marconi's researches open up a wide vista for future investigations and interesting matter for discussion at present; but as so many others will no doubt desire to speak on the paper, I will not detain you any longer, but conclude by expressing once more our thorough appreciation of all that Mr. Marconi has been able to achieve.

Dr. ERSKINE MURRAY: Four months ago, when I joined the Wireless Telegraph Company as Mr. Marconi's assistant, I was more ignorant, probably, than a good many of those present at the meeting to-night of the subject of Wireless Telegraphy in its practical application. Of course, every one interested in this subject could not but follow the experiments from the outsider's point of view, as they were reported in the newspapers and the technical press—though unfortunately a number of the journals were, at that time, rather hostile to Mr. Marconi, so that the reports were perhaps not quite as full as they might have been. On my first going to Bournemouth and seeing the action of the telegraph, I was much surprised to find how very ordinary it was in character. It just worked like any other telegraph—not quite as fast as you hear the instruments going in the General Post Office, but working at a very rational speed and with very considerable certainty. Dr. Murray.

The law governing the relation of the height of the vertical wires and the distance between them is a most interesting point. A somewhat simple illustration may be taken from the ordinary electrical machines. In the frictional and influence-electric machines, the electricity is collected by means of a number of fine points. The "tension," as defined by Clerk Maxwell, is very much greater on the point than it is elsewhere on the more rounded part of the conductor, and therefore the force from the parts near the points (not only from the actual point itself) is greater than it would be nearer to the body of

Dr. Murray. the conductor. Now, in this case the earth forms one large conductor, and the wires standing up are the points. When such a wire is charged the surface density increases with the elevation, probably very nearly in proportion to the distance from the earth. The tension depends on the square of the surface density, and therefore the action at the top will be more or less proportional to the square of the height. This is not perhaps perfectly accurate, but it will serve to indicate what, I think, may be one cause of the action in this case.

Mr. Evershed. Mr. EVERSHED: I have done some work in connection with wireless telegraphy of the electro-magnetic kind, but have never even made a single experiment on Hertzian telegraphy, and it follows therefore that I feel unable to discuss Mr. Marconi's paper. Ever since I heard of Mr. Marconi's work I have been hoping that we should have the advantage of hearing it described by himself in this room, and that hope, I feel sure, was shared by every member of this Institution. During the time he has been carrying out his work, various questions have occurred to me concerning such points as the power used, the law of the variation of distance from which signals can be sent, and the connection between this distance and the length of the vertical wire, but, in his paper, he seems to have anticipated every one of my questions.

I notice that the law connecting the length of vertical wire and the distance to which he can signal is an exceedingly simple one, and I should like to draw attention to the analogy there is between that law and one which I explained in connection with my own system of wireless telegraphy a few weeks ago. I showed that the distance to which signals could be sent by means of electro-magnetic induction depended, among other things, on the product of the areas of two circuits. Now, during that same discussion, Dr. Fleming referred to the analogous case of the vertical or horizontal wire used in Hertzian telegraphy, and suggested that the true function of that wire was to integrate the electric force over a considerable space, and therefore get sufficient energy to work the coherer. That explanation seems exceedingly simple, and I think we might adopt it until some one proposes a better. Now it follows from what Dr. Fleming said during that discussion, that Mr. Marconi's law would almost necessarily be true, because the effect either of the transmitting or the receiving vertical wires should be proportional to its length. Consequently, if the length of the two wires be doubled the effect will be quadrupled, a law which Mr. Marconi appears to find very accurately fulfilled in his experiments, or at least sufficiently accurately to enable him to design new telegraphs for greater distances.

It is well known that I have been working on the subject of telegraphy to lightships for a good many years, and I should like to say, in conclusion, that one proposal that I made for signalling to lightships was submitted to the Royal Commission, which inquired into the subject. It was tried and was a total failure—and I want to be the first to congratulate Mr. Marconi very heartily on having succeeded.

Mr. Sullivan. Mr. H. W. SULLIVAN: We have been told that great improvement is effected in signalling by Mr. Marconi's system, by having an earth

at both ends—that is to say, both on the transmitter and on the receiver. I think it is very important to know if that earth should be a perfect one, more especially in view of the difficulties, which Mr. Granville so well described lately before the Institution, as being met with at the Fastnet Rock, in establishing and maintaining an earth—difficulties which will be found in all similar cases of weather-beaten and exposed igneous rocks. Hearing Mr. Granville, one could not but admire the persistent ingenuity which has been applied to overcome that difficulty. The Fastnet is a very difficult case, not only because the rock itself is non-conducting, but because the wire itself is frequently thrown up by the surf. Mr. Sullivan.

If, as seems to be the case, Mr. Marconi has successfully solved the problem of communicating with lightships and lighthouses, he has conferred a great and lasting benefit upon the world.

Mr. W. P. GRANVILLE: Whilst congratulating Mr. Marconi upon the energy and skill with which he has conducted his most interesting experiments, I should like to ask one or two questions. First, with regard to the vertical wire at Alum Bay, the base of the standard on which the wire is erected is on a hill perhaps 100 or 200 feet in height. It is the same at Bournemouth. I should therefore like to know whether the height of the vertical wire above the sea line makes any material difference. Mr. Granville.

The other question relates to the neighbourhood of conducting masses at the transmitting and also at the receiving end. For instance, in the case in which I have been more particularly interested at the Fastnet lighthouse, the whole mass of the tower, as I pointed out a little while ago, is of thick iron plate; and the whole of the buildings on the rock are of iron. In a case of that kind, where one cannot get more than ten or twenty feet away from the mass of conducting metal, I should like to ask if Mr. Marconi would find any great difficulty with his apparatus?

Captain W. P. BRETT, R.E.: It is somewhat difficult to discuss the paper, which is mainly a record of facts, which are, however, very interesting from the point of view that success has been obtained over distances unattained as yet by any other system of wireless telegraphy. Capt. Brett.

With regard to the question of the earth connection, I do not want to anticipate Mr. Marconi's reply, but I happened to be present about a month since at some experiments which he was conducting between the South Foreland and the East Goodwin Lightship. I was at that time most interested in the question as to the extent to which the earth connection affected the receiving of signals, and at my request Mr. Marconi, while signals were being received, suddenly disconnected the earth connection of the receiver. The result was that the signals came just as before. It would therefore appear that, at any rate where there is an open space, the earth connection of the receiver is not a necessity for the working of the apparatus, at least over twelve miles. I am quite prepared to accept that the earth connection in conjunction with a vertical line is a necessity at the *transmitter*, simply because it confers, or, in conjunction with the vertical wire, assists in conferring an oscillating character on the discharge. It does not appear as if

Capt. Brett. good or bad earth comes into the question of receiving at all. No earth at all is necessary so far as the receiving portion of the apparatus is concerned. I should like to ask Mr. Marconi whether he has had an opportunity at Poole of repeating the experiment of disconnecting the earth when messages are being received over the greater distance between that station and Alum Bay. That would probably throw a great deal of light on whether the earth conduction, or waves skimming, as it were, over the surface of the earth, play any part in the transmission, or whether it is due to pure ether waves generated from the vertical wire. I hope Mr. Marconi has followed out that experiment, because I think the result at South Foreland came rather as a surprise to himself.

The Chairman. The CHAIRMAN: Trinity House is represented here this evening. May I ask if any of the Brethren of Trinity House or any representative of Trinity House will be prepared to speak ?

Capt. Vyvyan. Captain VYVYAN (Deputy Master of Trinity House): I am totally unprepared, sir, to enter into any discussion on this subject, of which, an hour ago, I was comparatively ignorant. I have learned a good deal on the subject since, and I congratulate Signor Marconi most heartily upon the success he has obtained and the abnormal gathering and reception he has had here to-night. I should like to confirm everything he has said about the success that has been obtained with regard to the lightships. For over two months this experiment has been going on, and I can confirm every word that he has said with regard to its success. Had there been any hitch at all I should have heard of it. I congratulate Mr. Marconi very heartily.

Capt. Kennedy. Captain J. M. KENNEDY, R.E.: When Mr. Marconi first came to England, through the kindness of Mr. Preece and Mr. Gavey, under whom I was then serving, I was able to attend and take part in the experiments at Weston, Salisbury Plain, and, more recently, those in the Isle of Wight. In a paper which I read just a year ago at the United Service Institution, I dealt chiefly with the military application of this system of wireless telegraphy. As regards communication between fixed stations, such as forts and permanent camps, we may say from what we have seen to-night that the question is solved, but an army in the field—in the Soudan or in Afghanistan, for instance—cannot be expected to carry about with it 100 or 150 feet poles. Some other way must be found for raising the vertical wire. I therefore tried several experiments with kites and balloons, the latter when there was no wind and the former when there was, and experienced no difficulty with them. The balloons were only 4 feet in diameter; they were no trouble to carry about, and could be expanded from a small cylinder. One advantage of these balloons and kites is that they obviate all the difficulties of insulation. No wire stays or boiler plates exist to cause annoyance, and there is no limit to the length of wire that can be used, for it is as easy to put up 1,000 feet of wire as it is 10 feet; other things being equal, this means practically unlimited latitude as regards range. In some experiments I made at Bath, I had two miles of wire up on one of Captain Baden Powell's kites. It is needless to say that the enormous impedance in that long wire was too

great for use as a resonator, a much shorter conductor answered our purpose better ; but we had very strong, and very painful, atmospheric effects—in the shape of 6-inch sparks. Capt. Kennedy.

I think I am right in saying, and I am proud to be able to say it, that about a year ago I was, with the exception of Captain Brett and of Captain Jackson, of the Royal Navy, the only true believer in Mr. Marconi and his system. Consequently it is very easy for me now to join with the multitude in sounding his praise.

The CHAIRMAN : I think that we must now adjourn this discussion. Speaking for myself, as one who has believed in this system of communication with lighthouses and ships ever since he heard of the Hertzian experiments, I should like to ask Mr. Marconi if he could not settle for us experimentally before the date of the adjourned discussion, whether it is really necessary to have the earth connection, because this assumed necessity for an earth connection is one of the most difficult things for me to understand. The Chairman.

I have to announce that the scrutineers report the following candidates to have been duly elected :—

Member:
(Father) E. Lafont, S.J., C.I.E.

Associate Member:
Hugh McCormick.

Foreign Member:
Thomas Norberg-Schulz.

Associates:

George Davis.	Archibald John Howard.
Ernest Holmes Llewelyn Dickson.	Edmund Howl.
	Alexander Lindsay.

Students:

Basil George Burgess.	Thomas Cowell Nesbitt.

The Three Hundredth and Twenty-Ninth Ordinary General Meeting was held at the Institution of Civil Engineers, Great George Street, Westminster, on Thursday evening, March 9th, 1899, Professor S. P. THOMPSON, D.Sc., F.R.S., Vice-President, in the chair.

The minutes of the Ordinary General Meeting held on March 2nd were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council :—

From the class of Associates to that of Members—

F. B. Nicholson.

From the class of Associates to that of Associate Members—

S. E. Andrew.	A. E. du Pasquier.
P. M. Benest.	J. W. Polley.
I. Bulfin.	G. Porter.
A. T. Cummins.	W. H. Russell.
W. Hawkings.	F. Suter.
O. M. C. Heyl.	H. K. Tavaria.
H. Nash.	W. R. C. Wakley.
J. A. Newton.	T. C. Wolley Dod.

Messrs. B. G. Stewart and J. Toulmin were appointed scrutineers of the ballot for new members.

The CHAIRMAN : The Council to-night decided to have read at this meeting the Report of a Committee which it appointed on a previous occasion to inquire into certain aspects of the Telephone question. The report, and an appended rider, both of which were adopted by the Council, are as follows :—

"That, in the opinion of this Committee, it is undesirable from the point of view of the progress of electrical engineering, for the purely local Telephone Industry throughout the Country to be held as a monopoly ; or that legislation on the future of Telephony in these

kingdoms should discourage the undertaking of exchange systems within telephone areas by independent enterprise.

"The committee is of the opinion that the Council should strongly urge on the Government that such legislation may have the further effect of removing the existing disabilities to the carrying out of the recommendation."

I may perhaps add that before adopting the recommendations of this committee, the Council had already so far made up its mind on this question as to cause information to be conveyed to Her Majesty's Government that in the event of Her Majesty's Government desiring to communicate with the Institution, the Council of the Institution would be prepared to express, on behalf of the Institution, the views which they might hold as to the effect of possible pending legislation upon the electrical engineering industry in this country.

There is one other announcement before we commence the business of the evening. In consequence of the very large number of gentlemen who were unable to obtain even admission to this building on the occasion of the reading of Mr. Marconi's paper a week ago, it was decided and announced at the close of the meeting that Mr. Marconi had kindly consented to read his paper and give his demonstrations a second time. During the week that has elapsed arrangements have been made, and notices have been sent to the members of the Institution, informing them that the second reading of Mr. Marconi's paper and demonstration would be held in the Examination Hall on the Thames Embankment; and it was also announced that applications must be made to the Secretary for tickets for admission on that occasion, both for members and for friends. As a matter of fact, already, so many applications have been received that it is quite evident the hall would not be large enough, and consequently the Council has to-night made a change in the arrangements, which I take the earliest opportunity of announcing, and that is, that the meeting will be held, not in the Examination Hall, but in the Lower Exeter Hall at the same time as previously announced. Further, I may add that it is of little use for any member to go to that Lower Exeter Hall unless he is provided with one of the special tickets. The announcement that went out said that, if there was room, people might come with the ordinary ticket of admission, but it is

evident that only those who are fortunate holders of special tickets for the occasion will be able to get in.

I will now ask Mr. Gavey to re-open the discussion on Mr. Marconi's paper.

Mr. Gavey. Mr. J. GAVEY: I should like first of all to congratulate Mr. Marconi on the advance which his paper shows has been made in the perfecting of his system during the last fifteen months. I was happy to be associated with him in his early experiments, and I took share in those that were carried out across the Bristol Channel. Those experiments were of course, to some extent, crude, in that the apparatus was, to a great extent, untried, and a little uncertain in its action. We did succeed in obtaining thoroughly good signals from one side of the Channel to the other, and we learned enough then to render it desirable to proceed with fresh experiments. It was arranged by the Post Office that these fresh experiments should be made, the necessary apparatus was immediately put in hand, and the further trials were carried out. The object in view in these experiments was to ascertain the law connecting the height of the conductors with the distance over which transmission was possible, and generally to test the system as a practical system. We obtained the permission of the War Office to provide a transmitting station at one of their forts, Fort Burgoyne, near Dover, and we provided a number of receiving stations at different distances apart.

The result of these experiments, I am glad to say, led us to certain conclusions, most of which have, since then, been verified by Mr. Marconi and many of which have been set forth in his paper. Thus, we proved the law of the square of the height of the wires, within certain limits, and we went a little further. We found that with single elevated conductors syntony did not come into effect at all; so that not only did the distance over which signals could be transmitted increase as the square of the height, but it increased as the product of two vertical conductors of irregular heights when not widely disproportional. Thus, for example, assuming that it were possible to transmit signals over a certain distance with two conductors 50 feet high, the square of which number would be 2,500; under favourable conditions signals could be transmitted over the same distance with two conductors, one of which was, say, 70 feet, and the other 36 feet in height, these two numbers giving the same product as the square of 50. We found, however, that there were more marked effects of syntony if a capacity were placed at the top of the vertical conductor; and further, that the use of a capacity of that sort enabled us to transmit over a certain distance with a less height of conductor.

We, however, came across a curious phenomenon, namely, that a greater height of vertical conductor was required to transmit signals over the land than over the sea. Thus, taking the experiments in the Bristol Channel as a guide, and also subsequent experiments made by Signor Marconi at Spezzia, we calculated that it required a vertical height of 35 feet to transmit signals over a distance of a mile of water;

Mr. Gavey.

but, to our surprise, we found that we could only transmit signals over the same distance on land with a vertical conductor 50 feet in height. We naturally made numerous experiments to ascertain the cause of this apparently abnormal result, and ultimately we asked Signor Marconi to come down with some of his own most recently devised apparatus, and we found that our previous observations were confirmed. Subsequently I observed that Professor Slaby, of Charlottenburg, an eminent scientist who had been sent over by the German Emperor to watch the experiments in the Bristol Channel, obtained the same results in his experiments conducted in Germany. He attributed the difference to the particles of dust floating in the atmosphere; but, with due deference to so high an authority, I am afraid I cannot quite agree with him, because in the case of the experiments at Dover there would scarcely be such a wide difference in the atmosphere over the Downs immediately adjoining the Straits and over the Straits themselves. I venture to suggest that it is rather due to reflection from the surface of the water. The water over such distances may be assumed to be almost an even plane, a reflecting mirror, and there is no doubt that as these experiments over water are generally conducted from considerable heights from the high land overlooking the sea, many of the waves which would ordinarily travel right away are reflected back from the water and impinge on the receiving vertical wire; whilst on land many of these waves would be absorbed, or possibly reflected, into space through the undulations of the surface.

I referred just now to the vertical height of 35 feet, which was necessary to speak over a mile in the early experiments. Signor Marconi has now reduced this to 20 feet, a very marked improvement. But in connection with that I venture to ask whether this height of 20 feet is a practical working height with a fair margin—what might be termed a good factor of safety—which will admit of working under all conditions, or whether this is the shortest height which could be obtained when skilled experts were at the transmitting and the receiving instruments?

We discovered one other interesting matter in connection with these experiments, for when we found the discrepancy that existed between the results obtained over the water and those over the land, we investigated the causes that might have led to the difference, and, naturally enough, immediately suspected the earth, for, as every one knows, on the chalky Downs in the neighbourhood of Dover it is very difficult to get thoroughly good earth at a moderate depth. We therefore disconnected the earth entirely from the coherer, and substituted an ordinary coil of wire, one end of which was insulated. In fact, we substituted a capacity for an earth, and found the signals were equally good in the two cases. We then gradually reduced this capacity from a length of a few hundred yards of wire to a simple strip of brass about 2 feet long and an inch wide; and we found that with that very small capacity we could still receive just as well as we did when the coherer was connected to earth. We naturally tried the same effect on the transmitting end, but here we found we could not get good signals without an earth connection. Probably if we had used a capacity of sufficient size to dispose of the large amount of energy that was being sent into the

Mr. Gavey. vertical conductor, we could have done without an earth at either end. But I think that goes some way towards answering the question put by one of the previous speakers.

We also discovered that the oil transmitter in the Righi form was quite superfluous when connected with a vertical conductor; a simple air gap in the coil was all that was necessary.

One of the speakers asked what amount of energy was taken up in producing these results. Well, it is a question rather of frequency than of energy, and this was shown by one experiment. We had a very large coil, capable of giving a 20-inch spark, but as a general rule we adjusted it so that with 5 amperes of current it would give a rapid vibration and a 6-inch spark. Now, as a matter of fact, when we increased the current to 10 amperes to get the 20-inch spark we had naturally to screw up the contact-breaker very tightly, and that reduced the periodicity, so that we got a worse result with 10 amperes and a slow vibration than with 5 amperes and a rapid vibration.

We also tried large copper reflectors, of the type exhibited by Signor Marconi; and at first I hoped that we were going to obtain very promising results, for at a distance of half a mile, with the transmitter in the focus of one of the reflectors, we could pick up the signals at a distance of half a mile in the open air without the use of a reflector at the far end. In fact, walking about within the range of the focussed rays we could pick up those signals everywhere. But when we extended the experiments further we got very disappointing results; in fact, so disappointing that the experiments were, after a time, abandoned. It appeared that even slight undulations which did not come in the direct line of the two reflectors were quite sufficient to prevent satisfactory transmission of signals.[1] I gather from Signor Marconi's paper that he has obtained satisfactory results over a distance of two miles. I hope that the reason he has not extended them beyond that distance is want of time to carry out experiments; because I must confess that it appears to me that signalling by means of reflectors will be far more satisfactory than signalling from vertical conductors. If you signal from vertical conductors and have a large number of stations within a limited range, they are practically in permanent contact; in other words, whatever you send on one conductor is heard on all the others, and it is only possible to transmit one message at one time. The use of reflectors would of course remedy this evil, and would be a great advantage in the practical application of the system.

Of course in dealing with a paper like that of Signor Marconi's it is always difficult to draw the line between popular description and technical results; but probably in his reply Signor Marconi would like to glance briefly at one or two practical points, which would no doubt be of great interest to the Institution. First of all there is the question

[1] *Note, added March 25th.*—It should be added that a Righi Transmitter was used in the focus of a parabolic reflector, and that due regard was paid to the relation between the wave length and the distance of the reflecting surface. The use of reflectors with long vertical wires was, of course, never considered as practical for the reasons given by a subsequent speaker.

of screening the receiving apparatus from the effect of the violent surgings in the transmitting apparatus—a question of very great interest. The difficulty was solved by Captain Jackson, of Her Majesty's ship *Defiance*, in one way, and I think Signor Marconi has solved it in another way. But if he sees his way to give some brief explanation in his reply, I think it will be of very great interest. There are one or two other points, too, which will no doubt strike him; such, for instance, as the protection of an installation from the effects of lightning. Of course the vertical conductor used for this signalling is practically a lightning conductor; and to introduce a lightning conductor of this sort into the centre of a building or of a lightship without adequate protection would be a very undesirable thing. I believe he has provided protection against transient currents that might interfere with the signalling, by means of a coil of high resistance, of considerable self-induction, but that would not serve against lightning effects. No doubt other practical points he has come across in the course of his investigations will strike him as being of interest, and I have no doubt he will be pleased to give some further information upon those and other subjects. Mr. Gavey.

Mr. M. O'GORMAN: I wish to suggest a theory of coherer action at long distances with extended vertical wires and to ask a question, the answer to which will either condemn or admit my explanation. Mr. O'Gorman.

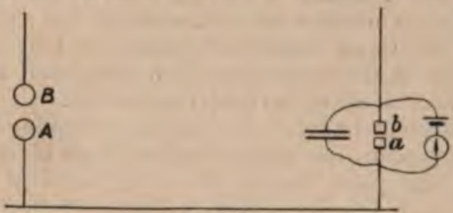


FIG. A.

On sparking, there is an oscillating difference of potential between the lower knob, A (which is earthed), and the upper, B, which is connected to a capacity, viz.: the vertical wire which is erected away from the earth's inductive influence along its length.

The result is a surging of the potential of the surface of the earth or sea, A a (see Fig. A).

This surging is entirely superficial, owing to the high frequency of the disruptive spark. It is more superficial, and extends to a greater distance over the sea than over dry land because the sea is a better conductor. It is capable of being detected in all directions round the source of disturbance (up to a radius of 18 miles and more) by interposing between the earth's surface and a conductor at some height from its surface the only instrument we have, capable of detecting such practically instantaneous reversals—the coherer.

To test the coherer under small changes of potential, I applied to its terminals two wires connected to a capacity (1/3 mfd.) previously charged by applying from 10 to 2 volts. It always cohered even when the discharge of the condenser (its ballastic throw) was directed to

Mr. O'Gorman. oppose the coherer deflection, thereby reducing for an instant the pressure on the coherer to zero, or nearly so.

I distinguish between Mr. Marconi's two methods of transmitting: the one where a transmitter knob is not earthed and reflectors of true electro-magnetic waves are used; the other where one knob and one coherer terminal are connected together by the earth and in which, although Hertz waves are formed, it will be found that reflectors are of little or no assistance, as the signals are solely due to the surface of the earth fluctuating in potential.

The simple test by which my theory may stand or fall is: "Does a reflector arranged to screen all Hertz waves from the direction of the coherer prevent the apparatus from working, or does it still continue to transmit signals?"

This would at once explain:—

(1) Why a horizontal wire is of little avail.

(2) Why a vertical wire is useful.

(3) How Mr. Marconi signalled "through" a hill 135 feet higher than his transmitter when Hertz waves will not either go through or round such a hill.

(4) Why Mr. Gavey got signals with the lower side of his coherer connected to earth through a small capacity instead of a copper wire.

(5) Why the "proximity of the lighthouse to the vertical wire at Rathlin diminishes the effectiveness of that station."

(6) Why Mr. Gavey found that distance of transmission was a matter of frequency rather than energy and that reflectors were of no use on the long distance tests.

Mr. Carter. Mr. E. TREMLETT CARTER: It would be interesting to know how Mr. O'Gorman explains the action of reflectors on his theory. If the whole action takes place through the earth, how is it that when a reflector is put behind the transmitter an intensified effect is observed?

Mr. O'Gorman. Mr. O'GORMAN: I do not suggest for a moment that the coherer does not work with the reflectors. I only suggest that when telegraphing with these tall wires and earthing one side, the system becomes, as it were, an impossible radiator because it is absolutely out of balance, having a large capacity at one end and a small capacity on the other. I distinguish between two kinds of wireless telegraphy; one in which a difference of potential is developed, and the other in which radiator and receiver are used. Within the latter class is placed the experiment Mr. Marconi showed us, in which the reflector either increased the communication, or when turned away decreased it down to zero.

Mr. Granville. Mr. W. P. GRANVILLE: Since the last meeting one or two practical points have arisen upon which I should like to be enlightened. First with regard to the 100 Obach cells used at Alum Bay. I think I have heard that those 100 cells were not directly connected to the induction coil, but were used to charge an accumulator, and that the induction coil was then worked off the accumulator. I should like to know whether this is correct, because it would show that practically we could not use the system in those places where we were not able to use a dynamo.

With regard to the screening effect. We have read in the press so much that is incompatible, that one is glad to have this opportunity of gaining authentic information. First, we have heard from published accounts that even three men-of-war interposed between the receiving ship and the signalling station did not prevent the receipt of messages. Then again, we hear that the waves are reflected by thin sheets of metal. One would like to know which statement is correct. Mr. Granville.

Again, how is it that when these lectures are given we have not the opportunity of seeing the signals sent over a greater range? It was a little disappointing that the signals were not received in this room from a distant room, or, better still, from another building. Is it that the walls themselves utterly screen the effect, and that the transmitting and receiving apparatus must be, as it were, in free space?

Mr. L. B. MILLER: I have made a number of experiments, but only with horizontal wires, and some of the results I obtained do not agree with those of Mr. Marconi, who uses a vertical wire. For instance, I should like to know whether it is not better, as it appears to me to be, to use several spark gaps in series for the transmitter? When using horizontal wires two gaps are certainly better than one; and I think more gaps would be of still further advantage. Mr. Miller.

As to the coherer, I should like to know which are really the most sensitive metals for contacts. Apart altogether from the difficulty of de-cohering, what are the best and most sensitive metals to use? As to the relay, I have had an opportunity lately of working with an extremely sensitive relay of the D'Arsonval type, made by Mr. Paul, which worked with less than $\frac{1}{100}$th of a milli-ampere. Rather to my surprise, with that relay I did not get any better results than I obtained with one much inferior in sensitiveness. The reason is that the ordinary form of coherer does not break down until a wave of a certain strength reaches it. It seems to me a coherer is wanted for some purposes that will gradually break down, so that if successive weak waves meet it, they will finally cause the relay to act. I should be glad to know if any one has made experiments with carbon coherers, as I have read that they act in that way.

As to the earth connection, a connection for wireless telegraphy is rather a vague expression. The receiving apparatus will act without a wing being actually attached to it. A wing may be held, say, a foot, or even a yard, off. The effect is diminished by the break but does not cease. The mere fact of presenting the metal bar to a coherer will always cause it to act if it be properly adjusted. It therefore seems to me rather difficult to say what is an earth connection.

Mr. CAMPBELL SWINTON: I can answer Mr. Miller's inquiry with regard to carbon coherers. Some time ago, I tried experiments to ascertain whether Röntgen rays affected the resistance of carbon, and found that, apparently, they did so. I discovered, however, afterwards it was not the Röntgen rays at all that were affecting the carbon, but the Hertzian waves produced by the contact breaker of the induction coil. Mr. Swinton.

Mr. A. C. BROWN: I also have tried many experiments with the carbon coherer, and have found that it breaks down gradually. One Mr. Brown.

Mr. Brown. Hertzian wave will slightly reduce its resistance; another will reduce its resistance still more, and so on; until perhaps a dozen or so waves, according to their strength, will have considerably diminished the resistance of the coherer, unless restored by tapping.

Mr. Stevenson. Mr. C. A. STEVENSON [*communicated*]: It is not advantageous to discuss any one system of telegraphy through space without considering other tried systems, as each may have special advantages under special conditions. I propose to deal with each system under the conditions which are likely to occur in immediate practice.

Communication with Lightships.—For lightships distant a few miles from shore perhaps a Hertz wave system may be best, but the height of mast, the uncertainty of action, and the necessity for the skill of an electrician may be against it.

The system of communication by induction from a submarine cable or coil on the sea bottom and a coil or magnet in the ship, is however, more practical for light-vessels at, say, twenty miles from land, such as the Galloper, Kentish Rock, North and West Hinder light-vessels, and many of the American light-vessels. Indeed, at the light-vessels where communication is most wanted, it will be found that the Hertz method is inapplicable and the induction method easy of application.

There are several things, however, which must be attended to to insure success. In the first place it is essential that the cable, if it make a single turn on the sea-bottom round the lightship, should be taken back to shore. If, after making the turn, the copper be bared, and the bit of the cable adjoining the loop be hooked up from the bottom and the copper be spliced on to the sheath, it might be thought possible that action might then take place; but my experience shows that this is not a reliable method, and that there is nothing for it in such a case but to bring the cable practically back to shore, as I have by experiment found on a sheathed cable two miles in length immersed in the sea by measuring the induction from it. Again, the cable should be of the best, with a resistance of 3 ohms per knot, and the coil should be lightly sheathed, although this does not appear to be an all-important matter. Making several turns of the cable round the vessel is very essential. It is evident that the extra resistance put in my making, say, four turns on the bottom is small, but the inductive effect is thereby greatly increased. Putting only one turn on the bottom, and using the copper in the other three turns to reduce the resistance of the cable to and from shore, would give in comparison but a poor effect. It is necessary to explain that, as it has been erroneously stated that number of turns has nothing to do with the action, only weight of copper.

In regard to the supposed great screening effect of salt water, no one has given an experiment to show this supposed effect—the experiment at the Goodwin being practically without induction effect cannot be cited. It is a dangerous thing to let theory precede experiment and to generalise. With proper apparatus there is no such great screening even with armoured cables.

With reference to the system suggested by me used at Sandy Hook Lightship, and designed by Professor Blake, on the grid or conduction system, not the coil system, good results were got, so good that the

Gardinia, steaming about with the proper apparatus on board, could receive the signals. Mechanically it was imperfect, owing to the conductors lying on the area of travel of the ship. It is by no means impossible, however, that a very good system would be got by carrying two cables out from shore and leaving the core bared on either side of the lightship. Experiment has shown me that it is unlikely to be able to work with one wire, leaving the earth or water to complete the circuit. Mr. Stevenson.

Another method I have experimentally tried is to terminate a single cable from the shore in a small coil or electro-magnet below the vessel. If another electro-magnet be now passed through the swivel from the vessel and let down to a point near the bottom, it is evident that thus the movable electro-magnet may be brought close to the magnet on the bottom and nearly all screening effect, such as it is, avoided. There is thus also no possibility of any twisting action on the cable, or deterioration from raising it and letting it again rest on the bottom, taking place, which are actions serious in the present system of continuous connection. A wire rope, however, with wire wound on it to form a long electro-magnet, would form a sensitive and easily managed arrangement. Such a system by submerged magnet as I describe, where the induction action requires to be sent only a few feet, seems peculiarly well suited for communication with light-vessels 20 miles from shore, where a Hertz system would be difficult, if not practically impossible. A current of but a fraction of an ampere in the cable is ample to get communication between one electro-magnet and another over a space of 40 feet (7 fathoms), and as the resistance of a 20-knot cable and its coil may be about 70 ohms, it is evident how simple the apparatus would be. It is quite unnecessary that the cable should be taken back to shore with this system ; the sea or sheath can perform this work. There is no loss in the inductive action from a sheath, as the electro-magnet coil would be unarmoured. The existing cables to lightships at present on the continuous system, such as the Kentish Knock, if of good conductivity, can be used without laying them anew from shore, with merely the addition of two comparatively cheap electro-magnets and their connections. This system of communication by coil induction is certain in its action, and would be reliable at all times. I suggested this to the Postal and other authorities some years ago.

Communication with Lighthouses.—Communication with lighthouses on isolated rocks, such as the Bell Rock, Skerryvore, Dhuheartach, The Eddystone, and Bishop Rock is a difficulty under any system. Continuous connection by cable is hampered by the difficulty of making the connection and maintaining it. The coil or magnet system is hampered by the limited area; the earth-tapping system by the small distance which the sea plates can be separated, and the difficulty of sea connection; and the Lodge-Marconi system by the high mast, the life of the apparatus, and the amount of skill required, not in sending the message but in manipulating the apparatus.

Communication with lighthouses on islands does not offer such serious difficulties. One of these in Scotland, North Unst, is half a mile from land and two and a quarter miles from the shore station, commu-

Mr Stevenson

nication to which there can be no doubt, from the experiments I have made in 1894, can be easily and cheaply dealt with by coils, in a far more satisfactory and certain manner than by the Lodge-Marconi or by earth-tapping systems.

A Hertz system would involve a man situated on Hermaness, as owing to the very high ground coming between the station and the rock it would be inexpedient to transmit the signals to Balta Sound, and *vice versâ*, unless some device could be made to automatically transmit the signals; but such a device to be left in all weathers to itself does not seem very hopeful. The coil system with a complete metallic coil on the rock without sea connections, with a coil erected on the shore on poles, with an ordinary pole-line leading to the shore station, meets the circumstances. Reliability, simplicity, and cheapness would be the merits of such a system. In considering this scheme for North Unst, coils with their axes coincident were considered, but given up as impracticable. Consideration was also given to electro-magnets, but experiment showed that the magnets would require to be very large and costly. It is possible, however, that large magnets, some tons in weight, formed of the iron waste product from engineering shops, and which can be got for one-eighth of the cost of iron wire, may prove a simple means for distant communication.

Communication with lighthouses on rocks of considerable area, such as Sule Skerry and the Flannans, the former 40 feet in height and distant 40 miles from land, and the latter 280 feet in height and distant 20 miles, involves more costly installations, and if the Lodge-Marconi system can work at such distances (where no existing submarine cables lie on the bottom) without involving difficulties which have been pointed out, it would be of great value. The supposed action of the earth in preventing induction with horizontal coils is largely imaginary. What experiments have been made show that there is no such great retarding action, no doubt due to the fact that the action is dependent on conductivity per square inch, not on total conductivity as is the case with conduction. The damping of the back wire of coils which I have suggested may prove a great gain in efficiency.

It is possible that great advantage may be found in a battery of electro-magnets or in the combination of large horizontal coils or parallelograms of iron hawser of several turns wound with copper wire, the two sides thus forming long horizontal electro-magnets, the action from the back iron wire thus being damped, the copper coil thus forming another long horizontal magnet.

Coast Protection, &c.—The direction of winding must not be continuous, or the inductive action is annihilated. In respect to Coast Protection, there is a great future for electricity in order to warn vessels at night, but especially during fog, of their approach to the coast by ringing a bell on board, but it does not seem that the Hertz wave method is at all applicable. It appears to me, however, that the system with a long shore-line, or a submarine cable with a coil on board ship, or a magnet let down so as to pass near the submarine cable, is most suitable; in the first case the action having to be sent 4 or 5 miles, in the latter through only a few feet of salt water,

say, from 30 to 40 feet. Both these systems seem well suited to meet the necessities of the case. Mr. Stevenson.

The CHAIRMAN: There are a few points I should wish to submit to Mr. Marconi. The question has been raised, What advantage there is, or why there is an advantage, in having these vertical conductors earthed at the bottom? Well, it seems to me there is one advantage that has not yet been pointed out; it is, that the earth below the vertical reflector plays the part of a mirror placed directly underneath. In a great many electrical problems one may with advantage introduce optical ideas. Some years ago, in conjunction with Mr. Miles Walker, I pointed out that a good many electro-magnetic problems might be solved by the application of the idea of masses of iron acting as a magnetic mirror. I will give an illustration. Suppose you have in the coil of a solenoid (let us say a couple of inches in diameter, and six inches long) a certain number of turns of wire traversed by a current. That coil will have a certain magnetic moment,—it will produce a certain magnetic field at some place at a distance. If you put up against the end of that solenoid a large sheet of iron, sufficiently thick and with sufficiently high permeability, it will practically act as a mirror, and the coil with the sheet of iron placed behind it will have exactly double the magnetic moment that it had before; it will act as if you had a second six-inch coil end to end with it, and will be equal to a coil of a foot long. In the vertical conductor used by Mr. Marconi there are electric oscillations going up and down. If it ended at the top and bottom there would be a certain length for those electrical oscillations to traverse; but if a great conducting sheet be placed at the bottom, the length of the conductor is practically doubled, and the sheet acts as a mirror. You are no longer simply working from one end to the other of the vertical conductor, for the latter is practically connected to something which acts like an equal length down below. You have virtually doubled the length of your line by connecting to the earth, and therefore you ought, according to the rule of the square root of the heights, to be able to reach the square root of two times as far as if you did not so connect the line to earth. The Chairman.

Again, the relative reflecting powers of the earth and of water have been mentioned to-night by Mr. Gavey, in considering the problem how it is that a greater distance can be reached over water than over the earth. Mr. Marconi touches on it in his paper, and he throws out the suggestion there that one reason why a vertical conductor appears to behave better than an equally long horizontal conductor is because it has something to do with the polarisation of the waves; that is to say, with the direction of their plane of motion. Here again one may, I think, introduce optical ideas with advantage. How do reflecting surfaces act on polarised waves of light? Suppose one has a horizontal surface, will it more readily reflect vibrations that are executed in a vertical plane, or vibrations that are being executed parallel to the surface in a horizontal plane? As a matter of fact, we know that a transparent reflector, such as glass, for example, reflects far more readily the horizontal vibrations. As you are aware, the way to polarise light by reflection is to let the light come down at a certain angle most

The Chairman

suitable for the purpose, and the components which are reflected are those which execute vibrations parallel to the surface; whereas those of which the vibrations are executed vertically dive into the surface, and are refracted. There is therefore something here which needs further investigation. May we infer that the vertical vibrations—those sent off by a vertical conductor—are those which are best for the purpose? and if they are carried further over smooth water than they are over rough land, can we think the phenomenon has anything whatever to do with reflection? One would expect the horizontal vibrations to be better reflected and carried to a further distance. That, however, introduces a new question, because a transparent substance such as water or glass, when reflecting polarised vibrations, does not do so in virtue of any absorbing properties. Glass is a non-conductor of electricity. If we deal with metallic reflection, of course we find a different set of laws, and a very complicated set, as regards the polarisation of light. When we are dealing with reflection of waves of this large kind the conductivity of the material has to be taken into account, as indeed we were discussing not very long ago in connection with Mr. Evershed's communication. Well, it is quite clear that if we have horizontal instead of vertical vibrations, they may much more readily degenerate into heat in electric currents, when they come in contact with the reflecting surface, if that reflecting surface is not a perfect, but only a partial, conductor, as indeed the earth is, and all metals are. I think, therefore, it is possible to explain the greater absorption of the waves when they go over land as compared with water, by assuming that the conductivity of the earth for this purpose is greater than that of water.

Then, again, in respect to the use of mirrors, optical principles will help us a little here, because it is no use applying a mirror to a thing that is giving out light if the mirror is put within a quarter of the wave length of the light. Indeed, Hertz has pointed out that the region in the immediate neighbourhood of an oscillator is not a region where the mirror ought to be put. If you refer to his researches, and see the picture of the large parabolic mirrors that he employed, you will see that they were mirrors of, I think, something over six feet high, and the vertical oscillator which he used was a little under, perhaps, one foot long. The mirrors were much larger than the oscillator, and were put at more than a quarter wave length away from that oscillator. If we are going to do that with a vertical conductor that is eighty feet high, we shall want parabolic mirrors five or six times as high as that conductor, and must not place them within a quarter of the wave length—and I take it the wave length is about twice as long as that vertical conductor—so that they would be enormous things if they were of any benefit at all. If they are going to be put any nearer than a quarter of the wave length they will not act as mirrors, but as so much absorbing material in the neighbourhood of the conductor. We must keep these principles in mind; we must keep these optical guides that we have, which will suggest possibilities or impossibilities of applying apparatus which other experience may have suggested to us.

As Mr. Marconi is going to take the opportunity of replying when he repeats his demonstration, and as that is not an Ordinary Meeting,

and we shall transact no formal business that night, I move that a hearty vote of thanks be accorded to Mr. Marconi for the paper which he has been so good as to read to the Institution. The Chairman.

The motion was carried by acclamation.

Mr. G. MARCONI, in reply [*communicated*]: Before proceeding to reply to the questions asked by those gentlemen who have taken part in the discussion, I wish to thank the members of the Institution of Electrical Engineers for the kind way in which they have received my paper, which is my first paper in the English language. Mr. Marconi.

I am glad to note that many results obtained by Professor Fleming coincide with my experience. He has found that a well-made coherer is not the uncertain, treacherous, and unreliable instrument it has so often been stated to be, and he has also noted the advantages obtainable by the use of the vertical conductor attached to the instrument. The reason why the employment of oil in the spark-gap is no advantage (rather in my opinion a disadvantage) is because the oil seems to offer a very variable opposition to the passage of the discharge. Thus, if the oscillator containing the oil has not been used for, say, two or three hours, I find that the potential required to pass a spark through it is then far greater than what it is after a few moments of work.

It seems as if the continuous discharge maintains a kind of hole or channel in the oil after the first discharge, and therefore if the receiving instrument is at a considerable distance (that is near the limits of the distance over which it is possible to signal), I find that while the dots, which are produced by single impulses, are all recorded, the dashes in great part miss. When a comparatively large capacity is connected to each of the discharging spheres, air seems to be quite an ideal substance in the spark-gap, and curiously enough, as I have already stated, better and absolutely constant results are obtained if the spheres are never polished, which is contrary to what one might expect.

With reference to the coherers, I have found that the use of mercury is necessary in order to obtain the best results. I find it also advantageous, or rather, so far as commercial work is concerned, necessary, that the tube should be exhausted to about 1 millimetre. The tubes we make are always tested over a distance of 18 miles, and if they fail to give signals at that distance with a moderate length of vertical wire, they are not considered satisfactory.

Although signals have been received at times over that distance by non-exhausted coherers, no reliable results have been obtained with such tubes at that distance. Perhaps if we employed a vertical wire 200 feet long instead of 75 feet, we might obtain fair results even with non-exhausted tubes; but a mast 200 feet high would be a somewhat clumsy piece of apparatus.

I have also noticed that tubes or coherers containing air seem to be always diminishing their sensitiveness. This would not do for commercial installations.

In reply to the questions asked by various members as to the necessity of having the earth connection, my experience has been that in order to obtain the greatest possible distances, with a given height, the earth connection at both receiver and transmitter is necessary. W,

Mr. Marconi.

however, suitable capacities are substituted for the earth, communication can still be maintained, although at a somewhat shorter distance than when the earth connection is used.

The results obtained when capacities are used in place of earth are similar to those obtainable when a condenser is inserted in the earth connection, and I believe that the result of connecting the earth terminal of one of the instruments to a capacity area is equivalent to inserting a condenser of a similar capacity into the earth connection.

As has also been noticed by Mr. Gavey, a small capacity is sufficient on the earth wire of the receiver to enable one to obtain good results, whilst a much larger capacity is required at the transmitter. The fact that effects can be obtained without earth connection might seem at first sight as if proving that the earth had nothing whatever to do with conducting oscillations from one station to another, the possibility of which I pointed out in my paper; but it seems to me that even if a great part of the oscillations are conducted by the earth, the fact of disconnecting one or both instruments from earth and substituting for the actual metallic earth connection a capacity area is no proof that the earth is unnecessary. With no capacity on the earth end of the coherer or radiator it is impossible to obtain results, and if a capacity is to be there (especially if the said capacity is near the earth or any earthed conductor) is not that equivalent to a condenser inserted into the earth connection?

If we consider with what very rapid oscillations we are dealing, it will be found that a very small condenser, say 1/100,000 microfarad capacity, is more than ample at the receiving end for allowing the dielectric currents to pass from or to the earth.

It is my opinion that unless we could transport the whole apparatus to a very considerable distance from earth or any earthed conductor, it would be impossible to come to any definite conclusion as to whether Mother Earth is or is not taking a very important part in this phenomenon, as I take it any capacity area connected to the instrument is only one plate of a condenser, of which the other plate is the earth.

It may surprise many of you if I state that with the instruments connected to earth, if you disconnect the vertical or aërial conductor from the receiver, you can still receive good signals at a distance of many miles. Here one might say that the effect must necessarily all come from the earth connection, but in this case we find that the lower end of the vertical conductor must be in proximity to the receiver, and from this lower end the oscillations jump across the gap in the same way as the oscillations would traverse the insulation separating the two plates of the condenser.

Over a distance of 14 miles, that is from Alum Bay to Bournemouth, I found that I could get signals even if the aërial conductor was separated from the terminal of the receiver by a gap more than 1 foot wide. In this case a metal plate about 1 foot square was attached to the terminal of the receiver, and to the end of the vertical wire. This seems to show that actual metallic connection between certain parts of the *instruments* is not always necessary, as with these high frequencies

oscillations can bridge over gaps which may exist in certain connections, in the same way as they traverse the dielectric of condensers. The results I have pointed out show that it is not necessary to have an actual metallic connection to earth, as that connection may be effected through the ether. It is therefore of no great importance for an installation, such as would be at the Fastnet Lighthouse, to have what telegraphists call a good earth, as a capacity area connected to the earth terminal of the instrument would be quite sufficient. In the case of the Fastnet Lighthouse, its being mainly built of boiler plate would afford an excellent capacity on which to connect the earth terminal of the instruments. Mr. Marconi.

Mr. Miller states that he has noticed these results in experiments which he has carried out on a smaller scale.

With regard to Captain Brett's statement that the results of obtaining signals with the earth off at the South Foreland Lighthouse came as a surprise to me, I would like to explain that the surprise was not caused by the receiver working with the earth off, but because I thought there was not sufficient spare power received to enable the receiver to work when the earth connection was substituted by such a small capacity as was used in that case.

Mr. Granville informs us that at the Fastnet Rock it would be impossible to get a radiating or receiving conductor more than 10 or 20 feet away from the mass of conducting metal ; but as the distance to the Fastnet from land is small, I believe less than 10 miles, I do not anticipate that there would be any difficulty in working that installation by means of my system. At Rathlin Island the conductor was suspended from the lighthouse and very near to it. On the Royal yacht the wire was sometimes almost within sparking distance of one of the funnels, but good results were always obtained.

On the East Goodwin lightship installation the wire is very near the iron mast of the ship, and surrounded by a great number of wire stays.

In reply to Mr. Gavey's remarks, I wish to say that in connection with the law of height, as you will see from my paper, where I quote a letter written to Mr. Preece on the 10th of November, 1896, this law had already been discovered, and had also been fully discussed in the Italian technical press.[1]

It is very interesting to know that Mr. Gavey working independently at Dover came to exactly the same conclusions as to this law. In connection with the height required for working over land and over sea, I have no doubt that over water or over perfectly open space it is easier than over undulating land, but my experience is that the difference is by no means so marked as that which Mr. Gavey finds, viz. : 35 feet over water and 50 feet over land.

At Salisbury, in the autumn of 1897, I found that 30 feet of vertical height was sufficient for 1 mile, and it was not until the apparatus was considerably improved that I was able to reduce the height at sea to

[1] See *Elettricista*, August, 1897, and work of Della Riccia in the *Rivista di Artiglieria e Genio*, 1897–1898 ; also lecture of Professor Bongiovanni at Ferrara, November, 1897.

Mr. Marconi. less than 20 feet. I am not at all certain that this height will not carry us practically the same distance over land as over sea, although I quite believe that over water the maximum results will always be attained. I have mentioned in my paper that signals were easily obtained between the Royal yacht and Osborne House, even with 9 miles of land intervening. The average height at each end being 90 feet, gives us the figure of 30 feet height for the first mile, although the said 9 miles were not across clear space. In my experience it is not necessary for the stations to be situated at a height over the water in order to obtain the best results. Indeed I am not at all certain whether having the base of the vertical wire on a height is any advantage. At Spezia the land station is on the sea level, and at Kingstown during the regatta the station was only about 20 feet above the water level, the corresponding station being on a ship in the Irish Sea, and it is worthy of note that it was off Kingstown where the greatest distance yet reached in telegraphing over water, that is 25 miles, was obtained.

Alum Bay station is on a height of about 200 feet, but the corresponding station at Poole has its pole fixed in the sand below high-water mark. As to whether 20 feet is a practical working height with a sufficient factor of safety, I can confidently state that it is. As a matter of fact, we are working across 18 miles with a height of 75 feet to-day, and this height could readily be reduced by 10 feet at each end if there were any object in doing so. We never adopt a height unless the signals prove to be absolutely accurate, one condition being that several cipher messages have to be transmitted every day.

In connection with reflectors, I notice that Mr. Gavey believes that the undulations of the ground may have been the cause of the experiments made with reflectors at Dover being unsatisfactory at a greater distance than half a mile. His results do not, however, in this case coincide with my experience, as my first experiments, carried out with much smaller reflectors in the presence of Post-office officials, were made over broken ground at Salisbury, and there, without difficulty, we got signals up to a distance of 1¾ miles. I quite agree with Mr. Gavey that attention should be devoted to working with reflectors, and I hope to give more consideration to this very shortly.

With reference to what Mr. Gavey says about tuning, I quite agree with him that it is difficult to obtain marked results by altering the length of the vertical conductors, or even if the said conductors are wound up into a coil or solenoid ; but there are other methods which I am confident will allow of tuning, and I propose repeating on a larger scale some experiments I carried out on a small scale some time ago. It is my opinion, however, that, say in lightship installation, tuning would be a disadvantage, as if Lighthouse A was tuned to Lightship B another lightship that might be tuned to a different note or pitch could not call up A or C, and this would prove to be a disadvantage. However, as soon as the cross-channel installation is started, I intend trying some syntonic experiments on a larger scale, and have little doubt as to the result.

In connection with danger from lightning where a vertical conductor is used, I think Mr. Gavey rather over-estimates this. Although

numerous vertical wire installations have been working for nearly two years, no wire has ever been struck by lightning. On the coast of Italy, where many installations have been erected and where thunderstorms, especially in the summer, are much more frequent and violent than in this country, nothing has ever happened. A lightning conductor higher than the top of the vertical wire can, however, be fitted in the ordinary way if there is any desire for it, without in any way interfering with the efficiency of the system. On board the Royal yacht *Osborne* the top of our wire was well below the top of the lightning conductor. We have, however, anticipated the possibility of danger by having an arrangement whereby the assistant has never to touch or handle the aërial wire, which is always connected to earth both when transmitting and when receiving. This prevents any accumulation of atmospheric electricity on it.

I shall be glad to show Mr. Gavey this system in operation any day that he may care to visit one of our stations.

The method I adopt to screen the local receiver from the effect of the violent surgings of the transmitting apparatus is, as I suggested in my Patent of 1896, to enclose all the receiving apparatus, with the exception of the inker or recorder, in an earthed metallic box. As some oscillations picked up by the inker and connections would, by travelling along the leads into the receiver, injure the coherer, I choke off such effects by interposing suitable choking coils between the inker connections and the terminals of the receiver. These choking coils consist of a few turns of insulated wire, each layer being separated from the next by means of sheets of tinfoil in electrical communication with the box. The earthed tinfoil prevents the oscillation passing inductively from one turn of the choking coil to the other. The earthed terminal of the receiver is connected to the box and need never be touched.

In connection with the use of reflectors with the vertical wire system, I am of opinion that considering the length of wave radiated (calculated by Professor Ascoli to be four times the length of the vertical conductor) they would require to be very large indeed to be effective. As to the effect of screening or of interposed objects, I described in my paper several instances in which intervening hills did not in any way interfere with the transmission of signals. Captain Jackson, of H.M.S. *Defiance*, Devonport, also obtained good results even when battleships intervened between the two stations. Lieutenant Della Riccia worked for some time an installation in Brussels between two positions about 400 yards apart, although some nine houses and the Metropole Hotel, which is constructed in a great part of iron, intervened between the two stations. His vertical wire was only 30 feet long.

In reply to Mr. Granville's questions as to whether I found it necessary to use accumulators for working the induction coil, I wish to state that although we are now using accumulators at several of our stations, their use so far is purely experimental. Excellent results have been obtained by dry cells alone, and a battery of 100 M size, although subjected to very heavy work, remained efficient for rather more than 10 months at Alum Bay.

The lightship installation is now worked by dry cells alone, and I am confident that the battery will last for much longer than a year.

Mr. Marconi.

I may also mention that the installations at Osborne House, on the Royal yacht, and at Kingstown, were worked by batteries of 50 cells, no accumulator being used. As to the question whether the transmission of signals is effected purely by radiation or by conduction of the vibrations along the surface of the earth, I am rather uncertain as to the reply that should be given. Doubtless, over clear space, radiation is the chief factor. As I have noticed—and I believe Professor Fleming has found the same—if the two conductors are slanting, it is necessary in order to obtain maximum results that they should be parallel to each other. A great number of experiments will be necessary in order to throw more light on this very interesting point.

The reason why signals are not sent over greater distances at these demonstrations is because I did not think it desirable to have to divide the audience, and in this hall no second room is available, as it would certainly be desirable to have a certain number of persons at each station. Another reason is that through lack of time, I have been quite unable to make more elaborate arrangements. Many members here present have seen working carried out over long distances, and if any one would like to inspect any of our stations where long-distance work is done, I shall only be too glad to afford him every facility for inspecting the said installations working.

In reply to Mr. Miller's question, it is my experience that nickel filings mixed with a very small percentage of silver and mercury are the best metals for reliability and sensitiveness. As to the question of the radiations being reflected by the surface of the sea or land, I am inclined to believe that this may possibly have something to do with the results. We have, however, no sure proof or experimental test of how much or in what measure the reflective waves affect the results.

Concerning Professor Thompson's views on the subject, I believe his theory may afford an explanation which I had never thought of before. I am very much indebted to him for his suggestions, and hope to have the opportunity of testing his theory experimentally. I would like, however, to draw his attention to the fact that the earth need not always be at the bottom of the aërial conductor, as during certain experiments I often had the earth at the top.

At Alum Bay I suspended the aërial conductor over the edge of the cliff, the instrument and earth connections being at the top, whilst the lower end of the conductor, about 100 feet long, hung free in space. I found that when the wire was held at about 30 feet from the cliff it was quite easy to speak to Bournemouth, 14 miles distant. It is to be noted that in this case no vertical pole was used for suspending the wire, as the cliff fulfilled that purpose. In a similar manner the wire might be suspended from the gallery of a lighthouse, in this way dispensing with the pole.

The Chairman.

The CHAIRMAN: The next business arranged for this meeting is a demonstration by Mr. Campbell Swinton, of the new electrolytic current interrupter invented by Mr. Wehnelt.

DEMONSTRATION OF THE WEHNELT ELECTROLYTIC CONTACT-BREAKER.

By A. A. CAMPBELL SWINTON, Member.

Mr. CAMPBELL SWINTON: Before I proceed to show the Wehnelt electrolytic contact-breaker in action, I desire to make one or two preliminary remarks. I think it was Professor Fleming who, in the discussion upon Dr. Lodge's paper, gave the real reason why there is great difficulty in obtaining syntonic results in wireless telegraphy on the Hertzian wave principle as opposed to the magnetic induction principle. The real reason why there is difficulty in getting syntony with the Hertzian system is because there are at present no known means of obtaining continuous trains of Hertzian waves. It is, in fact, only possible to obtain trains of such waves the total duration of each of which trains is very short as compared with the intervals between succeeding trains.

FIG. 1.

Some five years ago I made numerous experiments upon the continuity of electrical oscillations, the results of which are, I think, instructive in this connection. The experiments consisted in analysing disruptive electric discharges by means of a very rapidly revolving mirror, which reflected the images of the sparks on to a sensitive photographic plate. In this way the several oscillations of which each spark was composed showed themselves as bands in the resulting photograph. These bands could be counted, while the intervals between succeeding sparks could be measured and compared with the total duration of each spark and the duration of each oscillation. I endeavoured to obtain as many oscillations as possible, but so rapidly did the oscillations die away, I could never succeed in detecting more than

five or six oscillations in a single spark. Fig. 1 shows diagrammatically the kind of result obtained. The sparks were produced by means of an alternating current with 100 complete periods per second. Sparks consequently occurred every two-hundredths of a second, alternately in opposite directions. Each spark consisted of a train of five or six oscillations, and its total duration was about $\frac{1}{50000}$ second. Consequently, the spaces between the succeeding trains of oscillations when nothing was happening at all were each $\frac{249}{50000}$ second duration. In the diagram shown on the wall it has not been possible to show the spaces to scale, as if on the same scale as the oscillations, each complete train of which occupies 10 inches, the spaces ought to be no less than 208 feet.

I show this to give some clear indication of the very enormous gaps that there are in practice between these trains of oscillations. As a matter of fact, when produced in the way Mr. Marconi produces his discharges with an ordinary vibrating contact-breaker, the gaps are very much greater than those I have shown, because an ordinary vibrating

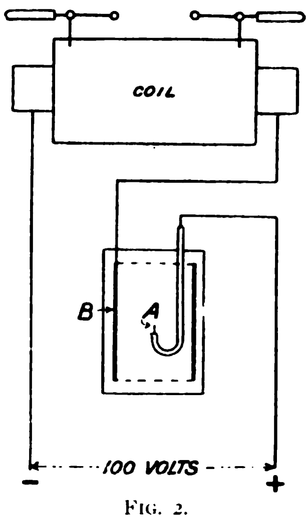


FIG. 2.

contact-breaker with a large coil will only give something like 30 or 40 sparks a second; whereas in the case shown in Fig. 1, I was working with alternate current, and obtaining 200 sparks per second. It is obviously quite impossible to get anything in the way of tuning or syntony when the total duration of each train of oscillations is so very short compared with the gaps between succeeding trains of oscillations.

Quite recently a new form of contact breaker for induction coils has been devised by Professor Wehnelt, of Charlottenburg. This contact-breaker, which in many ways is a very remarkable discovery, has the great advantage that it renders it easy to get discharges from your induction coil, not as with ordinary contact-breakers, at 30 or 40 a second; not as is obtainable with alternating current at, say, 200 a second, but at the rate of at least 1,000 a second. I am not at all sure

that under suitable conditions you might not get a much higher rate even than this. The subject requires further investigation, it being quite new.

The Wehnelt contact-breaker is shown in Fig. 2. It consists merely of a glass vessel which contains dilute sulphuric acid, about one part of acid to about five parts of water. In this dips a large cylindrical leaden plate B, and also a glass or ebonite tube A, through the end of which is sealed a platinum wire, to which connection can be made through the tube. The exposed platinum wire is entirely immersed in the liquid. This is the whole apparatus, which is beautifully simple and inexpensive. An ordinary induction coil is used, but the condenser and usual contact-breaking arrangements are dispensed with. The primary of the coil is simply connected straight on to the 100 volt supply from the mains, with the electrolytic cell in series. The only thing you have got to be careful about is to see that the platinum wire electrode is made the anode. It will not work if the lead plate is made the anode. What the exact action is seems fairly simple at first sight, but when one goes into it further it appears more complicated. Oxygen is given off at the anode pole, and one can imagine that on the current being started sufficient oxygen is rapidly formed round the pole to make an insulating sheath, and thereby interrupt the current. The oxygen is then absorbed by the solution, and the current starts again when the process is repeated. One reason possibly why the action takes place only when the platinum wire is made anode, and will not take place when the platinum wire is made cathode, is that oxygen is more soluble in water than hydrogen, and it is obvious that the gas must be soluble in the water, or else it would not disappear. That is a very simple explanation, but as a matter of fact the action does not appear to be quite so simple. I do not know the complete explanation, but in addition to the electrolytic effect there is a great deal of heat developed, and there is in fact an intermittent arc formed at the end of the platinum wire, which does not reach so far as the lead plate, but takes place between the wire and the solution. The distance between the wire and the lead plate does not appear to matter. Another curious and important point is that this contact-breaker will not work on a non-inductive circuit. It will not work if instead of having the primary of an induction coil or an electro-magnet in circuit with it, it is connected to a non-inductive resistance. Further, the amount of self-induction in the circuit affects the rapidity with which the circuit is made and broken. It is thus obvious that the explanation is not so simple as it at first sight appears. Probably electrical resonance has a good deal to do with the matter. Here I have an induction coil connected to one of these contact-breakers. I have inserted in series with the contact-breaker and the coil an electro-magnet, which has a movable core. What I wish you to observe is this, that as I draw out or in the core of the magnet, and thus alter the amount of self-induction in the circuit, the pitch of the sound produced by the discharge changes. The periodicity of the discharge in fact depends upon the amount of self-induction in the circuit, the periodicity being lower the greater the self-induction, and *vice versâ*. With less than a certain amount of self-induction, however, the apparatus will not operate. Again the periodicity

is found to vary with the voltage applied. It is higher the greater the voltage, and with low pressure of 30 or 40 volts the periodicity drops down to the rate of 40 or 50 per second. Another condition which determines the frequency is the area of the platinum wire surface exposed. Other things being equal, the greater the exposed surface the lower the frequency. By varying the amount of platinum surface it is also possible to control the strength of current passed.

As will be seen, with high periodicities of 1,000 or so per second, the discharge from the secondary of the coil is very powerful. It appears to partake much more of the nature of a flame than of an intermittent discharge, but if examined in a moving mirror—a revolving mirror is best—it can be seen that the discharge consists of a number of separate sparks. If you at the same time alter the amount of self-induction, or the voltage, you can see by means of the mirror that the periodicity varies. I do not think I have much more to say, except that it is a very surprising phenomenon how this thing works. It appears to me to be a very important discovery in many ways, and it may lead to great developments. It seems important from the point of view of wireless telegraphy, and I think very likely in other ways also, because it is the first real advance that has been made for many years towards producing continuous interruptions at high frequency, such as are required for many purposes. It is already possible to get up to 1,500 interruptions per second and more, with this very simple apparatus, which has no moving parts. With the improvements that experience and further experiment will no doubt indicate, it seems to me highly probable that this rate may be eventually greatly surpassed, even possibly to the large extent necessary to produce almost absolutely continuous trains of Hertzian waves, such as are required for perfect syntony in wireless telegraphy.

Mr. Carter.

Mr. TREMLETT CARTER: I should like to ask whether the facts observed by this interrupter would in any way suggest that it acts like a Trevelyan rocker. It is possible that the heat generated causes the production of steam, which would insulate the positive electrode, the subsequent condensation of the steam allowing contact to be made again? If the alternations between heating and cooling caused by the passage of current produced such an effect, there would be some vibration very much like that of the Trevelyan rocker.

Mr. Swinton.

Mr. CAMPBELL SWINTON: The temperature obviously has much to do with the action. The cell becomes very hot, and, as I mentioned, an arc is formed at the end of the platinum wire. Steam may have very much the same effect in this case as the gas due to electrolytic decomposition, but neither the one nor the other will account for the fact that the contact-breaker will not work upon a non-inductive circuit, and that when it is on an inductive circuit the amount of self-induction in the circuit, at any rate to some extent, determines the frequency..

The Chairman.

The CHAIRMAN: We owe Mr. Campbell Swinton very hearty thanks for having brought this illustration before us. Before we dismiss the subject I would like to remark that there is clearly something in the nature of a resonance taking place here. It has long been

common knowledge that a deposit of gas upon an electrode has the effect of making that electrode act to a certain extent like a capacity. There is a very well known research of the late Mr. Cromwell Varley on the capacity of polarised electrodes. If this small polarised electrode acts as a capacity, and if there is self-induction in the same circuit, we clearly have all the elements necessary for a resonance, provided that an instability is once set up. That instability is set up and that there is a periodic discharge of current in such a circuit is a phenomenon that has been known for a good many years. There is a very able paper, published in 1881 in the *Journal of the Russian Physico-Chemical Society*, by Professor Slouguinoff, upon the intermittence of the discharge by a similar electrode, when a sufficiently high electromotive force is applied. The intermittence was shown by means of a rotating mirror. That probably is known to Mr. Swinton, so I need not say any more about it.

The following paper was then read:—

THE INDUCTION MOTOR.

By Professor ERNEST WILSON, Member.

INTRODUCTION.

The advent of the Induction Motor may be said to have marked an epoch in the use of alternate currents for power distribution, for, up to the time of its introduction, the only alternate current motor for comparatively large sizes was of the synchronising type. This type of motor, as is well known, should be run up to such speed that the frequency of the armature corresponds to the frequency of supply before being switched on to the circuit. For small sizes the ordinary direct current motor with laminated magnets can be, and was, employed, but the want was felt of a motor possessing the characteristic features of the direct current type, and capable of dealing with considerable power. The transmission of energy by the aid of electricity for the propulsion of tram-cars and electrical locomotives may be cited as an example in which, one may say, until quite recently, the direct current motor has been exclusively used. In this case a motor is required which is capable of exerting an initially great torque for starting purposes, it must be easily controlled by drivers, and must be efficient. Consider the case in which power is transmitted from a distant waterfall to a district in which electrical traction is required. We should probably find that the turbines are coupled to two- or three-phase alternate current

generators which supply directly or through step-up transformers to the line wires. At the receiving end the energy is delivered to a motor generator directly, or through step-down transformers, such motor generator converting this energy into the form of direct current for transmission to the cars by any of the well-known systems. This motor generator is of the synchronising type, and should be run up to synchronising speed before being switched on to the supply circuit. Now if alternate currents could be employed on the car itself, it follows that no running machinery would be required in the sub-station, since the requisite reduction of potential difference could be accomplished by static transformers. At the present time the three-phase induction motor is being used at Lugano on the street cars, and is about to be installed on other lines, so that it is actually employed in practice. Now for two- or three-phase currents to be transmitted to the car it requires three conductors; if the rail be employed as one, then two other insulated conductors are required—these are overhead at Lugano. According to usual construction, the motor, even if single phase, requires two- or three-phase currents to start properly; that is, to give an initial torque of the required magnitude. The subject therefore has great practical importance at the present time. The primary object of this paper is to investigate in an actual motor the conditions upon which the starting torque depends.

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THE MOTOR.

The motor experimented upon was constructed by Messrs. Siemens Bros. & Co., and lent, by Mr. Alexander

Siemens, to the Siemens Laboratory, King's College, London. Figure 1 is a side elevation, and Fig. 2 an end

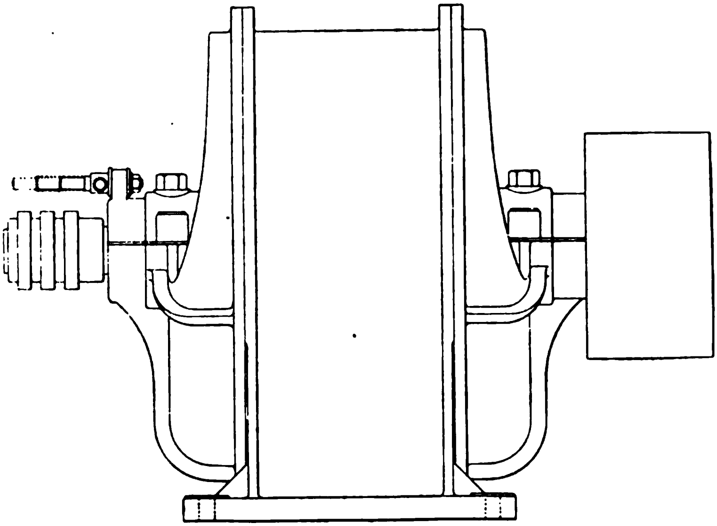
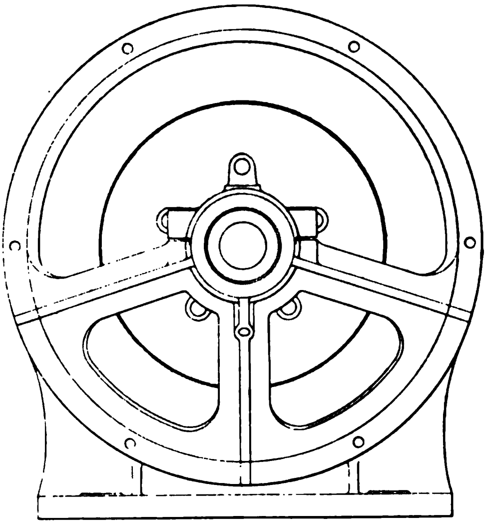


FIG. 1



Scale = ·084 of Full Size.
FIG. 2.

view of the motor which is intended to deliver 15 brake horse-power when running at 1,000 revolutions per minute,

and supplied with a single-phase alternate current at 220 volts, the frequency being 50 periods per second.

In order to measure the initial torque, a beam having a radius of 3·5 feet is fixed to the shaft, and the pull in pounds measured at this radius by a spring balance, the armature being brought to its zero position by raising the spring balance by aid of a rope passing over a pulley. The actual observations of pull are at the best rough, since the readings when small are seriously affected by the friction of brushes and bearings, but they are sufficiently accurate for the purpose of this paper. Fig. 2A shows details of the armature, and Fig. 6 gives details of the field and armature stampings. The primary or stationary portion, Fig. 6, is wound with two circuits marked respectively A and B. These circuits are so distributed that two-phase currents are required to give an approximate rotating magnetic field for starting purposes, and when once started the motor is intended to work on the single phase. The conductors are placed in holes, the main winding B consisting of 156 bars, and having a resistance of ·038 ohm. The auxiliary winding A has 144 bars, and resistance ·045 ohm. The conductors A B in the stationary portion are not parallel with the axis of the machine; that is to say any one conductor would not lie in a plane or be co-planar with the axis of the machine. The angular distance between the plates at the extreme ends of the core is the pitch of one tooth. The secondary or armature, Fig. 6, is wound three-phase with three circuits, C D E, which are pooled together at one end, and connected respectively to three rings insulated on the shaft. These rings are connected through brushes to a non-inductive rheostat capable of varying the resistance externally inserted in each circuit by steps as follows: open circuit, 2·37, 1·03, 0·42, 0·13 ohms at about 15° C, and short circuit; the ends of these three external resistances in the rheostat remote from the brushes are pooled together. Each armature circuit has 48 bars, and a resistance of ·025 ohm, the bars being placed in holes in the armature. There are 150 holes in the stationary portion and 72 holes in the armature, two bars being placed in each hole, and the number of periods per revolution is three. The thickness of iron left between the punched holes and the surface is $\frac{1}{2}$ mm. in the armature and $\frac{1}{2}$ mm. in the stationary

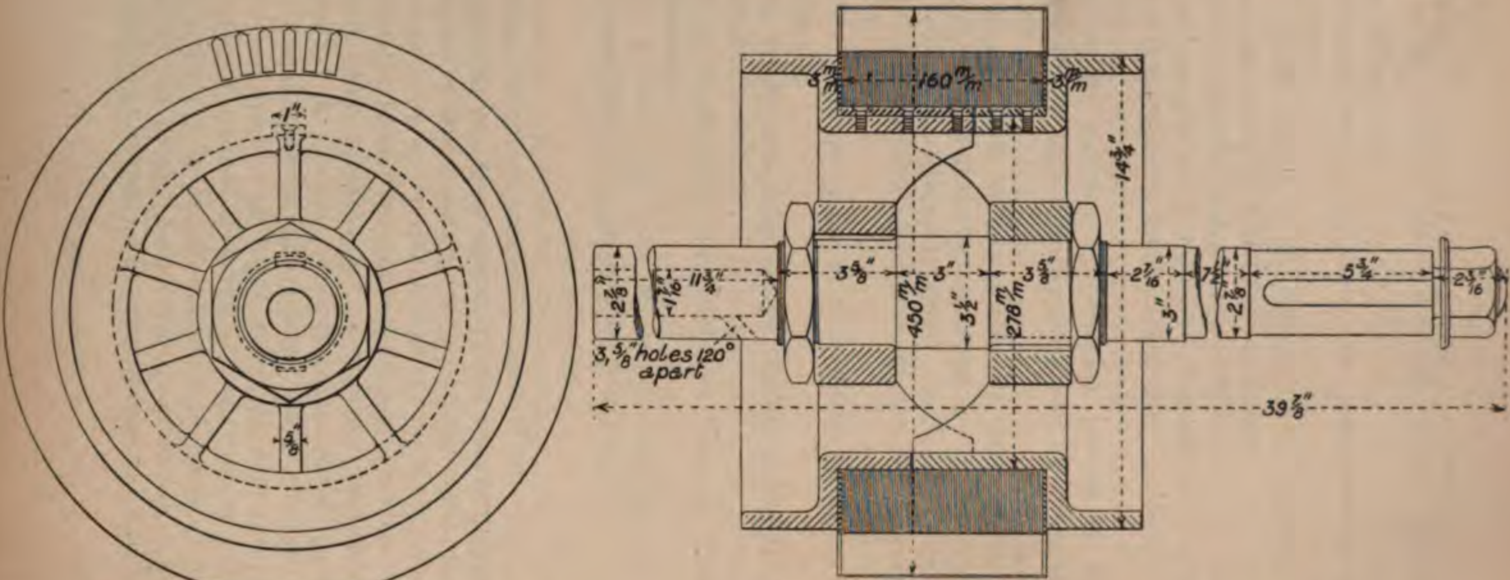

1"
5/8"
160 m/m
3 m/m
3 m/m
14 3/4"
2 7/8"
1 7/16"
11 3/4"
3 5/8"
3"
3 5/8"
2 1/16"
7 1/2"
5 3/4"
2 3/16"
450 m/m
3 1/2"
278 m/m
3"
2 7/8"
3, 5/8" holes 120° apart
39 7/8"

FIG. 2A.

position. The radius of the armature is 22·5 cms., and the length of core parallel to the shaft 16·6 cms.

The coils A B were supplied with currents from two Siemens Alternators[1] separately, or, if required together, in any desired phase. By aid of a Kelvin Quadrant Electrometer and revolving contact maker fixed to the shaft of the alternators, the currents in the respective circuits have been deduced from observations, at different positions of the phase, of the potential difference between the ends of a non-inductive resistance in the circuit. The potential differences across the circuit were reduced when necessary to suit the sensibility of the electrometer by aid of considerable non-inductive resistances forming a shunting ratio.

Symbols.

In the paper the following symbols are used :—

E_1, E_2, are the volts across the circuits A and B respectively.

I_1, I_2, are the total inductions per turn in C.G.S. units in the circuits A and B.

m_1, m_2, are the turns in the circuits A and B. $m_1=72$, $m_2=78$.

x_1, x_2, are the currents in amperes in the circuits A and B.

R_1, R_2, are the resistances of the circuits A and B. $R_1=\cdot045$ ohm, $R_2=0\cdot038$ ohm.

T is the periodic time in seconds.

f is the frequency of supply in complete periods per second.

N is the number of poles per circuit per revolution$=6$.

m is the number of coils per period on stationary portion.

θ is one period of surface revolution.

l is the length of armature and stationary iron cores measured parallel to axis$=16\cdot6$ cms.

r is the radius of armature$=22\cdot5$ cms.

l_1 is the length of air space measured radially$=$about $\cdot175$ cms.

A_1 is the area of air space over one group of windings, B, in square cms.

[1] For description of these machines and revolving contact maker, see *Phil. Trans. Roy. Society*, vol. clxxxvii. (1896), A. pp. 229–252.

l_2 is the length in cms of teeth on armature and stationary portion of motor.

A_2 is the area of teeth in square cms.

l_3 is the mean length of induction in iron in cms.

A_3 is the area of iron in square cms.

I is the total induction by characteristic curve.

ν is the leakage coefficient.

B is induction in C.G.S. units per square cm.

$r_1=r_2=r_3$=resistance of each armature circuit=·025 ohm.

$\rho_1=\rho_2=\rho_3$=extra non-inductive resistance in each armature circuit; these can have values ∞, 2·37, 1·03, 0·42, 0·13, and 0 ohms.

$n_1=n_2=n_3$=conductor turns respectively on each armature circuit C, D, E.=24.

y_1 y_2 y_3 are the currents in the armature circuits respectively, in amperes.

e_1 e_2 e_3 are the electromotive forces of the respective armature circuits C, D, E, in Fig. 6; that is, they are the potential differences between the ends of the respective circuits corrected for current into resistance ·025 ohm.

THE MAGNETIC CIRCUIT.

Let the horizontal lines *l l* (Fig. 3) represent the surfaces of the armature and the bored face of the stationary portion straightened out. The ordinates represent induction density; the areas induction. The windings A, B, are diagrammatically represented ∘∘∘∘∘ and • • • • • respectively. The lower row of large circles represent the armature conductors. Consider the winding A; and suppose that it only is the magnetising coil. One would expect the distribution of induction over the bored face to take the form represented by the full line (Curve No. II.), assuming for the moment that there is no leakage. Let the area *c d j* be called K; *j d c i*, L; *i e f h*, M; and *h f g*, N. Then K+L+M+N is the total induction over the half period $\left(\frac{\theta}{2}\right)$ between *c* and *g*. If the characteristic curve for the surface *j h* be worked out in the ordinary way, the total induction I given by it will be represented by the areas L+M.

For the purpose of finding the distribution of induction

near the bored face, a series of insulated copper wires numbered 1, 2, 3, 4, 5, 6, were stretched across the surface parallel with the axis of the machine and placed relatively to the coils A and B as shown in Fig. 3; the wires have a diameter of ·2 mms. and touch the surface of the bored face. Electromotive forces have been observed between adjacent wires for different positions of the phase by aid of a quadrant electrometer and revolving contact maker attached to the axle of the alternate current generators; first when the motor was supplied with one-phase,

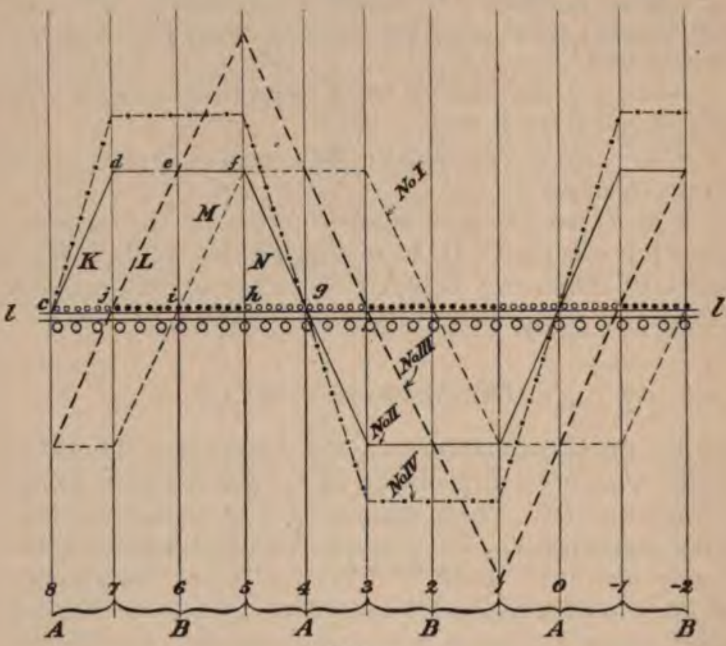


Fig. 3.

second when supplied with two-phase currents. These E.M.F. curves have been integrated, and give therefore at any epoch the average total induction over the area between the two wires considered. These wires cannot give the distribution of induction *in* the stationary portion itself, but a good deal can be inferred from the curves obtained. Deal with the single-phase curves first. The coil A was chosen for the single-phase current. Table I. gives particulars of the test in which the frequency was 51·7 and the square root of mean square value of E_1 was 63 volts, the

armature being at rest on open circuit. We see that the curves $\frac{dI_1}{dt}$ and (2–3), (3–4), (4–5), (5–6) are in phase. Their areas have been taken and give total maximum inductions of 0·37, 0·1097, 0·0723, 0·0679, 0·0891 in 10^6 C.G.S. units. The average inductions given by the exploring coils have been plotted in Fig 4, in which the abscissæ are, as in Fig. 3, the surfaces of the armature and bored face of the stationary portion of the motor straightened out. The full line in Fig. 4 has been drawn, and incloses an area per half period equal

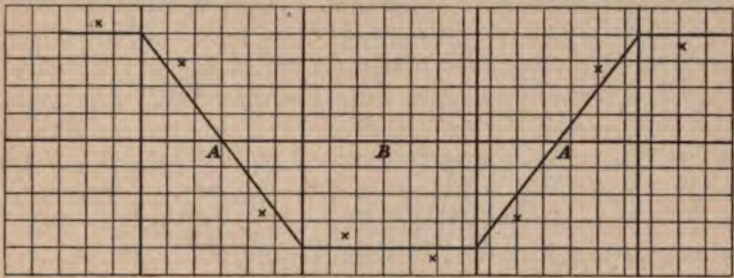


FIG. 4.

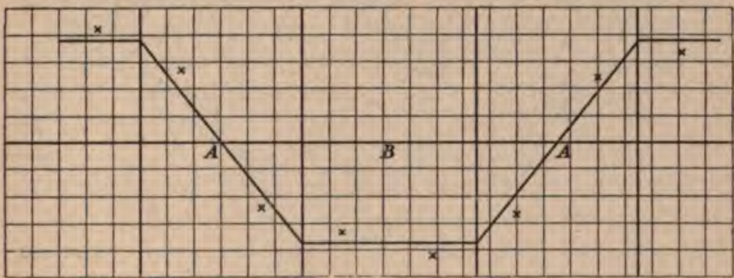


FIG. 5.

to ·1097 + ·0723 + ·0679 + ·0891, that is equal to the sum of the inductions given by the exploring coils shown in Fig. 3. Another set of curves at higher maximum induction density (Table II.) give ·676, ·211, ·135, ·122, ·168 in 10^6 C.G.S. units. These points have been plotted and the full line drawn through them as shown in Fig. 5, inclosing the area represented by ·676 + ·211 + ·135 + ·122. The frequency in this case is 56 and $\sqrt{\text{mean}^2}\, E_1$ 118 volts. The object of drawing the curves in Figs. 4 and 5 is to show that there is ground for assuming the form given in curves Nos. I. and II., Fig. 3. Assuming then in Fig. 3 that L = M = K + N., and

that K=N, one would expect the magnetising coil to experience a reaction represented by the rate of change of the average of areas L+M and L+M+K+N : *i.e.*, 1·25 (L+M). Further the exploring coils (2–3), (3–4), (4–5), (5–6), if the air space experienced the whole induction, would give over half a period $\left(\frac{\theta}{2}\right)$, $1\frac{1}{2}$ (L+M). In any case, on the assumption of distribution of induction represented by the full line, the maximum value of $\int \frac{dI_1}{dt}\, dt = \frac{1 \cdot 25}{1 \cdot 5} = \cdot 83$ of actual total maximum induction. If the distribution were

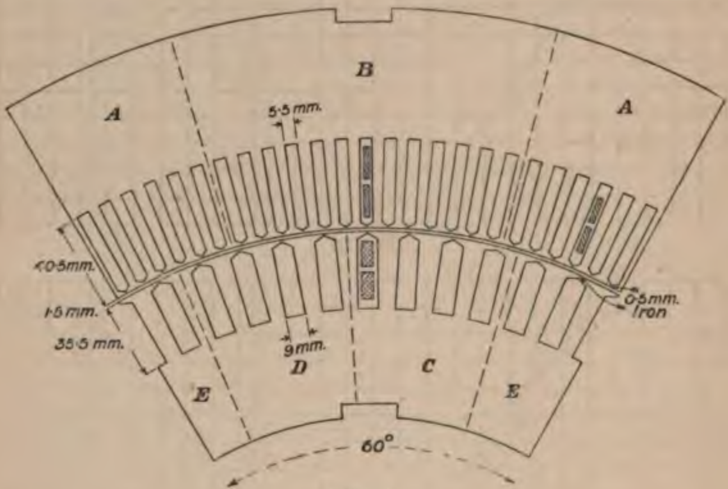


FIG. 6.

represented by a sine curve, this coefficient would be ·85, and has been obtained graphically. Referring to the figures, ·37/·83 = ·446, and ·676/·83 = ·814 in 10^6 C.G.S. units. Of these it appears that ·339 and ·635, in 10^6 C.G.S. units, gets into the air space; that is, the leakage is 24 and 22 per cent. respectively. The fact is there is serious leakage across the teeth in this machine, but the coils 1, 2, 3, etc., have a thickness of ·2 mms. and are covered with one layer of silk, so that they account for a portion of the air space. Coming now to the characteristic curve. In Fig. 6 the length of the air space (l_1) measured radially lies between ·15 and ·2 cms.; the area of the air space A_1 is taken to be 210 sq. cms.; l_2 is taken to be 7·5 cms., and is the length

of the teeth on armature and stationary portion ; A_2, the area of the teeth, is taken to be 105 sq. cms. ; l_3 is taken to be 24 cms. and is the mean length in the iron ; A_3, the area of the iron, is taken to be 200 sq. cms. Then the equation to the characteristic curve is $\frac{4\pi m_1 x_1}{10 \times 3} = 2 \left\{ \frac{l_1 I}{A_1} + l_2 \frac{I}{\mu_2 A_2} + l_3 \frac{I\nu}{\mu_3 A_3} \right\}$ where $m_1 = 72$ and is the number of conductor turns ; x_1 is the current in amperes ; I is the total induction over area A_1, that is area $j\,h$ (Fig. 3) of the armature, ν is the leakage coefficient which is taken to be 1·5. No great refinement can possibly be attempted in the results of characteristic curve calculations, since the air space length l_1 is not known very accurately and is of such importance. Taking an ordinary induction curve for transformer plate, and $l_1 = \cdot 175$ cms., the characteristic curve for $x_1 = 14{\cdot}4$ would give I the value $\cdot 215 \times 10^6$; for $x_1 = 25$, I would have the value $\cdot 390 \times 10^6$. We note that the maximum inductions given by coils (2–3) + (5–6) are ·200 and ·380 in 10^6 C.G.S. units respectively. This is sufficient to show that if the air space length were known accurately, the result might be predicted with sufficient accuracy for practical purposes.

Before going further, it might be useful to examine the conditions of a pure rotating magnetic field produced by two- or three-phase currents. Let a coil be wound so that the induction for current x may be $x\,(b_1 \cos 2\pi\theta + b_3 \cos 6\pi\theta + \ldots\ldots)$, and let a series of such coils be wound, m to the period of θ, with equal currents but later in period. In Fig 3, θ would be represented by the surface between o and 8.

$$x = a_1 \cos \frac{2\pi t}{T} + a_3 \cos \frac{6\pi t}{T} + \ldots\ldots$$

$$\text{Next coil } x = a_1 \cos \frac{2\pi t}{T}\left(t - \frac{T}{m}\right) + \ldots\ldots$$

$$\text{Induction} = \left(a_1 \cos \frac{2\pi t}{T} + \ldots\ldots\right)\left(b_1 \cos 2\pi\theta + \ldots\ldots\right)$$

$$+ \left\{ a_1 \cos \frac{2\pi}{T}\left(t - \frac{T}{m}\right) + \ldots\ldots \right\} \left\{ b_1 \cos 2\pi\left(\theta - \frac{1}{m}\right) + \ldots\ldots \right\}$$

Work out a simple case first two-phase.

Induction in coil $= x \cos 2\pi\theta$

In next coil $= x \cos 2\pi\left(\theta - \frac{1}{4}\right)$

Currents are $a \cos 2\pi \frac{t}{T}$ and $a \cos 2\pi \left(\frac{t}{T} - \frac{1}{4}\right)$

$$\text{Induction is} = a \left\{ \cos 2\pi\theta \cos \frac{2\pi t}{T} + \cos 2\pi \left(\theta - \frac{1}{4}\right) \cos 2\pi \left(\frac{t}{T} - \frac{1}{4}\right) \right\}$$

$$= a \left\{ \cos 2\pi\theta \cos \frac{2\pi t}{T} + \sin 2\pi\theta \sin 2\pi \frac{t}{T} \right\}$$

$$= a \cos 2\pi \left(\theta - \frac{t}{T}\right)$$

Taking the simple case of three-phase currents we have—

Induction in coil $= x \cos 2\pi\theta$

In next coil $= x \cos 2\pi \left(\theta - \frac{1}{6}\right)$;

In next $= x \cos 2\pi \left(\theta - \frac{1}{3}\right)$

Currents are $a \cos 2\pi \frac{t}{T}$ and $a \cos 2\pi \left(\frac{t}{T} - \frac{1}{6}\right)$ and $a \cos 2\pi \left(\frac{t}{T} - \frac{1}{3}\right)$

$$\text{Induction} = a \left\{ \cos 2\pi \frac{t}{T} \cos 2\pi\theta + \cos 2\pi \left(\frac{t}{T} - \frac{1}{6}\right) \cos 2\pi \left(\theta - \frac{1}{6}\right) + \cos 2\pi \left(\frac{t}{T} - \frac{1}{3}\right) \cos 2\pi \left(\theta - \frac{1}{3}\right) \right\}$$

$$= \frac{3}{2} a \cos 2\pi \left(\frac{t}{T} - \theta\right)$$

In this manner any number of phases may be treated.

Now consider a two-phase with the higher periodic terms—

Induction $= x\,(a_1 \cos 2\pi\theta + a_3 \cos 6\pi\theta + \ldots\ldots)$

In next $= x \left\{ a_1 \cos 2\pi \left(\theta - \frac{1}{4}\right) + a_3 \cos 6\pi \left(\theta - \frac{1}{4}\right) + \ldots\ldots \right.$

In first coil $x = b_1 \cos 2\pi \frac{t}{T} + b_3 \cos 6\pi \frac{t}{T} + \ldots\ldots$

In next coil $x = b_1 \cos 2\pi \left(\frac{t}{T} - \frac{1}{4}\right) + b_3 \cos 6\pi \left(\frac{t}{T} - \frac{1}{4}\right) + \ldots\ldots$

$$\begin{aligned}\text{Induction } &= \left(b_1 \cos 2\pi \frac{t}{T} + b_3 \cos 6\pi \frac{t}{T} + \ldots.\right)\\ &\quad (a_1 \cos 2\pi\theta + a_3 \cos 6\pi\theta + \ldots.)\\ &+ \left(b_1 \sin 2\pi \frac{t}{T} - b_3 \sin 6\pi \frac{t}{T} + \ldots.\right)\\ &\quad (a_1 \sin 2\pi\theta - a_3 \sin 6\pi\theta + \ldots.)\\ &= a_1\, b_1 \cos 2\pi \left(\frac{t}{T} - \theta\right) + a_3\, b_3 \cos 6\pi \left(\frac{t}{T} - \theta\right)\\ &\qquad + \ldots.\\ &+ a_3\, b_1 \cos 2\pi \left(\frac{t}{T} + 3\,\theta\right) + a_1\, b_3 \cos 2\pi \left(3\frac{t}{T} + \theta\right)\\ &\qquad + \ldots.\end{aligned}$$

If any one of a_3, b_3, etc., be not zero, the result will give terms travelling round with different speeds. It is essential to have a simple harmonic current and a simple harmonic field. In this motor a nearer approximation to a pure rotating field might be attained by allowing the coils A and B to overlap. It is evident that, with magnetising coils placed as they are in this motor, a pure rotating field cannot be produced with two-phase currents.

We now come to the two-phase current experiments. Each of the coils A B was supplied with alternate currents differing in phase by a quarter period. The results are given in Tables III. and IV. Table III. refers to the motor when its armature is at rest on open circuit. The frequency is 53·5 and $\sqrt{\text{mean}^2}$ E_1 and E_2 65 volts. Table IV. refers to the motor when its armature is at rest with an extra resistance in each circuit of 0·15 ohm. The frequency is 55 and $\sqrt{\text{mean}^2}$ E_1 and E_2 60 volts. The curves have been integrated, and Fig. 7 shows the important result. The abscissæ represents time in 60ths of a period, the ordinates total induction respecting the surface 2, 3, 4, 5, and 6 in Fig. 3.

Referring to Fig. 3, in which the areas or total Inductions due to the two phases are sine functions of *time* and displaced nearly 90°, we see that if we combine the two-phase curves Nos. I. and II. at different points along $l\ l$, the result is the curve No. III. The curves Nos. I. and II. have areas per half period ($\theta/_2$) which, when added together, equal $\sqrt{2}$ of the maximum area of either curve, and the resultant therefore represents the maximum induction density experienced, which occurs at positions 1, 3, 5, 7, etc. The

induction density over other portions of the surface will be less ; curve No. IV. has maximum area and shows maximum density at 2, 4, 6, 8, etc. In Fig. 7 the curve No. I. is the curve $\int \frac{dI_2}{dt} dt$ in Table III. ; curve No. 2 is the sum of the inductions given by coils (2–3), (3–4), (4–5), (5–6), Table III. when the motor armature is on open circuit. When loaded by inserting 0·15 ohm in each armature circuit, the curves Nos. 3 and 4, Fig. 7, represent the same combinations. We see that the curves Nos. 1 and 3 have the same amplitude and are nearly in phase. This will be seen to have importance in a subsequent part of this paper.

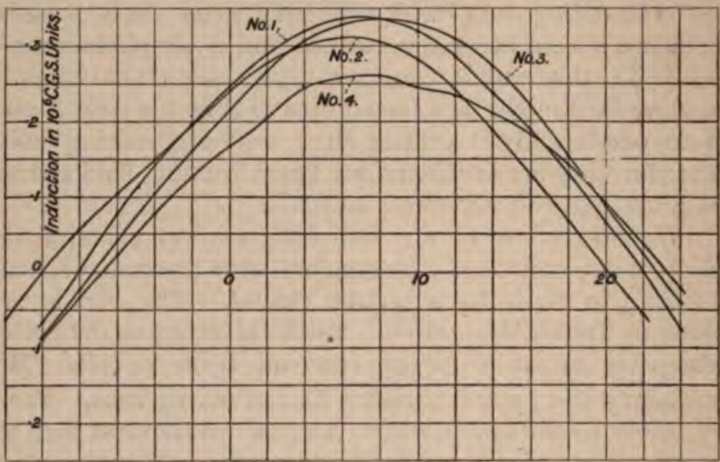


FIG. 7.

Taking the figures as given by the curves in Fig. 7 at the maximum value, and correcting Nos. 1 and 3 for 0·83, the differences between Nos. 1 and 2 on open circuit and Nos. 3 and 4 on closed circuit amount to 23 and 35 per cent. of the greater ordinate in each case. Table III. shows that on open circuit the maxima for the coils (2–3), (3–4), (4–5), (5–6) are 0·130, 0·123, 0·135, and 0·096 in 10^6 C.G.S. units. Table IV. shows that when loaded these maxima are 0·107, 0·102, 0·146, and 0·043 ; coil (5–6) suffering the greatest diminution. On open circuit the phase differences with regard to maximum induction are 48° for coils (3–4), (4–5), and 30° for coils (2–3), (5–6) as against 45° with a pure rotating field. On load these phase-differences are 72° and 72°.

Deal now with the armature of this motor. The armature being brought to the same position in each experiment by aid of the pulley-block, and the pull in pounds observed on the spring balance, observations were made for different positions of the phase of the primary potential differences E_1 E_2 and currents x_1 x_2; the potential differences between the ends of each of the armature circuits C, D, E (see Fig. 6), and the currents y_1, y_2, y_3, in each armature circuit. The relative position of armature and field in each experiment is as shown in Fig. 6. The electromotive force of each armature circuit, e_1 e_2 e_3, is given by correcting its potential difference for current into resistance of the armature coil. In this investigation, r_1 r_2 r_3 are respectively the resistances of the armature coils and are each equal to ·025 ohm; ρ_1 ρ_2 ρ_3 are the external resistances, in the armature circuits r_1 r_2 r_3, and constitute the independent variable. In any one experiment $\rho_1=\rho_2=\rho_3$. By correction for R_1 R_2 (the resistance of the primary circuit) into the instantaneous value of the current, the reactions $m_1\frac{dI_1}{dt}$ $m_2\frac{dI_2}{dt}$ have been found, and therefrom $\frac{dI_1}{dt}$ $\frac{dI_2}{dt}$. An integration of the E.M.F. curves $\frac{dI_1}{dt}$ $\frac{dI_2}{dt}$ $\frac{e_1}{n_1}$ $\frac{e_2}{n_2}$ $\frac{e_3}{n_3}$ gives the average total induction in the coils where $n_1=n_2=n_3=24$. A small correction must also be applied in the case of the armature coils, since there are three groups of 24 each per half period. If the distribution per half period of $(\theta/_2)$ along the surface circumferentially were a sine curve, the average total induction per coil would equal 0·93 of the maximum. In our case this coefficient is ·93 nearly. The integrals of $\frac{e_1}{n_1}$ $\frac{e_2}{n_2}$ $\frac{e_3}{n_3}$ have been increased respectively by multiplying by $\frac{1}{\cdot 93}$ in order to deduce the maximum average induction experienced by each coil. Four distinct cases have been dealt with at about the same frequency of 52 periods per second and with $\sqrt{\text{mean}^2}$ E_1 and E_2 equal to 60 volts. These correspond to values of $\rho_1=\rho_2=\rho_3$ equal to ∞ 1·03, 0·133 and 0·012 ohms. The results are given respectively in Tables V., VI., VII., and VIII. Table IX. gives the collected results.

Dealing with Table V. first, in which the armature is on

open circuit, the relation between the curves $\frac{d\,I_1}{dt}\,\frac{d\,I_2}{dt}$ and the E.M.F. curves $e_1\ e_2\ e_3$ may be found. Each of the $m_1\int\frac{dI_1}{dt}dt$ and $m_2\int\frac{d\,I_2}{dt}\,dt$ curves shows a maximum of $27{\cdot}2 \times 10^6$ C.G.S. units; the coils on the armature experience each a total change of $7{\cdot}2 \times 10^6$ C.G.S. units. We have therefore $\frac{27{\cdot}2}{75}$ as the total average maximum per turn due to primary, and $\frac{7{\cdot}2}{24}$ as the total average maximum experienced per turn by secondary. Multiplying the former by $\frac{1}{{\cdot}83}$ and the latter by $\frac{1}{{\cdot}93}$, we have 0·437 and 0·322. The difference gives 0·115, which is 26·3 per cent. of 0·437. These results are given in Table IX., and also the corresponding ones for the other experiments, in which $\rho_1 = \rho_2 = \rho_3 = 1{\cdot}03$, 0·15 and 0·012 ohms. At 95 periods per second the leakage is greater (see Table XIV.) but the field intensity is less.

Torque.

Let f = frequency of supply; N = number of poles on inner circumference of stationary portion; there are two poles per circuit A and B to the period θ; r = radius of armature in cms. Then $\frac{2\pi r}{N}$ = length of circumference per half period in cms.; and $\frac{4\pi rf}{N}$ = velocity of field in cms. per second. Let l = length of armature core in cms. Let e_1, e_2, e_3 = the electromotive forces in volts of each circuit on the armature, and n_1, n_2, n_3 = the turns per circuit. Then $\frac{e_1}{n_1 l}$ = average volts per cm. length of conductor. Let y_1, y_2, y_3 = the current in amperes in the armature circuits. Without analysing the variation of B, the induction per square cm. at the conductor, we may say that during time $\frac{N}{4\pi rf}$ the average $\frac{e_1}{n_1 l}$ = av. B $\times\ \frac{4\pi rf\,10^{-8}}{N}$. The force on the armature at radius r due to current y_1 in the con-

ductor is $\frac{B y_1}{10}$ dynes, if y_1 be in amperes; so that the whole force on the armature at this radius due to one circuit $= \frac{B n_1 l y_1}{10}$, and taking B to be the average given by $\frac{e_1}{n_1 l} \times \frac{N}{4 \pi r f 10^{-8}}$ we get the whole force $= \frac{e_1 y_1 N}{4 \pi r f 10^{-7}}$. At the same epoch the forces due to the other circuits will be $\frac{e_2 y_2 N}{4 \pi r f 10^{-7}}$ and $\frac{e_3 y_3 N}{4 \pi r f 10^{-7}}$ in this three-phase armature. The average of the total force $\frac{N \times 10^7}{4 \pi r f} \left\{ e_1 y_1 + e_2 y_2 + e_3 y_3 \right\}$ over a half period, will be the average force acting at radius r upon the armature. In the ideal motor this force would have no variation. We must divide by 981×454 to reduce to pounds.

It will be well to see if this verifies by actual experiment. The torque has been measured for three values of $\rho_1 = \rho_2 = \rho_3$, the external resistance in the armature of this motor, namely 1·03, 0·15, and 0·012 ohm, and found to be about 2, 7, and 2 lbs. respectively at a radius of 3·5 feet. Tables VI., VII. and VIII. give the results of the experiments in each case, and Table IX. shows the collected results, from which we see that the calculated values are 1·5, 6·72, and 2·2 lbs. at a radius of 3·5 feet. The agreement is as good as one could expect from the rough way in which the torque was measured, owing to the friction of brushes and bearings rendering an exact measurement difficult.

It is interesting to examine the effect of waste field upon the distribution of power in this motor over a half period in time. The sum of $m_1 \int x_1 \frac{dI_1}{dt} dt$ and $m_2 \int x_2 \frac{dI_2}{dt} dt$ over half a period divided by $2f$ is the energy delivered to the motor by the primary circuit irrespective of $x_1^2 R_1$ and $x_2^2 R_2$ losses, since the potential differences E_1, E_2, have been corrected for $x_1 R_1$ and $x_2 R_2$. Similarly $\int e_1 y_1 dt + \int e_2 y_2 dt + \int e_3 y_3 dt$ divided by $2f$ is the energy received by the secondary or armature circuits. The difference gives the dissipation of energy due to magnetic hysteresis and induced currents in the iron (and copper if there be any)

and that due to waste field. When the machine has open-circuited armature, the average rate of dissipation of energy in watts is 176 (see Table V.). Take Table VI., in which the external resistances $\rho_1 = \rho_2 = \rho_3 = 1.03$ ohms. The average watts due to waste field and iron losses = 1301. Assuming that 176 watts represents the rate of dissipation of energy in the iron, we have 1125 watts due to waste field. Take Table VII., in which the external resistances $\rho_1 = \rho_2 = \rho_3 = 0{\cdot}15$ ohm. The average rate of dissipation of energy is equal to 216 watts, leaving very little for waste field. In this experiment the magnitudes are great, and the difference we are considering is small. A small error in the value of either magnitude will affect this difference seriously, and it has not been measured as a direct quantity. We may say that the dissipation of energy by waste field in this experiment is comparatively small if we assume that 176 watts are dissipated in the iron. Coming now to the last Table VIII., we see that iron losses and waste field account for 905 watts, leaving 729 for waste field, if we assume 176 for the iron losses. The variation of torque over a half period is shown by the figures in Tables VI., VII. and VIII.; in Table VI. the maximum and minimum values deviate from the mean by 23·3 and 26·3 per cent. In Table VII., these percentage figures are 9·5 and 11·4; and in Table VIII. they are 58·8 and 43. With a pure rotating field the torque should be constant.

Table IX. shows collected results for each of the four experiments described. The phase differences have been taken from the curves after plotting on squared paper. We notice, firstly, as a numerical quantity the leakage as between the maximum average values of the primary and secondary per half period amounts to 26·3, 23·9, 28·7 and 76·5 per cent. in the four cases. With regard to phase differences we notice that with 0·15 ohm in each armature circuit the difference between $m_1 \frac{dI_1}{dt}$ and x_1 as also $m_2 \frac{dI_2}{dt}$ and x_2 has its minimum value of 47°·4 (360° = 1 period). Taking the phase with ∞ in the armature as an origin, we see that $m_1 \frac{dI_1}{dt}$ and $m_2 \frac{dI_2}{dt}$ experience little change; but e_1 e_2 e_3 are much altered, the signs however are not altered. The phase of x_1 and x_2 has maximum diminution with 0·15 ohm; the

phase of $y_1\ y_2\ y_3$ are much increased between ∞ and 0·15 ohm, but have not such a great increase between 0·15 and 0·012 ohm. The phase difference between x_1 and $y_1\ y_2\ y_3$ has a maximum value for 0·15 ohm.

As is well known, the conclusion is that unlike the direct current motor an induction motor gives maximum starting torque for a particular external resistance in its armature, below or above which the torque decreases. The power factor is a maximum, since phase difference between applied potential difference and current is a minimum with maximum torque.

In applying an induction motor for the propulsion of a tramcar, for example, the controller should be so arranged that at starting the driver cannot exceed the position for maximum torque. With direct currents it happens that a careless driver puts over the controller too far at starting, with the result that the car jumps forward. With an induction motor such use of the controller would result in an abnormal current from the line, but practically no starting torque.

To give a complete account of this motor it would be necessary to observe curves for variation of frequency and potential differences, E_1 E_2, and position of armature with respect to field. The preceding curves refer to a frequency 52 and $\sqrt{\text{mean}^2}$ E_1 and $E_2 = 60$ volts. A similar set of curves have been obtained at frequency 95 and $\sqrt{\text{mean}^2} =$ 80 volts, the position of armature with respect to field being unaltered, and as shown in Fig. 6. The results of these tests are given in Tables X., XI., XII., and XIII. The collected results are given in Table XIV. It will be seen that the same character of result is obtained, namely, that as the independent variables $\rho_1 = \rho_2 = \rho_3$ are given values between ∞ and 0, the torque rises to a maximum at about $\rho_1 = \rho_2 = \rho_3 = {}$·15 ohm, and then diminishes. These tests, as well as those at frequency 52, will be specially referred to in the next section on Armature Reaction.

Table XVIII. gives a few other tests at other frequencies for which no curves have been taken.

Armature Reaction.

Before dealing with the armature or secondary circuit of this motor, it will be well to examine the disturbance

produced in the primary induction, as the external resistance ρ is varied from ∞ to zero. The induction turns $m_1\int\frac{dI_1}{dt}dt$, $m_2\int\frac{dI_2}{dt}dt$ give the average over a half period $\theta/_2$, and are plotted in Figs. 8 and 9 respectively for values of $\rho=\infty$, 1·07, ·149 and ·0095, the frequency being 95 periods per

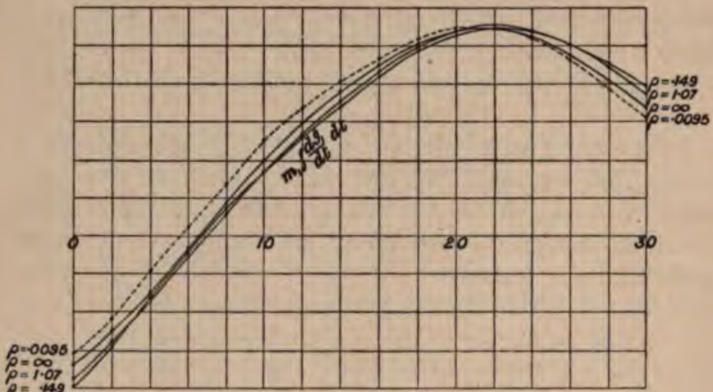


FIG. 8.

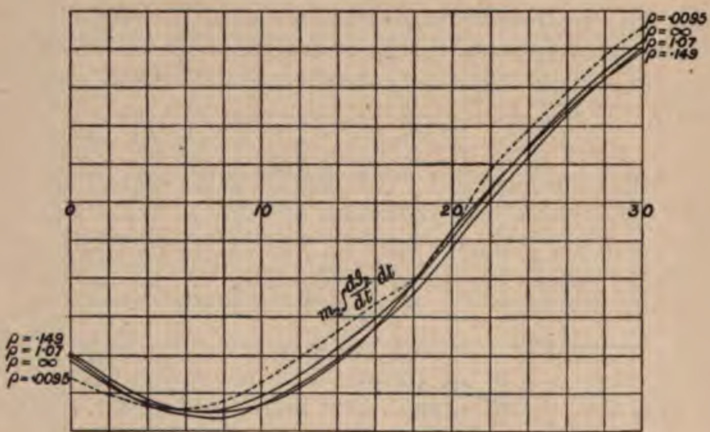


FIG. 9.

second. It will be seen that the disturbance is not great; it is greatest when $\rho=\cdot0095$. The variation of this average induction for $\rho=\cdot15$ at 52 periods per second, can be seen by comparing Table IV. with III.

We now come to the armature. If the leakage is known, and we have examined it in this motor, it is not difficult

to deduce the $\int e_1 dt$, $\int e_2 dt$ and $\int e_3 dt$ curves when $\rho = \infty$, since there is no disturbance due to armature currents. We have to investigate the relations between these curves for $\rho = \infty$, and when ρ has diminishing values given to it down to zero.

If the ordinary self-induction theory be assumed, and if the coils on the armature were simply closed on themselves and not pooled together as in this three-phase arrangement, the relation could be expressed in the form of the equations—

$$e_1 \text{ (for } \rho = \infty) = (r_1 + \rho_1)\, y_1 + L\dot{y}_1$$
$$e_2 \text{ (for } \rho = \infty) = (r_2 + \rho_2)\, y_2 + L\dot{y}_2$$
$$e_3 \text{ (for } \rho = \infty) = (r_3 + \rho_3)\, y_3 + L\dot{y}_3$$

where L is a coefficient of self-induction.

Integrating the above from time 0 to time t and equating periodic parts, we have—

$$\int_0^t e_1 \text{ (for } \rho = \infty)\, dt = \int_0^t (r_1 + \rho_1)\, y_1\, dt + \text{L}\, y_{1t}$$
$$\int_0^t e_2 \text{ (for } \rho = \infty)\, dt = \int_0^t (r_2 + \rho_2)\, y_2\, dt + \text{L}\, y_{2t}$$
$$\int_0^t e_3 \text{ (for } \rho = \infty)\, dt = \int_0^t (r_3 + \rho_3)\, y_3\, dt + \text{L}\, y_{3t}$$

If the epochs be chosen when $y_1\ y_2\ y_3$ are zero, the last term in each equation vanishes.

In our case the electromotive force of an armature coil is not $(r+\rho)\ y$ at any moment owing to the influence of the respective coils on one another. The extent of this disturbance can be judged by an examination of the phase difference between e_1 & y_1, e_2 & y_2, and e_3 & y_3 in each case (see Tables IX. and XIV.). But we may write—

$$e_1 \text{ (for } \rho = \infty) = e_1 \text{ (for } \rho = a) + \text{L}\,\dot{y}_1$$
$$e_1 \text{ (for } \rho = \infty) = e_1 \text{ (for } \rho = b) + \text{L}\,\dot{y}_1$$

where $a\ b$ . . . . . are the values of ρ in the actual experiments. In these cases also when $y_1 = 0$ we have—

$$\int_0^t e_1 \text{ (for } \rho = \infty)\, dt = \int_0^t e_1 \text{ (for } \rho = a)\, dt$$
$$\int_0^t e_1 \text{ (for } \rho = \infty)\, dt = \int_0^t e_1 \text{ (for } \rho = b)\, dt$$

and so on for e_2 and e_3.

Now the curves e_1, e_2, e_3 for $\rho = \infty$, a, b, ..., obtained at frequencies 52 and 95 (see Tables V., VI., VII., VIII., X., XI., XII., XIII.), have been integrated, and the difference between the ordinates (when $y_1 = 0$) of $\int e_1$ (for $\rho = \infty$) dt and $\int e_1$ (for $\rho = a, b, \ldots$) dt, which on the theory should be zero, is *not* zero. It appears that as ρ is varied the $\int e_1$ (for $\rho = \infty$) dt curve must be increased to cut the other when $y_1 = 0$, up to the value of ρ corresponding to maximum torque, but that after this with $\rho = \cdot 01$ ohm, the $\int e_1$ (for $\rho = \infty$) dt curve has to be decreased to cut the other curve when $y = 0$: and so for e_2, e_3. The extent to which

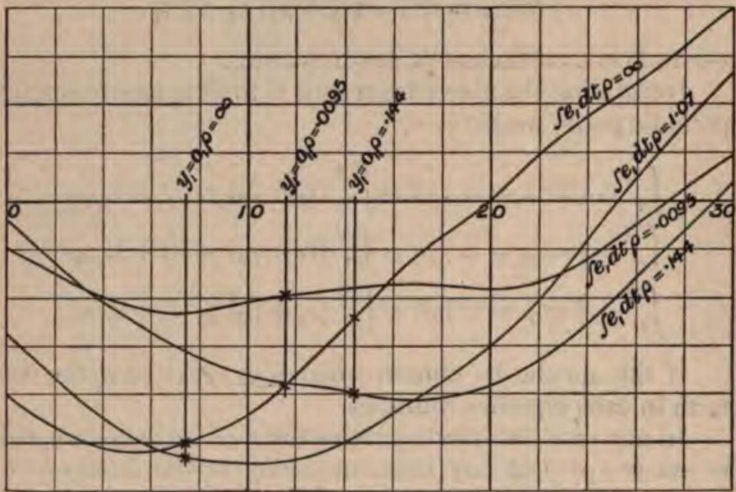


FIG. 10.

the ordinates have to be altered is given in Table XV. for the frequencies 52 and 95.

If the ordinates of the curve $\int e_1$ (for $\rho = \infty$) dt be increased or decreased according to the difference found in this way (when $y_1 = 0$), for the different positions of the phase, then the relation between such modified curve and $\int e_1$ (for $\rho = a, b \ldots$) dt is to be accounted for by the term L y_1 and so for e_2 and e_3. This has been done for different positions of a half period and the value of L deduced. The collected results are given in Table XVI. Fig. 10 gives one set of curves, and Table XVII. shows how L has been deduced. The values of L are not constant, but as ρ is decreased we see that L becomes somewhat smaller. This

may be connected with the disturbances shown in Figs. 8 and 9. The maximum value of L is $1{\cdot}8 \times 10^{6}$ C.G.S. units according to the experiments. We can calculate the value of L approximately from the magnetic circuit of this motor. The area enclosed by one armature coil is measured and found to be 280 sq. cms. The length of the air space can be taken to be 0·175 cms. Then the induction due to unit current-turn (C.G.S.) is approximately $4\pi \dfrac{280}{\cdot 175} =$ 20,110 C.G.S. units.

The reaction will be that due to the rate of change of this flux multiplied by $6\left(\dfrac{n}{6}\right)^2$, where $\dfrac{n}{6}$ is the number of turns per coil, there being six coils in series. In this armature $n = 24$, so that $L = 6 \times 16 \times 20{,}110 = 1{\cdot}9 \times 10^6$ C.G.S. units as against a maximum of $1{\cdot}8 \times 10^6$ found in the experiments.

This, then, gives an account of the armature reaction in this motor. Its effect is to increase and then decrease electromotive force as the external resistance ρ is varied as shown in the experiments.

An examination of the results in Tables IX. and XIV. shows that as ρ is decreased, the phase difference between e_1, e_2, e_3 (for $\rho = \infty$) and the currents y_1, y_2, y_3 increases up to the point of maximum torque; after this the effect at 52 periods per second is to slightly increase phase difference, but at 95 periods per second phase difference is decreased. The currents are not displaced a quarter period, in which case their demagnetising effect for a given current would be greatest. One has only to examine Tables VIII.[1] and XIII. to see how violent the reactions are when ρ has the smallest value. The torque fluctuates in any coil between positive and negative values; the effect is a differential one: on the whole there is a small positive torque. If we consider $r + \rho$ the independent variable, and the torque the dependent variable, we have for large values of $r + \rho$ the currents y practically in phase with impressed electromotive force e (for $\rho = \infty$): the torque is small because y is small. As $r + \rho$

[1] The experiments in this Table were very difficult to perform owing to the rapid heating of the alternators—the load was so excessive. It will be seen that the instantaneous value of $y_1 + y_2 + y_3$ is not always zero. However, the deductions are amply borne out by Table XIII., which is well up to the average accuracy of these experiments.

decreases the angle of lag of y increases, it is true, but y also increases, and we have seen that the electromotive force e (for $\rho = \infty$) is also increased by armature reaction as distinct from self-induction pure and simple; the torque increases. This goes on until the torque has a maximum value. After this, on further reduction of $r + \rho$ until $\rho = \cdot 01$ ohm, although phase difference is not greatly altered (it may even be decreased) the current y is increased; armature reaction diminishes the electromotive force very greatly, with the result that the torque decreases.

During these experiments I have received help from Messrs. Greenbank, Davey, Renfree, and Poole, past and present Student Demonstrators in the Siemens Laboratory, King's College, London. To these gentlemen I tender my thanks. I wish specially to acknowledge the assistance I have received from Mr. Renfree in the working out of results.

The author is continuing these experiments with the motor in motion, and hopes, at a later date, to be able to publish the results of his observations.

TABLE I.

Angle in 60ths of a period.	$\frac{dI_1}{dt}$ Volts.	x_1 Amperes.	Coil 2–3. Volts.	Coil 3–4. Volts.	Coil 4–5. Volts.	Coil 5–6. Volts.
0	+ ·774	+ 10·89	+ ·246	+ ·188	+ ·130	+ .217
3	+ ·556	+ 13·29	+ ·159	+ ·116	+ ·087	+ ·159
6	+ ·108	+ 14·43	+ ·029	+ ·029	+ ·0	+ ·043
9	+ ·326	+ 12·98	− ·130	− ·072	− ·087	− ·087
12	+ ·829	+ 9·765	− ·232	− ·145	− ·159	− ·188
15	− ·864	+ 5·859	− ·290	− ·174	− ·174	− ·232
18	− 1·049	+ 2·142	− ·333	− ·217	− ·203	− ·275
21	− 1·286	− 1·701	− ·304	− ·203	− ·188	− ·246
24	− 1·233	− 4·663	− ·304	− ·217	− ·188	− ·246
27	− ·949	− 8·44	− ·304	− ·203	− ·188	− ·232

TABLE II.

Angle in 60ths of a period.	$\frac{dI_1}{dt}$ Volts.	x_1 Amperes.	Coil 2–3. Volts.	Coil 3–4. Volts.	Coil 4–5. Volts.	Coil 5–6. Volts.
0	+ 1·51	+ 18·78	+ ·522	+ ·290	+ ·246	+ ·420
3	+ 1·05	+ 23·05	+ ·377	+ ·203	+ ·174	+ ·304
6	+ ·25	+ 25·04	+ ·087	+ ·014	+ ·029	+ ·072
9	− ·59	+ 22·75	− ·188	− ·145	− ·145	− ·159
12	− 1·28	+ 17·41	− ·420	− ·304	− ·275	− ·348
15	− 1·63	+ 11·45	− ·536	− ·362	− ·333	− ·448
18	− 2·01	+ 5·194	− ·652	− ·420	− ·377	− ·507
21	− 2·44	− 1·374	− ·623	− ·406	− ·348	− ·507
24	− 2·34	− 7·788	− ·695	− ·449	− ·406	− ·550
27	− 1·88	− 13·59	− ·608	− ·391	− ·348	− ·478

TABLE III.

Angle in 60ths of a period.	x_1 in amperes.	$\frac{dI_1}{dt}$ in volts.	$\int\frac{dI_1}{dt}dt$ in 10^6 C. G. S. units.	x_2 in amperes.	$\frac{dI_2}{dt}$ in volts.	$\int\frac{dI_2}{dt}dt$ in 10^6 C. G. S. units.	Coil 2-3. Volts.	Coil 2-3. Induction in 10^6 C. G. S. units.	Coil 3-4. Volts.	Coil 3-4. Induction in 10^6 C. G. S. units.	Coil 4-5. Volts.	Coil 4-5. Induction in 10^6 C. G. S. units.	Coil 5-6. Volts.	Coil 5-6. Induction in 10^6 C. G. S. units.	Total induction over surface 23456 in 10^6 C. G. S. units.	Total induction over surface 34567 in 10^6 C. G. S. units.
0	+ 6·62	− ·835	− ·245	− 9·45	− ·742	− ·247	− ·217	− ·105	+ ·0724	− ·120	+ ·449	− ·054	+ ·362	+ ·0411	− ·239	− ·028
2	..	..	− ·195	..	..	− ·289	..	− ·09	..	− ·123	..	− ·081	..	+ ·0174	− ·277	− ·0972
3	+ 3·21	− ·997	..	− 11·7	− ·510	..	− ·319	..	− ·029	..	+ ·377	..	+ ·377	..	..	..
4	..	..	− ·131	..	..	− ·320	..	− ·07	..	− ·120	..	− ·105	..	− ·0062	− ·302	− ·162
6	− 1·39	− 1·26	− ·057	− 12·3	− ·122	− ·336	− ·362	− ·048	− ·159	− ·113	+ ·232	− ·123	+ ·333	− ·028	− ·312	− ·216
8	..	..	+ ·024	..	..	− ·335	..	− ·025	..	− ·101	..	− ·133	..	− ·0467	− ·306	− ·256
9	− 4·35	− 1·27	..	− 11·4	+ ·256	..	− ·377	..	− ·304	..	+ ·029	..	+ ·232	..	..	..
10	..	..	+ ·103	..	..	− ·319	..	− ·0012	..	− ·082	..	− ·135	..	− ·0617	− ·280	− ·278
12	− 8·19	− ·983	+ ·172	− 9·14	+ ·599	− ·275	− ·435	+ ·025	− ·435	− ·058	− ·0869	− ·132	+ ·188	− ·0741	− ·239	− ·289
14	..	..	+ ·229	..	..	− ·234	..	+ ·052	..	− ·030	..	− ·125	..	− ·0841	− ·187	− ·290
15	− 10·8	− ·817	..	− 6·11	+ ·756	..	− ·406	..	− ·464	..	− ·188	..	+ ·116	..	..	..
16	..	..	+ ·280	..	..	− ·187	..	+ ·076	..	− ·0012	..	− ·113	..	− ·091	− ·129	− ·282
18	− 13·1	− ·589	+ ·323	− 2·27	+ ·958	− ·131	− ·333	+ ·099	− ·449	− ·027	− ·275	− ·098	+ ·043	− ·0955	− ·0673	− ·265
20	..	..	+ ·353	..	..	− ·066	..	+ ·117	..	+ ·054	..	− ·080	..	− ·096	− ·0044	− ·239
21	− 13·8	− ·218	..	+ 1·13	+ 1·15	..	− ·174	..	− ·377	..	− ·377	..	− ·072	..	..	..
22	..	..	+ ·366	..	..	+ ·006	..	+ ·128	..	+ ·078	..	− ·057	..	− ·0916	+ ·0573	− ·198
24	− 12·8	+ ·254	+ ·357	+ 4·03	+ 1·11	+ ·077	+ ·014	+ ·130	− ·217	+ ·095	− ·391	− ·032	− ·203	− ·0854	+ ·107	− ·153
26	..	..	+ ·334	..	..	+ ·142	..	+ ·126	..	+ ·106	..	− ·0062	..	− ·076	+ ·150	− ·102
27	− 9·95	+ ·625	..	+ 7·44	+ ·878	..	+ ·145	..	− ·145	..	− ·493	..	− ·319	..	..	..
28	..	..	+ ·295	..	..	+ ·197	..	+ ·117	..	+ ·115	..	+ ·024	..	− ·0623	+ ·194	− ·0399

TABLE IV.

Angle in 60ths of a period.	x_1 in amperes.	$\frac{dI_1}{dt}$ in volts.	$\int\frac{dI_1}{dt}dt$ in 10^6 C. G. S. units.	x_2 in amperes.	$\frac{dI_2}{dt}$ in volts.	$\int\frac{dI_2}{dt}dt$ in 10^6 C. G. S. units.	Coil 2–3.		Coil 3–4.		Coil 4–5.		Coil 5–6.		Total induction over surface 2 3 4 5 6 in 10^6 C. G. S. units.	Total induction over surface 3 4 5 6 7 in 10^6 C. G. S. units.
							Volts.	Induction in 10^6 C. G. S. units.	Volts.	Induction in 10^6 C. G. S. units.	Volts.	Induction in 10^6 C. G. S. units.	Volts.	Induction in 10^6 C. G. S. units.		
0	0·0	− ·919	− ·315	− 60·9	− ·878	− ·216	+ ·0287	− ·101	+ ·100	− ·0964	+ ·416	+ ·0097	+ ·115	+ ·0206	− ·167	+ ·0351
2	..	..	− ·257	..	..	− ·267	..	− ·107	..	− ·101	..	− ·0164	..	+ ·0133	− ·211	− ·003
3	− 10·6	− 1·06	..	− 60·9	− ·679	..	− ·0143	..	+ ·014	..	+ ·445	..	+ ·100	..	..	..
4	..	..	− ·193	..	..	− ·307	..	− ·102	..	− ·102	..	− ·0436	..	+ ·0073	− ·244	− ·04
6	− 31·8	− 1·23	− ·121	− 51·4	− ·227	− ·328	− ·244	− ·0933	− ·272	− ·0903	+ ·459	− ·0715	+ ·158	− ·0018	− ·257	− ·0703
8	..	..	− ·0448	..	..	− ·335	..	− ·077	..	− ·0776	..	− ·0982	..	− ·0103	− ·263	− ·109
9	− 49·5	− 1·26	..	− 37·0	+ ·091	..	− ·287	..	− ·344	..	+ ·387	..	+ ·115	..	..	..
10	..	..	+ ·0315	..	..	− ·328	..	− ·06	..	− ·0564	..	− ·121	..	− ·017	− ·254	− ·135
12	− 61·6	− 1·17	+ ·104	− 15·9	+ ·418	− ·310	− ·344	− ·04	− ·330	− ·0358	+ ·186	− ·137	+ ·100	− ·023	− ·236	− ·156
14	..	..	+ ·173	..	..	− ·275	..	− ·0188	..	− ·017	..	− ·145	..	− ·0297	− ·210	− ·173
15	− 66·2	− 1·07	..	− 0·38	+ ·914	..	− ·330	..	− ·272	..	+ ·0287	..	+ ·129	..	..	..
16	..	..	+ ·237	..	..	− ·219	..	+ ·0012	..	0	..	− ·146	..	− ·0376	− ·182	− ·185
18	− 65·4	− 0·879	+ ·296	+ 7·18	+ 1·14	− ·153	− ·344	+ ·0212	− ·358	+ ·0194	− ·129	− ·141	+ ·043	− ·0418	− ·142	− ·185
20	..	..	+ ·342	..	..	− ·0862	..	+ ·0424	..	+ ·0406	..	− ·131	..	− ·043	− ·0909	− ·176
21	− 56·0	− ·518	..	+ 30·6	+ 1·00	..	− ·358	..	− ·287	..	− ·244	..	0	..	..	..
22	..	..	+ ·373	..	..	− ·0242	..	+ ·0642	..	+ ·0576	..	− ·116	..	− ·0424	− ·0364	− ·165
24	− 39·7	− ·0611	+ ·387	+ 50·7	+ 1·06	+ ·040	− ·244	+ ·0824	− ·229	+ ·0727	− ·401	− ·0945	− ·057	− ·040	+ ·0206	− ·144
26	..	..	+ ·379	..	..	+ ·103	..	+ ·0945	..	+ ·0842	..	− ·0673	..	− ·0353	+ ·0764	− ·113
27	− 19·7	+ ·40	..	+ 60·5	+ 0·96	..	− ·086	..	− ·100	..	− ·501	..	− ·129	..	..	..
28	..	..	+ ·355	..	..	+ ·161	..	+ ·100	..	+ ·0903	..	− ·037	..	− ·0273	+ ·126	− ·074

TABLE V.

Angle in 60ths of a period.	A Circuit. $m_1\frac{dI_1}{dt}$ Volts.	A Circuit. x_1 Amps.	B Circuit. $m_2\frac{dI_2}{dt}$ Volts.	B Circuit. x_2 Amps.	$m_1\frac{dI_1}{dt}x_1 + m_2\frac{dI_2}{dt}x_2$ Watts.	e_1 Volts.	e_2 Volts.	e_3 Volts.
0	+ 61·4	− 6·68	+ 56·6	+ 9·76	+ 142	+ 14·1	− 24·4	+ 12·9
3	+ 73·9	− 3·08	+ 38·7	+ 11·8	+ 229	+ 8·45	− 24·2	+ 17·6
6	+ 91·6	+ 1·54	+ 9·7	+ 12·5	+ 262	− 0·73	− 18·7	+ 19·6
9	+ 88·9	+ 4·55	− 25·3	+ 11·3	+ 119	− 10·1	− 11·0	+ 20·9
12	+ 68·8	+ 8·22	− 50·2	+ 8·73	+ 127	− 17·4	− 7·9	+ 24·8
15	+ 54·5	+ 11·1	− 61·3	+ 5·72	+ 254	− 20·2	− 2·39	+ 22·6
18	+ 38·1	+ 13·2	− 76·5	+ 2·2	+ 335	− 22·2	+ 2·93	+ 18·7
21	+ 8·6	+ 13·9	− 93·2	− 1·39	+ 250	− 19·6	+ 10·1	+ 9·55
24	− 26·1	+ 12·7	− 87·4	− 4·25	+ 40	− 17·6	+ 17·4	− 0·918
27	− 50·5	+ 9·76	− 68·4	− 7·26	+ 1	− 17·3	+ 24·4	− 8·08
					1,759			

Frequency, 52·2. $\rho_1 = \rho_2 = \rho_3 = \infty$. $\sqrt{\text{mean}^2}$ E_1 & E_2 = 60 volts.

TABLE VI.

Angle in 60ths of a period.	A Circuit. $m_1\frac{dI_1}{dt}$ Volts.	A Circuit. x_1 Amps.	B Circuit. $m_2\frac{dI_2}{dt}$ Volts.	B Circuit. x_2 Amps.	$m_1\frac{dI_1}{dt}x_1 + m_2\frac{dI_2}{dt}x_2$ Watts.	e_1 Volts.	y_1 Amps.	e_2 Volts.	y_2 Amps.	e_3 Volts.	y_3 Amps.	$e_1y_1 + e_2y_2 + e_3y_3$ Watts	Dissipatio of energ due to waste fiel hysteresi and eddy currents Watts.
0	+ 57·8	− 5·0	− 59·0	− 41·2	+ 2141	+ 17·4	+ 17·3	− 24·2	+ 21·9	+ 3·33	− 4·22	− 845	+ 1296
3	+ 78·2	+ 3·6	− 42·2	− 41·4	+ 2032	+ 13·3	− 12·4	− 23·4	+ 18·7	+ 7·99	− 7·54	− 663	+ 1369
6	+ 81·3	+ 15·1	− 17·4	− 36·3	+ 1862	+ 6·2	− 4·83	− 18·8	+ 18·4	+ 14·75	− 14·0	− 582	+ 1280
9	+ 82·5	+ 33·3	+ 21·4	− 27·0	+ 2172	− 0·6	+ 3·47	− 17·8	+ 19·3	+ 26·9	− 23·2	− 970	+ 1202
12	+ 70·4	+ 41·6	+ 40·7	− 14·1	+ 2355	− 11·8	+ 11·8	− 14·0	+ 12·8	+ 26·23	− 25·0	− 974	+ 1381
15	+ 57·6	+ 44·6	+ 60·2	− 2·2	+ 2438	− 17·3	+ 16·3	− 8·0	+ 6·79	+ 24·0	− 22·9	− 886	+ 1552
18	+ 40·7	+ 46·6	+ 81·3	+ 4·2	+ 2241	− 17·0	+ 15·8	− 4·3	+ 3·47	+ 20·5	− 19·3	− 680	+ 1561
21	− 12·9	+ 42·2	+ 87·6	+ 13·9	+ 1764	− 22·0	+ 17·8	− 0·6	− 3·32	+ 12·6	− 15·2	− 582	+ 1182
24	− 13·6	+ 30·3	+ 82·9	+ 29·2	+ 2008	− 25·2	+ 22·5	+ 12·8	− 15·2	+ 7·0	− 7·7	− 816	+ 1192
27	− 42·0	+ 16·1	+ 69·0	+ 37·3	+ 1894	− 21·7	+ 20·4	+ 21·5	− 21·4	+ 0·41	+ 0·75	− 903	+ 997
					20,907							7,901	13,012

TABLE VII.

Angle in 60ths of a period.	A Circuit.		B Circuit.		$m_1\frac{dI_1}{dt}x_1 + m_2\frac{dI_2}{dt}x_2$	e_1	y_1	e_2	y_2	e_3	y_3	$e_1 y_1 + e_2 y_2 + e_3 y_3$	Dissipation of energy due to waste field, hysteresis and eddy currents.
	$m_1\frac{dI_1}{dt}$	x_1	$m_2\frac{dI_2}{dt}$	x_2									
	Volts.	Amps.	Volts.	Amps.	Watts.	Volts.	Amps.	Volts.	Amps.	Volts.	Amps.	Watts.	Watts.
0	+ 58·7	− 2·0	+ 56·9	+ 70·6	+ 3903	+21·6	−114	−14·1	+ 90·4	− 3·10	+ 24·9	+3839	+ 64
3	+ 69·3	+ 10·9	+ 41·7	+ 69·8	+ 3665	+18·8	−103	−16·4	+ 98·3	− 1·10	+ 7·35	−3558	+ 107
6	+ 71·9	+ 31·5	+ 16·0	+ 60·5	+ 3228	+13·3	− 77·4	−20·2	+110	+ 3·39	− 25·4	−3106	+ 122
9	+ 73·9	+ 54·9	− 10·7	+ 41·2	+ 3619	+ 6·8	− 40·1	−20·6	+114	+ 11·9	− 72·9	−3491	+ 128
12	+ 67·7	+ 67·8	− 51·2	+ 20·6	+ 3535	+ 3·54	+ 5·08	−14·1	+102	+ 22·5	−102	−3712	− 177
15	+ 57·4	+ 71·8	− 67·8	+ 3·2	+ 3903	− 5·83	+ 31·1	−15·0	+ 83·1	+ 19·2	−111	−3561	+342
18	+ 46·7	+ 71·0	− 73·0	− 8·47	+ 3938	−10·9	+ 48·0	−14·6	+ 68·9	+ 16·8	−112	−3413	+ 525
21	+ 18·7	+ 60·5	− 76·6	− 33·9	+ 3730	−20·3	+ 88·2	− 9·06	+ 20·9	+ 13·6	−106	−3419	+ 311
24	− 10·7	+ 44·4	− 82·0	− 52·1	+ 3795	−25·7	+107	− 3·16	− 21·5	+ 8·8	− 87·6	−3453	+ 342
27	− 25·5	+ 23·8	− 70·4	− 64·2	+ 3913	−23·6	+115	− 7·40	− 65·0	+ 6·11	− 53·7	−3519	+ 394
					37,229							35,071	2,335 177
													2,158

Frequency, 52·7. $\rho_1 = \rho_2 = \rho_3 = 0·15$ ohm. $\sqrt{\text{mean}^2}$ E_1 & E_2 = 60 Volts.

TABLE VIII.

Angle in 60ths of a period.	A Circuit.		B Circuit.		$m_1\frac{dI_1}{dt}x_1 + m_2\frac{dI_2}{dt}x_2$	e_1	y_1	e_2	y_2	e_3	y_3	$e_1 y_1 + e_2 y_2 + e_3 y_3$	Dissipation of energy due to waste field, hysteresis and eddy currents.
	$m_1\frac{dI_1}{dt}$	x_1	$m_2\frac{dI_2}{dt}$	x_2									
	Volts.	Amps.	Volts.	Amps.	Watts.	Volts.	Amps.	Volts.	Amps.	Volts.	Amps.	Watts.	Watts.
0	+ 51·7	− 15·12	− 31·9	−123·4	+ 3158	+10·40	−144·0	+ 1.30	+ 99·2	+ 3·74	+ 35·4	−1234·8	+ 1923
3	+116·5	+ 11·06	− 19·0	−122·6	+ 3620	+ 6·26	−142·6	− 2·98	+108·0	− 0·64	+ 39·4	−1239·2	+ 2381
6	+ 79·1	+ 33·2	+ 12·9	−114·0	+ 1160	+ 2·26	−109·4	− 6·08	+126·6	− 1·52	− 12·0	−1000·8	+ 159
9	+ 58·9	+ 65·6	+ 28·7	− 93·6	+ 1170	+ 0·82	− 61·4	− 6·24	+128·6	+ 0·48	− 58·6	− 882·4	+ 288
12	+ 39·6	+ 97·0	+ 44·1	− 45·0	+ 1860	− 1·96	− 3·6	− 6·30	+110·6	+ 0·92	− 98·0	− 778·8	+ 1081
15	+ 38·6	+ 97·8	+ 73·2	− 21·2	+ 2220	− 0·48	+ 23·4	− 0·80	+103·4	+ 5·28	−105·4	− 650·0	+ 1570
18	+ 16·4	+110·6	+119·8	0	+ 1810	− 3·76	+ 56·6	− 3·44	+ 58·6	+ 2·56	− 98·0	− 665·2	+ 1145
21	− 2·2	+109·0	+ 61·1	+ 34·0	+ 1840	−11·50	+124·0	− 7.60	+ 32·0	− 3·50	− 86·0	−1366·4	+ 474
24	− 27.7	+ 78.2	+ 56·4	+ 69·8	+ 1770	−14·12	+163·4	− 7.34	− 16·4	− 5·28	− 72·6	−1804·4	− 34
27	− 41·1	+ 43·4	+ 37·0	+100·4	+ 1890	−13·16	+168·0	− 3·80	− 80·0	− 6·20	− 29·4	−1725·6	+ 64
					20,498							11,347·6	9,051

Frequency, 52·7. $\rho_1 = \rho_2 = \rho_3 =$ ·012 ohm. $\sqrt{\text{mean}^2}$ E_1 & E_2 = 60 volts.

TABLE IX.

Extra resistance in armature $\rho_1=\rho_2=\rho_3$ in ohms.	Total resistance of armature $v_1+\rho_1={}_2v+\rho_2=v_3+\rho_3$ in ohms.	Torque in lbs. at 3·5 feet radius. Calculated.	Torque in lbs. at 3·5 feet radius. Observed.	Mean $m_1\frac{dI_1}{dt}x_1+m_2\frac{dI_2}{dt}x_2$ over a half period in watts.	Mean $e_1y_1+e_2y_2+e_3y_3$ over a half period in watts.	Average dissipation due to magnetic hysteresis, eddy currents, and waste field over a half period in watts.	If dissipation due to magnetic hysteresis and eddy currents be assumed constant & equal to 176 watts, waste field would account for in watts.	For further particulars see Tables.	Maximum average values of $m_1\int\frac{dI_1}{dt}dt$ and $m_2\int\frac{dI_2}{dt}dt$ $\div 75\times{\cdot}83$ in C. G. S. units, 10^6.	Maximum average value of $\int e_1dt, \int e_2dt$, and $\int e_3dt$ $\div 24\times{\cdot}93$ in C. G. S. units, 10^6.	Difference.	Difference expressed as a percentage of max. av. value of $m_1\int\frac{dI_1}{dt}dt$ and $m_2\int\frac{dI_2}{dt}dt$ $\div 75\times{\cdot}83$.	Frequency f.	$\sqrt{\text{mean}^2}$ in volts. E_1	$\sqrt{\text{mean}^2}$ in volts. E_2	Maximum volts. $m_1\frac{dI_1}{dt}$	Maximum volts. $m_2\frac{dI_2}{dt}$	Maximum amperes. x_1	Maximum amperes. x_2
∞	∞	0	0	176	0	176	0	V.	·437	·322	·115	26·3	52·2	60	60	96	94	13·9	12·5
1·03	1·06	1·5	2 about	2091	790	1301	1125	VI.	·419	·319	·100	23·9	52·1	"	"	83	88	46·6	41·4
0·150	0·175	6·72	7 "	3723	3507	216	40	VII.	·401	·286	·115	28·7	52·0	"	"	74	82	71·8	70·6
0·012	0·037	2·2	2 "	2050	1135	905	729	VIII.	·367	·0864	·2806	76·5	52·7	"	"	116	120	111	123

Alteration in phase from zero on open circuit. (360° = 1 period.) $m_2\frac{dI_2}{dt}$	$m_1\frac{dI_1}{dt}$	x_1	x_2	Phase-difference between $\frac{dI_1}{dt}$ & x_1	$\frac{dI_2}{dt}$ & x_2	Maximum volts. e_1	e_2	e_3	Maximum amperes. y_1	y_2	y_3	Alteration in phase from zero on open circuit. e_1	e_2	e_3	y_1	y_2	y_3	Phase-difference between e (for $\rho=\infty$) and y. e_1 & y_1	e_2 & y_2	e_3 & y_3	Phase-difference between e (for $\rho=ab\ldots$) and y. e_1 & y_1	e_2 & y_2	e_3 & y_3	Phase-difference between x_1 & y_1	x_1 & y_2	x_1 & y_3	x_2 & y_1	x_2 & y_2	x_2 & y_3
0°	0°	0°	0°	+ 79·2	+ 79·2	22·2	24·4	24·8	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	4·8	69·6	114	94·8	159·6	24
+ 3·6	+ 2·4	−18	−25·2	+ 57·6	+ 51·6	25·2	24·2	26·9	22·5	22	25	+18	+27·6	+19·2	+13·2	+14·4	+14·4	+13·2	+14·4	+14·4	+ 4·8	+12·0	+ 3·0	36	102	146·4	133·2	199·2	63·6
+ 6·0	+ 6·0	−28·4	−25·2	+ 46·8	+ 48·0	25·7	20·6	22·5	115	114	112	+42	+49·2	+60	+35·4	+34·8	+57·6	+36	+34·8	+57·6	+ 7·2	+15·6	+ 2·4	66	130·8	198	155·4	219·6	106·8
− 7·2	−13·2	−20·4	−12·0	+ 66·0	+ 80·4	14·1	7·6	6·2	168	129	105	+22·8	+86·4	+48	+38·4	+40·8	+68·4	+38·4	+40·8	+68·4	−15·6	+45·6	−20·4	63·6	130·8	202·8	145·2	212·4	104·4

TABLE X.

Angle in 60ths of a period.	A Circuit. $m_1\frac{dI_1}{dt}$ Volts.	A Circuit. x_1 Amps.	B Circuit. $m_2\frac{dI_2}{dt}$ Volts.	B Circuit. x_2 Amps.	$m_1\frac{dI_1}{dt}x_1 + m_2\frac{dI_2}{dt}x_2$ Watts.	e_1 Volts.	e_2 Volts.	e_3 Volts.
0	+ 80·5	− 4·20	− 72·2	− 7·08	+ 173	− 17·3	+ 30·6	− 16·4
3	+ 100·9	− 1·44	− 49·3	− 8·34	+ 266	− 10·2	+ 30·1	− 22·1
6	+ 122	+ 1·69	− 7·2	− 8·71	+ 269	+ 2·12	+ 23·4	− 24·3
9	+ 114	+ 3·82	+ 36·4	− 7·84	+ 150	+ 12·5	+ 11·0	− 22·1
12	+ 87	+ 6·21	+ 64·4	− 6·02	+ 152	+ 22·9	+ 8·32	− 29·3
15	+ 71·6	+ 8·28	+ 81·1	− 3·76	+ 288	+ 26·4	+ 3·45	− 28·1
18	+ 47·5	+ 9·84	+ 101·3	− 1·32	+ 333	+ 17·3	− 3·77	− 23·1
21	+ 7·9	+ 10·16	+ 121·6	+ 1·44	+ 255	+ 25·4	− 12·7	− 11·6
24	− 34·9	+ 9·22	+ 112·8	+ 3·32	+ 52	+ 16·9	− 18·4	+ 3·47
27	− 65·9	+ 7·02	+ 87·4	+ 5·27	− 2	+ 20·6	− 27·9	+ 11·2
					1,936			

Frequency = 95·5. $\rho_1 = \rho_2 = \rho_3 = 8$
$E_1\sqrt{\text{mean}^2} = 80$; $E_2\sqrt{\text{mean}^2} = 80$ volts.

TABLE XI.

Angle in 60ths of a period.	A Circuit. $m_1\frac{dI_1}{dt}$ Volts.	A Circuit. $m_2\frac{dI_2}{dt}x_1$ Amps.	B Circuit. $m_2\frac{dI_2}{dt}$ Volts.	B Circuit. x_2 Amps.	$m_1\frac{dI_1}{dt}x_1 + m_2\frac{dI_2}{dt}x_2$ Watts.	e_1 Volts.	y_1 Amps.	e_2 Volts.	y_2 Amps.	e_3 Volts.	y_3 Amps.	$e_1 y_1 + e_2 y_2 + e_3 y_3$ Watts.	Dissipation of energy due to waste field, hysteresis and eddy currents. Watts.
0	+ 81·7	+ ·25	− 77·5	− 18·5	+ 1455	− 25.1	+ 23·6	+ 27·5	− 24·2	− 0·2	+ 1·33	− 1258	+ 196
3	+ 109·9	+ 2·65	− 54·6	− 18·4	+ 1296	− 19·8	+ 19·4	+ 26·7	− 22·1	− 1·2	+ 2·95	− 977	+ 319
6	+ 114·7	+ 7·69	− 15·9	− 15·1	+ 1122	− 11·7	+ 11·4	+ 24·7	− 21·4	− 9.6	+ 10·8	− 766	+ 356
9	+ 109·3	+ 16·9	+ 27·98	− 9·95	+ 1569	− ·7	− 13·9	+ 25·1	− 23·9	− 27·2	+ 24·5	− 1256	+ 313
12	+ 93·2	+ 19·9	+ 61·9	− 4·66	+ 1567	+ 4·5	− 7·2	+ 20·5	− 20·8	− 33·1	+ 27·6	− 1371	+ 195
15	+ 78·4	+ 20·5	+ 84·6	+ ·13	+ 1618	+ 15.2	− 13·9	+ 15·9	− 14·0	− 29·8	+ 27·4	− 1251	+ 367
18	+ 56·1	+ 19·8	+ 110·9	+ 2·52	+ 1390	+ 16·6	− 14·9	+ 9·5	− 7·5	− 26·3	+ 24·0	− 949	+ 441
21	+ 15·2	+ 16·8	+ 118·7	+ 7·06	+ 1093	+ 23·8	− 19·2	+ 4·6	− ·59	− 17·3	+ 18·7	− 784	+ 310
24	− 22·4	+ 11·8	+ 112·5	+ 14·0	+ 1311	+ 33·3	− 25·5	− 5·7	+ 10·6	− 11·1	+ 14·7	− 1072	+ 238
27	− 57·4	− 3·91	+ 93·9	+ 17.8	+ 1447	+ 30·1	− 26·5	− 22·4	+ 21·5	− 4·0	+ 3·83	− 1295	+ 151
					13,867							10,981	2,886

Frequency, 95. $\rho_1 = \rho_2 = \rho_3 = 1·07$ ohms;
$E_1\sqrt{\text{mean}^2} = 80$; $E_2\sqrt{\text{mean}^2} = 81$ volts.

TABLE XII.

Angle in 60ths of a period.	A Circuit. $m_1\frac{dI_1}{dt}$	A Circuit. x_1	B Circuit. $m_2\frac{dI_2}{dt}$	B Circuit. x_2	$m_1\frac{dI_1}{dt}x_1 + m_2\frac{dI_2}{dt}x_2$	e_1	y_1	e_2	y_2	e_3	y_3	$e_1y_1+e_2y_2+e_3y_3$	Dissipation of energy due to waste field, hysteresis and eddy currents.	Remarks.
	Volts.	Amps.	Volts.	Amps.	Watts.	Volts.	Amps.	Volts.	Amps.	Volts.	Amps.	Watts	Watts.	
0	+ 75·9	− 3·45	− 72·6	− 61·7	+ 4217	+22·8	−108	− 5·18	+ 67·9	− 2·46	+ 42·1	−3056	+ 1161	Frequency 95. $\rho_1=\rho_2=\rho_3=0·149$ ohm. $E_1\ \sqrt{\text{mean}^2}=78$. $E_2\ \sqrt{\text{mean}^2}=80$ volts.
3	+129·8	+ 4·7	− 54·4	− 64·3	+ 4108	+19·6	−111	−11·0	+ 81·2	− 3·0	+ 30·7	−3357	+ 751	
6	+119	+23·2	− 31·1	− 58·0	+ 3520	+14·5	− 96·1	−16·6	+ 95·6	− 3·3	0	−3254	+ 266	
9	+ 97	+45·1	+ 19·1	− 42·9	+ 3556	+ 8·9	− 66·9	−18·0	+106·0	+ 3·54	− 39·6	−2985	+ 571	
12	+ 83·2	+61·1	+ 50·4	− 23·2	+ 3915	+ 1·8	− 22·8	−17·6	+103·0	+ 10·68	− 78·8	−3013	+ 902	
15	+ 77	+67·4	+ 97·1	− 7·2	+ 4490	+ 5·8	+ 6·44	− 7·06	+ 95·6	+ 23·6	− 98·1	−3232	+ 1258	
18	+ 57·3	+69·3	+131·9	+ 2·5	+ 4302	− 3·85	+ 23·8	−12·6	+ 86·7	+ 18·26	−105	−3332	+ 970	
21	+ 21·2	+62·4	+104·4	+ 16·6	+ 3056	−13·1	+ 44·1	−15·2	+ 59·9	+ 11·35	−101	−2748	+ 308	
24	− 24·6	+43·6	+104·4	+ 40·7	+ 3177	−22·9	+ 84·2	−10·5	+ 6·93	+ 6·8	− 90·6	−2618	+ 559	
27	− 46·2	+24·4	+ 85·3	+ 52·7	+ 3368	−25·4	+ 99·6	− 4·39	− 28·2	+ 4·49	− 71·3	−2950	+ 418	
					37,709							30,544	7,165	

TABLE XIII.

Angle in 60ths of a period.	A Circuit $m_1\frac{dI_1}{dt}$	A Circuit x_1	B Circuit. $m_2\frac{dI_2}{dt}$	B Circuit. x_2	$m_1\frac{dI_1}{dt}x_1 + m_2\frac{dI_2}{dt}x_2$	e_1	y_1	e_2	y_2	e_3	y_3	$e_1y_1+e_2y_2+e_3y_3$	Dissipation of energy due to waste field, hysteresis and eddy currents.	Remarks.
	Volts.	Amps.	Volts.	Amps.	Watts.	Volts.	Amps.	Volts.	Amps.	Volts.	Amps.	Watts.	Watts.	
0	+ 73·53	−25·1	− 52·2	−74·6	+ 2049	−14·67	+159	− 4·50	−137·5	− 7·78	− 37·5	− 1419	+ 630	Frequency 95·5. $\rho_1=\rho_2=\rho_3=·0095$ ohm. $E_1\ \sqrt{\text{mean}^2}=78$. $E_2\ \sqrt{\text{mean}^2}=80$ volts.
3	+123·8	− ·81	− 32·9	−86·4	+ 2740	−10·88	+153	− ·55	−147	− 4·78	− 15·6	− 1509	× 1231	
6	+112·2	+15·8	+ 6·8	−83·9	+ 1202	− 5·21	+107·5	+ 3·74	−153	− 1·84	+ 6·25	− 1143	+ 59	
9	+114	+41·0	+ 52·7	−72·6	+ 848	+ 5·92	+ 68·7	+15·6	−144	+12·25	+ 50	− 1231	− 383	
12	+ 72·2	+68·1	+ 64·9	−49·1	+ 1733	+ 2·82	− 9·37	+ 7·37	−147	− 1·73	+122	− 1317	+ 416	
15	+ 66·6	+84·8	+ 77·6	−23·9	+ 3284	+ 3·76	− 50·0	+ 6·22	−125	− 3·31	+153	− 1472	+ 1812	
18	+ 40·6	+95·7	+111·2	− 6·08	+ 3210	− 4·53	− 56·2	− 2·63	−103	−10·94	+153	− 1144	+ 2066	
21	+ 1·63	+92·5	+100·1	+ 5·68	+ 1064	+ 4·27	− 53·1	+ 4·61	− 59·4	− 1·83	+134	− 746	+ 318	
24	− 38·01	+74·2	+ 90·6	+34·9	+ 431	+15·4	−147	+ 8·41	+ 53·1	+ 6·54	+106	− 1121	− 690	
27	− 86·2	+49·5	+ 90·5	+59·2	+ 1092	+19·4	−137·5	+11·94	+ 71·9	+12·27	+ 84·4	− 776	+ 316	
					17,560							11,880	568	

TABLE XIV.

Extra resistance in armature $\rho_1=\rho_2=\rho_3$ in ohms.	Total resistance of armature in ohms.	Torque in lbs. at 3·5 feet radius. Calculated.	Torque in lbs. at 3·5 feet radius. Observed.	Mean $m_1\frac{dI_1}{dt}x_1+m_2\frac{dI_2}{dt}x_2$ over a half period in watts.	Mean $e_1y_1+e_2y_2+e_3y_3$ over a half period in watts.	Average dissipation due to magnetic hysteresis, eddy currents, and waste field over a half period in watts	If dissipation due to magnetic hysteresis and eddy currents be assumed constant & equal to 194 watts, waste field would account for in watts.	For further particulars see Tables.	Maximum average value of $m_1\int\frac{dI_1}{dt}dt$ and $m_2\int\frac{dI_2}{dt}dt$ $\div$ 75 × ·83 in C. G. S. units, 10^6.	Maximum average value of $\int e_1dt, \int e_2dt, \int e_3dt,$ $\div$ 24 × ·93 in C. G. S. units, 10^6.	Difference.	Difference expressed as a percentage of max. av. value of $m_1\int\frac{dI_1}{dt}dt$ and $m_2\int\frac{dI_2}{dt}dt$ $\div$ 75 × ·83.	Frequency f.	$\sqrt{\text{mean}^2}$ in volts. E_1	$\sqrt{\text{mean}^2}$ in volts. E_2	Maximum volts. $m_1\frac{dI_1}{dt}$	Maximum volts. $m_2\frac{dI_2}{dt}$	Maximum amperes. x_1	Maximum amperes. x_2
∞	∞	0	0	194	0	194	0	X.	·309	·208	·100	32·5	95·5	80	80	122	122	10·2	9·8
1·07	1·10	1·2	1¼ abt.	1386	1112	274	80	XI.	·315	·205	·110	34·9	95	80	81	114·7	119	20·5	18·5
·149	·174	3·2	4 "	3771	3054	717	523	XII.	·308	·141	·167	54·2	95	78	80	130	132	69·3	64·3
·0095	·0345	1·2	1 "	1756	1188	568	374	XIII.	·312	·065	·247	79·2	95·5	78	80	124	161	95·7	86·4

Alteration in phase from zero on open circuit. (360° = 1 period.) $m_1\frac{dI_1}{dt}$	$m_2\frac{dI_2}{dt}$	x_1	x_2	Phase-difference between $\frac{dI_1}{dt}$ & x_1	$\frac{dI_2}{dt}$ & x_2	Maximum volts. e_1	e_2	e_3	Maximum amperes. y_1	y_2	y_3	Alteration in phase from zero on open circuit. e_1	e_2	e_3	y_1	y_2	y_3	Phase-difference between e (for $\rho = \infty$) and y. e_1 & y_1	e_2 & y_2	e_3 & y_3	Phase-difference between e (for $\rho = ab \ldots$) and y. e_1 & y_1	e_2 & y_2	e_3 & y_3
0°	0°	0°	0°	77°·4	78°·0	26·4	30·6	29·3	0	0	0	0°	0°	0°	0°	0°	0°	0°	0°	0°	0°	0°	0°
+ 4·8	+ 3·6	− 28·2	− 27·6	44°·4	46°·8	33·3	27·5	33·1	26·5	24·2	27·6	+ 24	+ 30·6	+ 40·8	+ 12	+ 30	+ 46·2	12°·0	30°·0	46°·8	− 12°·0	− 9·6	+ 4·8
+ 3·0	+ 4·2	− 19·2	− 13·8	55°·2	60°	25·4	21·2	23·6	111	106	105	+ 69·6	+ 60·0	+ 87·0	+ 39·4	+ 50	+ 85·2	54·0	50°·4	86°·4	− 15·6	− 9·6	− 2·4
1·8	− 6·0	− 8·4	+ 0·6	70°·8	84°	19·4	15·6	12·3	159	153	153	+ 87·6	+ 103	+ 80·4	+ 37·2	+ 37·4	+ 71·4	37·2	38°·4	72°·0	+ 50·4	+ 115·2	− 81·6

TABLE XV.

$\rho_1 = \rho_2 = \rho_3$	Frequency.	Alteration in the ordinate of $\int e$ (for $\rho = \infty$) dt when $y = o$. + signifies increase ordinate. − „ decrease „			
		y_1	y_2	y_3	Average.
		%	%	%	%
1·03	52	+ 7·3	+ 3·85	− 2·7	+ 2·81
·150	52	+ 29·4	+ 11·1	+ 28·0	+ 22·8
·012	52	− 44·1	− 75·0	− 83·0	− 67·3
1·07	95	+ 6·0	+ 25·6	+ 13·1	+ 14·9
·149	95	+ 62·5	+ 25·0	+ 92·3	+ 59·9
·0095	95	− 47·4	− 92·3	− 63·8	− 67·8

TABLE XVI.

Angle in 60ths of a period.	Frequency 52.			Frequency 95.		
	$\rho_2 = 1·03$	$\rho_1 = ·15$	$\rho_1 = ·012$	$\rho_1 = 1·07$	$\rho_1 = ·149$	$\rho_1 = ·0095$
	L in 10^6 C.G.S. units.	L in 10^6 C.G.S. units.	L in 10^6 C.G.S. units.	L in 10^6 C.G.S. units.	L in 10^6 C.G.S. units.	L in 10^6 C.G.S. units.
0	1·03	0·418	0·089	0·58	0·499	0·064
3	..	..	..	..	..	..
6	1·35	0·386	0·077	0·31	0·513	0·036
9	..	..	..	..	..	..
12	1·20	0·289	0·018	1·81	0·798	0·024
14	..	..	0·355	..	..	..
15	..	..	0·400	..	..	..
16	..	..	0·387	1·74	..	..
18	0·92	0·871	0·336	1·75	1·06	0·208
20	..	..	..	1·46	..	0·323
21	..	..	0·198	1·37	..	0·333
24	0·97	0·489	0·126	0·93	0·564	0·127
27	..	..	..	..	..	..

TABLE XVII.

Angle in 60ths of a Period.	Frequency 95. Increase $\int e_1$ (for $\rho = \infty$) dt by 6 per cent. $\rho = \infty$ $\int e_1 dt$	$\rho = \infty$ $1{\cdot}06 \int e_1 dt$	$\rho = 1{\cdot}07$ $\int e_1 dt$	$1{\cdot}06 \int e_1 dt$ $- \int e_1 dt$	$1{\cdot}06 \int e_1 dt$ $- \int e_1 dt$ in 10^6 C.G.S. units.	γ_1 in C.G.S. units. See Table XI.	L in 10^6 C.G.S. units.
0	+ 20·2	+ 21·4	+ 13·6	+ 7·8	+ 1·37	+ 2·36	+ 0·58
3	..	..	..	..	..	..	..
6	+ 25·7	+ 27·2	+ 25·2	+ 2·0	+ ·351	+ 1·14	+ 0·308
9	..	..	..	..	..	..	..
12	+ 18·0	+ 19·1	+ 26·5	− 7·4	− 1·30	− 0·72	+ 1·81
15	..	..	..	..	..	..	..
16	+ 7·7	+ 8·16	+ 22·1	− 13·9	− 2·44	− 1·40	+ 1·74
18	+ 3·7	+ 3·92	+ 18·8	− 14·9	− 2·61	− 1·49	+ 1·75
20	0·0	0·0	+ 15·0	− 15·0	− 2·63	− 1·80	+ 1·46
21	− 2·0	− 2·12	+ 13·0	− 15·0	− 2·63	− 1·92	+ 1·37
24	− 8·9	− 9·43	+ 4·2	− 13·63	− 2·39	− 2·55	+ 0·93
27	..	..	..	..	..	..	..

TABLE XVIII.

Frequency.	Extra Resistance in Armature $\rho_1 = \rho_2 = \rho_3$	Measured Torque in lbs. at 3·5 ft. radius.		Volts $\sqrt{\text{mean}^2}$ $E_1 E_2$	Amperes $\sqrt{\text{mean}^2}$ in A + B Circuits.	See Tables for further particulars.
	ohms.	lbs.	ozs.			
34	·01	2	3	44	99	
,,	·136	4	0	34	39	
,,	·43	1	14	33	15	
,,	1·02	0	14	34	Less than 12	
,,	2·35	0	8	33	,, 12	
52	·012	2	0	60	..	VIII., XI.
,,	·15	7	0	60	..	VII., XI.
,,	1·03	2	0	60	..	VI., XI.
99	·01	0	15	60	45	
,,	·136	1	5	60	36	
,,	·43	1	5	60	20	
,,	1·02	0	14	60	9	
,,	2·35	0	8	60	Less than 6	
95	·0095	1	0	80	80	XI., XIV.
,,	·149	4	0	81	63	XII., XIV.
,,	1·07	1	1	78	..	XIII., XIV.
95·5	·01	2	0	91	102	
,,	·136	5	0	91	75	
,,	·43	3	4	91	36	
,,	1·02	1	6	91	18	
,,	2·35	0	9	91	12	

Mr. Mordey.

Mr. MORDEY: I must confess that to me this paper is too complex and too full of matter to allow of the full meaning being grasped by hearing it read in abstract. Professor Wilson's concluding remarks, that ordinary theories of self-induction are not applicable to induction motors is interesting in view of what we know as to the development of such motors. But if we ask Mr. Brown, or any of the engineers who have developed the induction motors, we shall find that theories of any sort have had very little indeed to do with the production of such motors. They are the outcome of experiments. Professor Wilson gives us the results of his experiments, but does not sufficiently interpret them for us—perhaps when we have mastered the paper we shall find the explanations are there. For example, what is the explanation of the increased magnetic leakage at 100 periods as compared with 50 periods? May I ask, in the first place, if the machine was suitable for 95 period working; and, in the second place, whether the explanation is not to be found in the increased eddies at the higher periods? The effect of such eddies would be to cause a splaying of the lines, and that would, of course, make the leakage greater. I have tried to find in Tables IX. and XIV. some verification of that supposition, and it appears that the proportion of rise of leakage with armature open-circuited – 26·3 to 32·5 is roughly the same as the increase of the eddies and magnetic hysteresis. In the tables the lower value is 176 and the higher value is 194. It is to be noticed, however, that the eddy and hysteresis loss is very nearly proportional to the impressed volts. But as the impressed volts do not seem to be proportional to the impressed periodicity—that is to the expected increase of speed —it is not easy to see what the author was really investigating.

Another point: Will the author explain to us in what way the waste field accounts for the loss of energy? So far as I understand this question—and I am afraid that is not very far—the waste magnetism is not really a cause of loss of energy except indirectly in the sense that perhaps a little more energy may be used in the copper; but it is not in that way the author traces the increase of loss. It may be, of course, that the waste field, being a field which does not wholly follow the proper direction but goes at right angles to, or, at any rate, across the direction of lamination instead of with it, may cause an increase of eddies, and so cause a loss of energy. But this does not seem to me likely. Broadly, I think it is not correct to put magnetic leakage down as the cause of dissipation of energy. I suggest a fuller explanation of this point would be useful, especially as the loss attributed to this cause is at page 338 so enormously out of proportion to other losses. The whole subject is far from easy, and I hesitate even to make a suggestion at all on the first reading.

Mr. Field.

Mr. M. B. FIELD [*communicated*]: I am sure we all owe our thanks to the author of this paper for giving us the benefit of such a large amount of experimental data upon so important a subject as the Induction Motor.

I think, however, the title is a little misleading. Hearing that a paper was to be read entitled "The Induction Motor," I naturally supposed that we were going to have an account of induction motors

in general, *i.e.*, single-, two-, and three-phase motors; their adaptability to various kinds of work; how they compare with their rivals the continuous current motors; the general principles of design, &c., &c. I was, therefore, not unnaturally disappointed to find that the author has confined himself to but *one* motor, and nearly entirely to but one characteristic of this motor, viz., its starting capabilities. Mr. Field.

In the first place, I notice the motor under consideration was designed for 50 ∿, 220 volts, single-phase currents, and 15 B.H.P.

Now, all the tests given in the paper are for voltages from 44—118, frequencies 34—99, the greater bulk being for a voltage of about 60 and a frequency of about 50 ∿.

Again, in Table XVIII. the greatest torque tabulated is 24·5 lb. ft., corresponding at a speed of 1,000 revolutions per minute to but 4·65 B.H.P. In other words, the greatest starting torque actually measured does not equal ⅓ of the full load torque.

The test conditions were thus widely different from those for which the motor was designed, and I would first ask the author how far we are justified in pushing his conclusions to those cases where the motor is working under normal conditions?

Secondly, when a mass of experimental data is placed before us, we naturally ask if the motor in question is a typical one; if it is such that it is desirable to copy it; or if it embodies errors in design which we should in future avoid. Of this we could get a fair estimate did we know: (*a*) Power factor at half, three-quarter, and full load; (*b*) efficiency at different loads; (*c*) no-load current; (*d*) slip at full load; (*e*) breaking-down point, &c., &c. We are, however, left entirely in the dark as to all these points.

The first thing that strikes me is the large air-gap. 1·5 to 2 mm. for a 15 H.P. motor seems to me enormous. I am sure many motors are now made of the above out-put with about half this air-gap. To halve the air-gap means practically to halve the magnetising current and vastly to decrease the leakage, both most desirable points in the working of all induction motors.

Again, if the diagram shown in Fig. 6 be drawn to scale, I think the teeth in the stator are very long indeed; to this cause Professor Wilson attributes the great leakage of this motor. He measures 25 per cent. with open-circuited rotor, and reckons on 50 per cent. I am of opinion that no motor of the induction type could possibly be a practical success with anything like this leakage.

In a paper being published in the *Electrical Review* I show how, with an ordinary rotor as usually constructed, a 6 % leakage coefficient in the stator will bring down the maximum or breaking-down torque to 43 % of what it would be if leakage were absent; it is almost inconceivable how disastrous leakage is, even though the leakage coefficient (or percentage leakage with rotor on open circuit) be quite small.

The next question I should like to ask is, How was this motor intended to be connected when once started? Were the phases A and B to be joined in parallel or in series, or was B alone supplied with power?

Mr. Field.

Take, for example, Table V. The rotor was open-circuited. The square root of mean square of current in phase B is 8·4 amperes; frequency = 52·2 ∿; E_2 = 60 volts. Reducing this to 50 ∿, and, say, 215 volts (to allow for CR. drop), we get as magnetising current *at least.*

$$8{\cdot}4 \times \frac{215}{60} \times \frac{52{\cdot}2}{50} = 31{\cdot}3 \text{ amperes.}$$

When running as single-phaser the no-load current would be about 1·9 times the above, or, say roughly, 60 amperes. Now 15 B.H.P., reckoning on 88 % efficiency and 75 % power factor, is represented by 77·2 amperes and 220 volts. The no-load current would then appear to be very excessive.

The following are some figures relating to a modern 15 B.H.P single-phase motor as manufactured by a leading continental firm. The comparison of the two motors is interesting:—

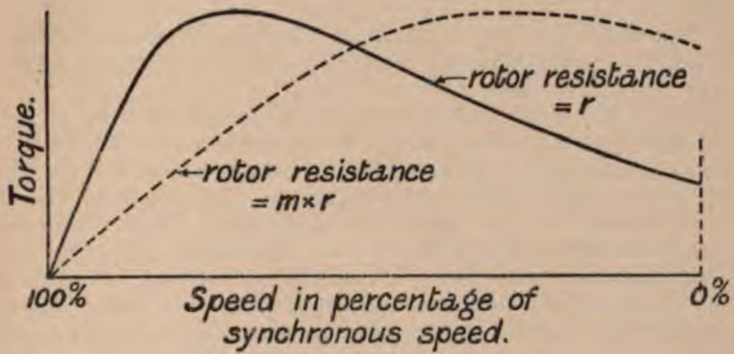
Messrs. Siemens Bros. & Co.'s Motor, used by Professor Wilson.		Modern Continental Motor.
B.H.P.	15	15
Cycles	50	50
Revolutions...	1,000	1,000
Type...	Single-phase	Single-phase
Volts...	220	—
Rotor	Y-grouping	Y-grouping
Diameter of rotor	45·0 cm.	34 cm.
Length of rotor	16·6 cm.	15 cm.
Length of air-gap	1·5–2 mm.	0·75 mm.
Power factor, full load	—	·78
Efficiency, full load	—	90 %
Slip	—	2 %

Altogether I judge, in the absence of more detailed information, that the motor in question was not such a very perfect one that we should be much inclined to copy it, and while recognising the scientific interest attaching to Professor Wilson's data, I am at a loss to see to what practical purpose we may apply them. His conclusions on pp. 338, 339 are more or less self-evident. If we have the torque T at a speed corresponding to 10 % slip, and we wish to get the same torque at 20 % slip, we have only to double the resistance of the rotor circuits by inserting between the slip-rings resistances equal to those of the individual circuits. The torque is determined by the field strength in which the rotor is, by the current strength in the rotor circuits, and

by the phase displacement of the latter relatively to the former. If r = resistance of the rotor circuits and l the coefficient of self-induction, the above phase displacement is determined by $\frac{l \times \text{slip}}{r}$. The rotor currents are, for a given field strength, proportional directly as the slip and inversely as $\sqrt{r^2 + l^2(\text{slip})^2}$. Hence, if we double r and double the slip, the rotor currents remain as before, the phase displacement is unaltered, the torque, field strength, and stator current remain identically as before. The only change is that double the power is now absorbed in the rotor circuits, and the motor runs slower to a corresponding degree (or with double the slip), delivering a correspondingly smaller amount of power to the shaft. Mr. Field.

In the same way, if we wish to get the same torque on starting, we must increase the rotor resistance ten times. In fact, if we draw the torque-slip curve and multiply the horizontal distances m times, we have the torque-slip curve for a motor with m times the resistance in the rotor circuits.

On pp. 332, 333 Professor Wilson shows the existence of harmonic



fields rotating with various velocities, and he deems it essential to eliminate them. I have called attention to these same harmonic terms in the paper above referred to, but I do not think them so *very* harmful. They are most injurious when dealing with single-phase motors with squirrel-cage rotors, as there we have terms rotating backwards as well as forwards, all producing backward torques. I have investigated the amount of the diminution of the torque on this account, and am of opinion that it does not amount to more than 2 or 3 per cent. when the motor has once been brought up to speed.

On page 337 Professor Wilson arrives, after some length, at the result that the torque on the rotor is equal to the C^2R loss in the rotor circuits divided by the slip, for this is what the expression given by him amounts to.

This result seems to me to be self-evident; if the field be rotating at 1,000 revolutions and pulling on the rotor with a torque T, the power

Mr. Field. which it will impart to the same will be 1,000 T; the rotor is revolving with, say, 10 % slip, and due to the load on the motor or the mechanical effect its shaft is producing, it will pull back on the rotating field with an equal torque T. The power developed will then be 900 T, and the difference, 100 T (or torque × slip), will be the loss inside the rotor, *i.e.*, the rotor C^2R loss.

Hence, generally, the rotor C^2R loss, divided by the slip, is equal to the torque on the rotor.

Professor Wilson, unfortunately, does not go on to compare the torque developed on starting with that when working as a single-phase motor, and thus point out whether the motor in question be at all suitable for traction purposes after all; nor does he even show any reason for the existence of a torque when working as a single-phase motor; nor, indeed, does he touch upon the way he is going to get his two-phase currents to start up the single-phaser, and the effect it has upon the starting torque if the two-phase currents be not exactly in quadrature, as, for example, when condensers are used as starters.

In Table XVIII. it is remarkable that the maximum torque for 95 $\sim$ occurs at the same value of ρ as it does for 52 $\sim$; viz., $\rho = {\cdot}15$ ohm. This is because sufficient values of ρ were not tried, the steps were too great.

It is easy to see that for a given field strength the maximum torque is arrived at when l (slip) $= r + \rho$, l being a coefficient of self-induction and $r + \rho$ the resistance of the rotor circuit.

Now I have pointed out that with constant supply E.M.F. the field strength[1] does not remain constant, as the load increases, and to maintain a constant field, the supply E.M.F. should be increased as the rotor currents increase. This is, of course, not the case in the experiments before us, as for each set of experiments the supply E.M.F. is kept very nearly constant. The value of $r + \rho$ for maximum torque as given by these experiments should then be rather in excess of that calculated on the assumption of a given constant field strength for any set of readings at the stated frequency. If we take the figures for frequency 95, and assume the maximum torque occurs when $r + \rho = {\cdot}174$ ohm, we have $l = {\cdot}00183$. We can now calculate the value of $(r + \rho)$ for any frequency to give a maximum starting torque, assuming, as stated above, that for each set of experiments at the given frequency, the supply E.M.F. be so manipulated as $(r + \rho)$ is varied that the field strength remains constant.

[1] By "field strength" I mean the strength of the *useful* field, or those lines threading both rotor and stator circuits. All other lines which thread one or the other, but not both, circuits, I consider as leakage, and these I do not include under the head "field strength."

Mr. Field.

Frequency.	$(r + \rho)$.	Measured Torque — Field strength increasing as $(r + \rho)$ increases.	$(r + \rho)$ calculated on assumption of field strength remaining constant as $(r + \rho)$ varies.
		lbs. oz.	
34	·035	2 3	
34	·161	4 0	·062
34	·455	1 14	
52	·037	2 0	
52	·175	7 0	·095
52	1·05	2 0	
99	·035	0 15	
99	·161	1 5	·181
99	·455	1 5	
95	·0345	1 0	
95	·174	4 0	·174
95	1·09	1 0	
95·5	·035	2 0	
95·5	·161	5 0	·175
95·5	·455	3 4	

We see, then, that the above relation between ρ and frequency is corroborated by Professor Wilson's figures.

It is to be very much regretted that Professor Wilson did not compare the starting torque in different positions of the rotor relatively to the stator, for this often varies between wide limits, and when considering starting torque is a matter of far greater importance than the variation over the half-period of time.

Professor E. Wilson in reply [*communicated*]: Mr. Mordey suggests that the dissipation of energy I have assigned to "waste" field is due to the field having a direction across the lamination instead of with it. In the absence of any experimental evidence I will express no opinion. That the energy is dissipated there can, I think, be no doubt, as the result does not depend upon one isolated experiment, but on six independent experiments. My definition of "waste" field in this case is that it is a field which wastes or dissipates energy, and I call attention to it in the way I have done in order to show what a great difference exists between the dissipation on open circuit and when the armature is loaded. In the absence of precise information as to the magnetic Professor Wilson.

Professor Wilson.

properties of the iron of this motor, and the conductivity there may be owing to the iron stampings being in contact, I cannot enter into a full discussion of leakage.

Dealing with Mr. Field's communication, I wish to point out that, as armature reaction was specially studied in these experiments, it was desirable to make it large. The experiments were difficult to make since it was necessary to keep to excessive starting conditions for a considerable time. The currents in the armature were as large as possible, and to get large armature reaction the field intensity was made small by employing low voltage for a given frequency. I have expressly stated in the paper that it does not deal with the motor when running; but the following information[1] was obtained from this same motor when running single-phase on the A circuit, with the armature short-circuited.

Applied Volts $\sqrt{\text{mean}^2}$.	Amperes $\sqrt{\text{mean}^2}$.	Power Factor.	Load on Motor.	Frequency of Supply.	Slip per cent.
175	53·4	0·16 about	0	50	1·3 about
149	70	0·70 ,,	0·37 of 15 B.H.P.	50	2·6 ,,

The importance of small air-space in these motors is well known, and I have given it some attention.[2] Mr. Field says that for constant slip the phase-difference between the armature currents and the actual field is determined by an expression inversely proportional to the resistance of the armature. Judging from the electro-motive force curves $\left\{e \text{ (for } \rho = a, b \ldots)\right\}$ and the curves of current (y), I should say this is *not* the case in my experiments. The data are given in the paper, and the curves only require to be integrated to prove this.

I have made a good many experiments with this motor, starting by aid of condensers from a single-phase circuit, and I had incorporated the results into the paper. With a condenser consisting of aluminium plates in potash alum solution I succeeded in getting about 55° (360° = 1 period) phase-difference between the fields.[3] I cut out the account of these experiments, since, as constructed, this motor should only be supplied with currents differing in phase by 90°. The observed torque, with 55° phase-difference, was of course less than in the experiments described in the paper. A different disposition of the primary windings should be employed for such a phase-difference as 55°, as I have endeavoured to show.

My object was not to vary the resistance in the armature circuit so as to find the actual maximum torque—all I required was to get an increase and then a decrease in the torque as the resistance (ρ) is continuously decreased. At the same time the actual maximum, if not given, could be found by interpolation from the observed data in Table XVIII. I took the steps in the resistance (ρ) just as I found them.

[1] See *Proceedings of the Institution of Civil Engineers*, vol. cxxxiii., session 1897–98, part 3.

[2] See *Electrician*, August 28, 1896.

[3] An account of some experiments made with Aluminium Condensers and Alternate Currents is given in *Proc. R. S.*, vol. lxiii. p. 329.

Professor Wilson.

I am sorry no remarks have been made upon the last section of the paper on Armature Reaction. This does not pretend to be *the* theory of armature reaction in an induction motor, and is simply the test of a simple way of looking at a difficult problem.

In conclusion, I beg to thank Mr. Mordey and Mr. Field for their remarks.

The Chairman.

The CHAIRMAN: I will ask you to accord your thanks to Professor Wilson for having laid before us this paper, which has been the result of a great deal of exceedingly patient and careful investigation. We do not yet know how valuable such a research may prove to be.

Carried by acclamation.

The CHAIRMAN: I have to announce that the scrutineers report the following candidates to have been duly elected:—

Member:

Walter C. Clatworthy.

Associate Members:

Alfred William Money.	Aubrey Ward Thomas.

Foreign Member:

Professor Joseph Epstein.

Associates:

George Finch.	Charles Henry Phillips.
Alfred Smith Hampton.	Andrew Stewart.
Edgar Loam.	William J. Unwin-Sowter.
Raphael Sanzio Mansel.	Charles Townend Joseph Young.

Students:

John Percy Bradshaw.	Harry Leonard Percy.
William Bradshaw.	Charles William Salt.
George Henry Van Corback.	William A. Toppin.
Colin Kidner.	Arthur Albert Watkins.

ORIGINAL COMMUNICATION.

NOTE ON STANDARD FREQUENCY.

By L. B. STILWELL, Member.[1]

IN the autumn of 1890, after the author had returned to the United States from a visit to England and the Continent, the subject of frequency was carefully considered in collaboration with Mr. Shallenberger, Mr. Schmid, and Mr. Scott, with a view to adopting standards for our manufacturing practice, and finally it was determined that apparatus should be built for two distinct frequencies, namely, 60 cycles per second and 30 cycles per second, and later experience has only tended to confirm the views then formed. The former frequency was fixed upon as suitable to incandescent lighting, arc lighting by alternating current, and the operation of motors. We should probably have further reduced this frequency to, say, 50 cycles per second, but for the fact that with hard carbon arc lamps we found some fluctuation of light. This fluctuation, as since ascertained, depends not only upon the hardness of the carbons, but also upon the form of the potential wave of the generator, the tendency to flicker being especially noticeable when lamps are supplied from generators, the potential curve or wave of which is of the "saw-tooth" type, and being much less when the current comes from generators whose potential curve is a sinusoid or a flattened sine curve.

In other words, this higher frequency aimed to take care of all kinds of service. The principal arguments in favour of a further reduction are (1) less inductance in circuits, which is somewhat important where large secondaries are to be dealt with; (2) fewer poles in generators for the machines of ordinary commercial sizes; (3) fewer poles and smaller size and cost of commutators for rotary converters.

[1] This communication is an extract from a letter, written in reply to an inquiry by one of the members of the Institution Committee on Uniformity in Electrical Engineering Practice, at the time when the subject of Standard Frequency was under consideration. It is published at the suggestion of the committee, and by consent of the author.—[ED.]

The principal arguments against a further reduction of frequency for a system aiming to supply all kinds of service are (1) greater cost of transformers; (2) possibility of fluctuation in arc lamps.

These arguments against further reduction weigh less to-day than they did in 1890, but another strong argument in favour of 60 cycles as against 50 cycles, or anything lower, now exists in the fact that the larger manufacturing companies in America have, for a number of years, adopted this frequency, and have on the market very extensive lines of apparatus which could not be utilised to advantage in connection with any lower frequency.

The system having a frequency of 30 cycles per second was designed for use particularly in connection with plants in which a very large part of the power transmitted and distributed is converted to continuous current by means of rotary converters. As compared with 60 cycles, it has a marked advantage in reducing the size and cost of rotary converters. A frequency of 30 cycles rather than one still lower was chosen chiefly for the reason that we desired a frequency not too low for successful lighting by incandescent lamps. I still think this frequency slightly preferable to that which we have at Niagara, namely, 25 cycles per second.

You will perhaps recall the fact that the frequency for the Niagara plant was largely determined by the fact that the speed of the turbines had been fixed before the generators were finally designed. This speed was 250 revolutions per minute, and a 12-pole machine would, of course, give 25 cycles per second, while a 16-pole machine would produce $33\frac{1}{3}$ cycles per second. Professor Forbes being in favour of a still lower frequency ($16\frac{2}{3}$ cycles), that of 25 cycles was finally adopted, although the frequency which the Westinghouse Company originally recommended was 30 cycles per second.

The potential curve (wave-phase) here at Niagara is a flattened sine, and we find that lighting by incandescent lamps (even 16 c.p. 100-volt lamps) is entirely satisfactory. We are now using this frequency for incandescent lighting in our village of Echota, and we have had no complaints whatever.

The same frequency, however, in connection with a

potential curve having a higher ratio of maximum value to mean value will cause very objectionable fluctuations of the light from incandescent lamps; I am, therefore, of the opinion that, for standard practice, a frequency of 30 cycles per second is preferable to one of 25 cycles in cases where any considerable amount of incandescent lighting is to be done.

Many of the larger plants in America have followed the lead of this plant, and consequently a frequency of 25 cycles is perhaps used more extensively in the United States than one of 30 cycles. Apparatus, however, which is designed for 25 cycles may operate quite as well on 30 cycles. The speed is, of course, increased, but not beyond reasonable limits, and the cost per h.p. output is reduced.

Another argument in favour of 30 cycles rather than 60 cycles, which is sometimes of weight, lies in the fact that the inductance of transmission circuits is materially less. For long-distance work this is of considerable importance.

NOTICE.

1. The Institution's Library is open to members of all Scientific Bodies, and (on application to the Secretary) to the Public generally.

2. The Library is open (except from the 14th August to the 16th September) daily between the hours of 11.0 a.m. and 8.0 p.m., except on Thursdays, and on Saturdays, when it closes at 2.0 p.m.

JOURNAL

OF THE

Institution of Electrical Engineers.

Founded 1871. *Incorporated* 1883.

ERRATA, PART 139.

On page 326, line 1, for "position" read "portion."

" 342, Fig. 10, for "$y_1 = 0_1\ \rho = \infty$" read "$y_1 = 0_1\ \rho = 1{\cdot}07$."

" 343, line 2, for "$10^{\cdot 6}$" read "10^6."

In Table IX. col. 2, for "$v_1 \ldots {}_2v \ldots v_3$" read "$r_1 \ldots r_2 \ldots r_3$."

In Tables IX. and XIV., cols. 9 and 11, for "$\div 75 \times {\cdot}83$" read "$\div (75 \times {\cdot}83)$."

" IX. and XIV., col. 10, for "$\div 24 \times {\cdot}93$" read "$\div (24 \times {\cdot}93)$."

In Table XI., in col. 2 (of A Circuit) delete $m_2 \dfrac{dI_2}{dt}$

" XIII., last col. but one (2nd line) for "-1231" read "$+1231$."

" XVIII., col. 6, lines 1, 2, 3 of col., for "XI." read "IX."

" " line 4 of col., for "XI." read "XIII."

" " line 6 of col., for "XIII." read "XI."

p. 63, 1891.

Ayrton, *Journal of the Institution of Electrical Engineers*, vol. xxii. p. 340, 1893.

Friese, *Elektrotechnische Zeitschrift*, February and March, 1894.

Hanappe, *L'Éclairage Électrique*, July, 1896.

Heldt, *Electrical World*, vol. xxviii., July, 1896.

Ritter, *L'Éclairage Électrique*, September, 1896.

[1] For full description of these machines see *Phil. Trans. Roy. Soc.*, vol. clxxxvii. (1896), A., pp. 229–252.

JOURNAL

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VOL. XXVIII. 1899. No. 140.

ORIGINAL COMMUNICATIONS.

A ROTARY CONVERTER.

By ERNEST WILSON, Member.

ONE object of this paper is to examine in a Rotary Converter the effect of variation of its exciting current and load upon phase difference between current and potential difference, and upon mechanical phase displacement of the armature of the converter. Another object is to investigate the disturbance in the induction over the bored face of the converter under different conditions as to load and excitation. The function of the converter has been to transform energy in the form of one- or two-phase alternate currents into energy in the form of direct currents. The alternate current generators[1] employed are rigidly coupled together with their armatures displaced one quarter period for two-phase working.

The following papers refer to the subject of this communication :

Thompson, *Philosophical Magazine*, August, 1888.
Webber, *Journal of the Institution of Electrical Engineers*, vol. xx. p. 63, 1891.
Ayrton, *Journal of the Institution of Electrical Engineers*, vol. xxii. p. 340, 1893.
Friese, *Elektrotechnische Zeitschrift*, February and March, 1894.
Hanappe, *L'Éclairage Électrique*, July, 1896.
Heldt, *Electrical World*, vol. xxviii., July, 1896.
Ritter, *L'Éclairage Électrique*, September, 1896.

[1] For full description of these machines see *Phil. Trans. Roy. Soc.*, vol. clxxxvii. (1896), A., pp. 229–252.

Routin, *L'Éclairage Électrique*, June, 1897.
Berg, *American Electrician*, 1897.
Woodbridge and Child, *Electrical World*, 1898.
Steinmetz, *Elektrotechnische Zeitschrift*, March, 1898.
Kapp, *Elektrotechnische Zeitschrift*, September, 1898.
Parshall, *Engineering*, 1898.
Thompson, *Journal of the Institution of Electrical Engineers*, November, 1898.[1]

THE CONVERTER.

The machine used as a Rotary Converter is a Siemens two-pole dynamo,[2] with smooth core armature intended to

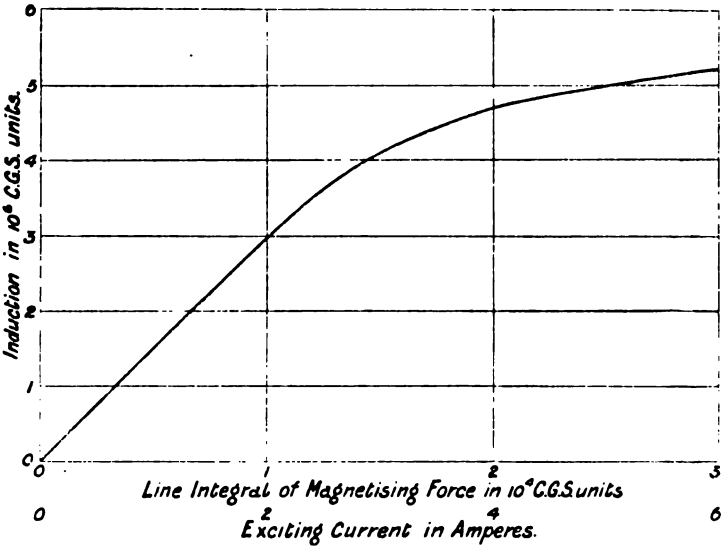

FIG. 1.

deliver 80 amperes 140 volts at 880 revolutions per minute as a direct current shunt wound generator. In addition to the two ordinary brushes, the machine is fitted with four gun-metal rings, making contact respectively with the commutator at 0°, 90°, 180°, and 270° of the circumference; otherwise they are insulated from the commutator by means of a hard wood cylinder one-eighth of an inch thick. Each

[1] The "communicated" portion of the discussion of Dr. Thompson's Paper, which came into my hands after writing this paper, contains some interesting remarks on the subject of phase variation.

[2] For full description of this machine, see *Proc. Roy. Soc.*, vol. li. p. 49; also *Electrician*, vol. xxviii. p. 609; also *Dynamo Machinery*, by Dr. J. Hopkinson, pp. 143–145.

ring is supplied with a copper brush. In addition, the commutator is provided with two exploring brushes for the purpose of examining the disturbance due to the current in the armature. These brushes touch two adjacent commutator plates, and are capable of being set to different positions round the commutator. The characteristic curve of this dynamo is given in Fig. 1.

Fig. 2 is a diagram of connections showing how the converter is connected to the alternators when working two-phase. The converter A was run up as a direct current motor, and synchronised by aid of an ordinary synchronising transformer. The load on the converter was adjusted by

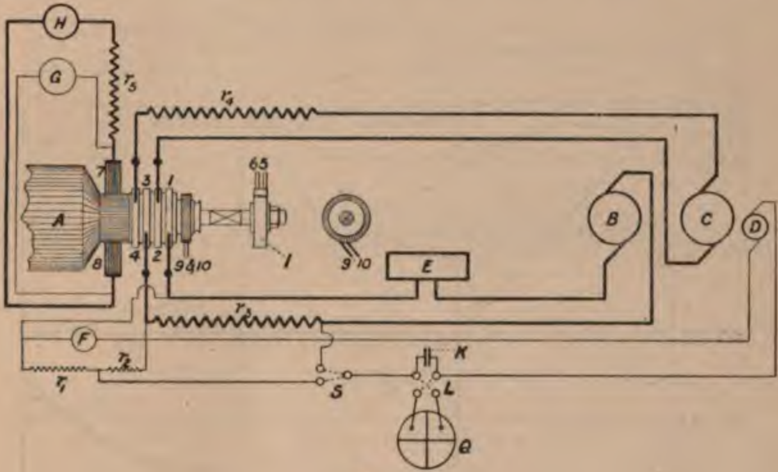

FIG. 2.

means of a variable non-inductive resistance r_5 and measured by a Weston voltmeter G, and Weston amperemeter H. The alternate current generators B, C are rigidly coupled together and connected respectively to the gun-metal rings 1, 3, and 2, 4, on the converter. The resistances r_3, r_4, are non-inductive and 0·063 ohms each. E is a Kelvin balance; F is a Kelvin multicellular voltmeter; Q is a Kelvin quadrant electrometer; D is a revolving contact-maker fixed to the alternate current generator axle; I is a contact-maker fixed to the converter axle, which makes contact once in a revolution; K is a condenser of 1 microfarad. When observing the potential difference at any moment between

the rings 1, 3, the two-way switch S was set to connect the junction of r_1, r_2, two considerable non-inductive resistances, to one pole of the electrometer reversing switch L. The revolving contact-maker D was then set to different positions of the phase, and for each the double deflection on Q was observed. When observing the current in the circuit of the generator B the switch S was set to connect one pole of L to the terminal of r_3; the deflection on Q is then proportional to the current, since r_3 is non-inductive. Similarly the circuit of the generator C can be dealt with by changing over the connections to the brushes on 2, 4, and the non-

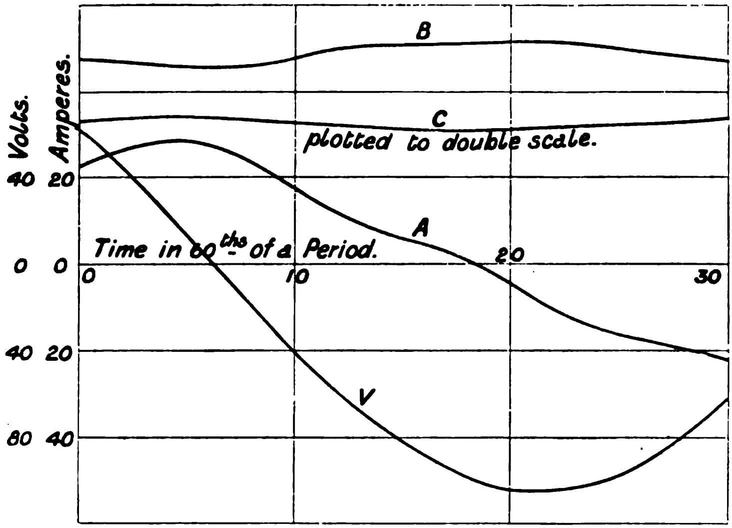

FIG. 3.

inductive resistance r_4. When observing the mechanical displacement between the armatures of the converter and alternate current generators B C, the contact-makers I and D were placed in series with one another, a reflecting galvanometer and a primary battery being included in the circuit. The brushes 5, 6, on I were set so that when the contact between them was established, the armature of A developed zero potential difference between brushes 1, 3, with no current in the armature. The reading on D when both contact-makers made contact simultaneously, and when B developed zero potential difference on open circuit was 7·5.

In any experiment D was then moved until both contact-makers made simultaneous contact as was shown by the deflection of the galvanometer needle. The reading on D enabled one to find the displacement required, since it was only necessary to subtract the reading 7·5 from it.

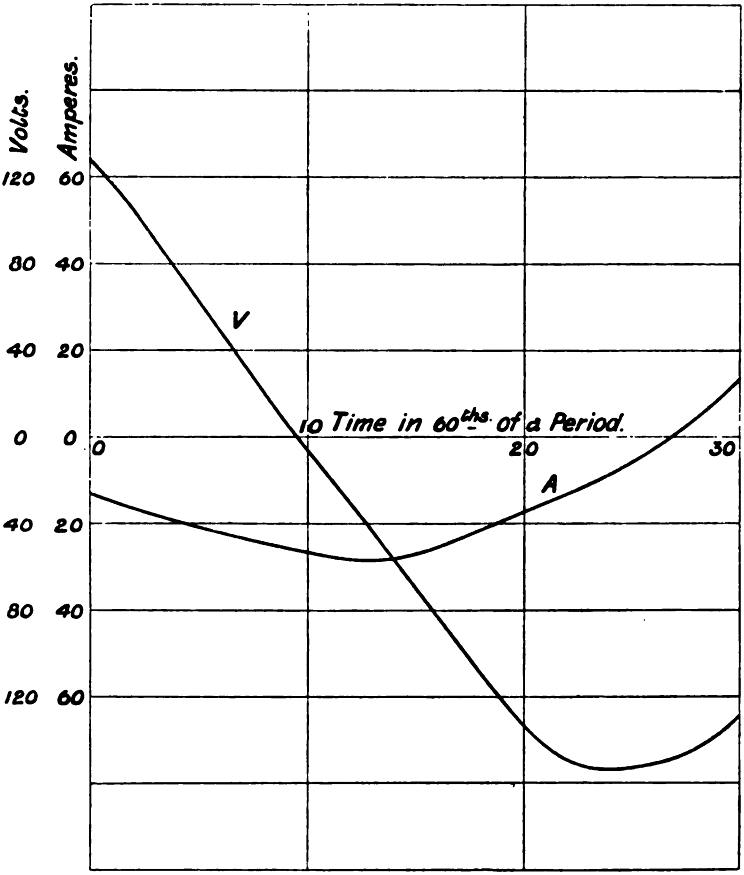

FIG. 4.

TWO-PHASE EXPERIMENTS.

1. Rotary converter not loaded between brushes 7, 8, Fig. 2. In this case we have simply to consider a two-phase synchronous motor in which energy is dissipated by brush bearing and wind friction, by magnetic hysteresis and eddy currents, and by currents in the copper conductors.

Figs. 3 and 4 give the observed volts and amperes for the B circuit in terms of time plotted horizontally in 60ths of a period, the frequency in each case being 23·5 periods per second. Particulars of these experiments are given in the table, the tests being numbered 1 and 2 respectively. It is necessary to explain here how the actual displacement of the armature A is found, for it is not wholly given by the contact-makers I and D when placed in series as already described. There is the effect due to armature reaction in B and C, and the fall of potential due to current into the resistance of the armature and connecting wires to be considered; that is to say, phase of potential difference between brushes on rings 1 and 3 is not the same when B is loaded as when unloaded. Now the fiducial mark on D is known when the armature of B has a known position with regard to the pole pieces; any displacement of phase due to armature reaction and fall of potential due to current must therefore be subtracted from the displacement found by I and D in order to obtain the actual displacement of the armature A. This would not be necessary if the generator were devoid of armature reaction and current were in phase with potential difference. In experiment 1 the actual displacement so found is 19·8°, in experiment 2 it is 21·6°, and these are due to armature reaction and fall of potential due to current in the armature conductors in the converter. The exciting current in the converter is given in amperes, and by inspecting Fig. 1 the position on the characteristic curve can be found. In experiment 1 the converter is under-excited; that is, on open circuit it would generate an electromotive force smaller than B would generate on open circuit. We see, as we should expect, that there is a considerable lag (72°) of current with regard to potential difference. As the excitation of the converter is increased, that of the alternator remaining constant, the curves come into phase. When the excitation of A is increased to the extent shown in experiment 2—that is when the converter may be said to be more highly excited than B and C, or it would generate a higher E.M.F. on open circuit than B or C on open circuit—the current leads the potential difference by 77°, and the actual displacement of the armature A is 21·6°. In each experiment the machines B and C are alternately generators and motors, and A is alternately motor

and generator during each half-period. On the whole, of course, B and C are generators, and the effect is simply to alter the position in each half-period at which B and C are alternately generators and motors. This subject has received careful attention [1] when B and C were rigidly coupled together, the one running as a single-phase motor and driving the other as a single-phase generator, in which case it was shown that an over-excited motor produces a lead of

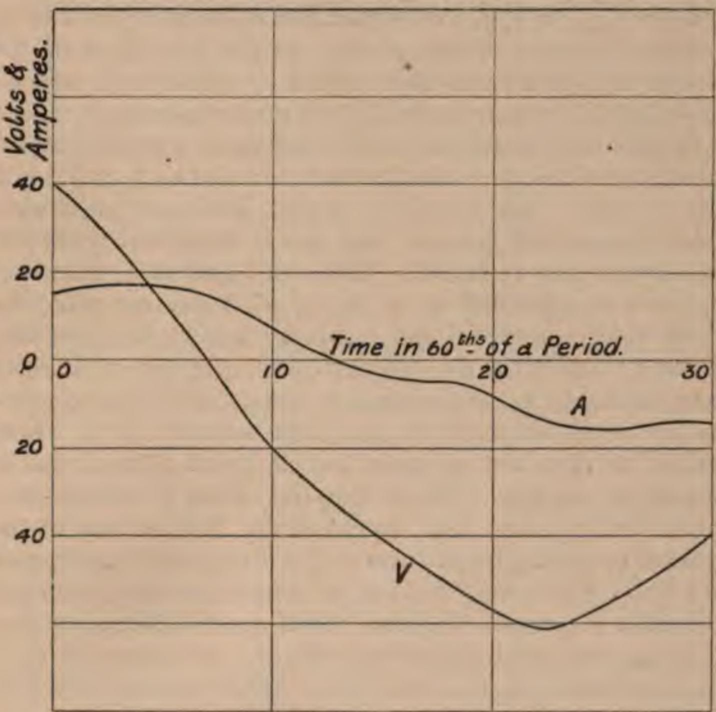

FIG. 5.

current with regard to E.M.F. In the present investigation the motor is two-phase, and there is no mechanical constraint between the generators B C and the motor A. In experiment 3, Fig. 5, the converter excitation is reduced, whilst the generator is the same. The phase difference between potential difference and current is reduced to 35°, the current lagging.

[1] See *Phil. Trans. Roy. Soc.*, vol. clxxxvii. (1896), A. pp. 229–252.

We have the following variables to deal with :—(1) Excitation of generator and converter. (2) Armature reaction in generator and converter, and drop of potential difference due to current into resistance. (3) Phase difference between the current and potential difference observed across the terminals 1, 3 in the one circuit, and current and potential difference across 2, 4 in the other circuit. (4) The power to be transmitted between generator and converter. Starting with the curve of potential difference between the brushes 1, 3 or 2, 4, we see that current may lag or lead by relative variation of excitation. As the excitation of the converter is increased the current is accelerated, and by pushing this far enough for a given power to be transmitted, it follows that the lead of current becomes so great that the converter is unable to do the work required of it, and it falls out of step. But there is another influence at work—namely, armature reaction and the potential difference due to current into resistance. Take the potential difference between the brushes 1, 3, or 2, 4 at any moment, the electromotive force of the converter will be this potential difference corrected for potential difference due to current into resistance of the armature coils. Now this electromotive force differs from the electromotive force which would be observed on open circuit by an amount due to armature reaction. What happens, then, is this :—The converter armature lags mechanically behind that of the generator, although synchronising with it, such lag depending upon the current passing, its magnitude and phase, and armature reaction. We find, therefore, that when running as in experiments 1 and 2, for instance, a certain power must be transmitted to the converter. If we increase the converter field, the current is immediately accelerated by a certain amount, the converter armature takes up a new position with regard to phase, lagging to a greater extent, but only until equilibrium is produced as between applied potential difference corrected for fall of potential due to current in armature resistance and the true E.M.F., which is again influenced by armature reaction. In the limit the current leads the potential difference to such an extent that the average power transmitted is smaller than that demanded, and the converter falls out of step.

The effects of local currents in the magnet windings and

cores of this converter come in as a part of armature reaction, and this subject has already received special attention in the case of single-phase alternate current dynamos.[1] In experiment 1, Fig. 3, curve B shows the variation of potential difference between the main brushes 7, 8, Fig. 2; the reading on the Weston voltmeter during the observations for this curve was 99 volts. The curve C shows the variation of potential difference between the terminals of the exciting coil on the converter magnet. This coil consists of 3,968 turns of resistance 26·6 ohms, and was supplied with current

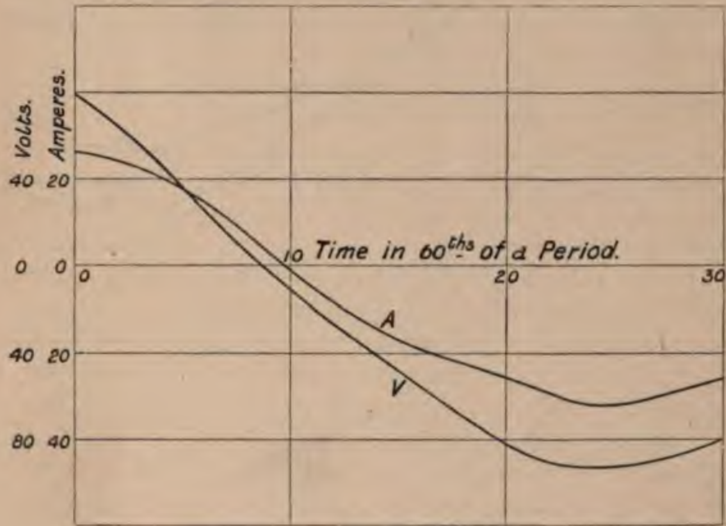

FIG. 6.

from a set of 57 storage cells, a non-inductive resistance being included in the circuit for the purpose of regulation. We see, as we should expect from the experiments on single-phase alternators,[1] that the periodic time of the variation is half that of the alternate current machine.

2. Rotary converter loaded non-inductively between brushes 7 and 8, Fig. 2. In experiment 5, Fig. 6, the excitations in A, B, and C are the same very nearly as in experiment 1, but the converter is loaded with 2,320 watts. We see that the phase difference between current and potential difference is reduced from a lag of 72° to a lag

[1] See *Phil. Trans. Roy. Soc.*, vol. clxxxvii. (1896), A. pp. 229–252.

of 6°, the potential difference of the converter is reduced to 93 volts on the direct current side, and the actual displace-

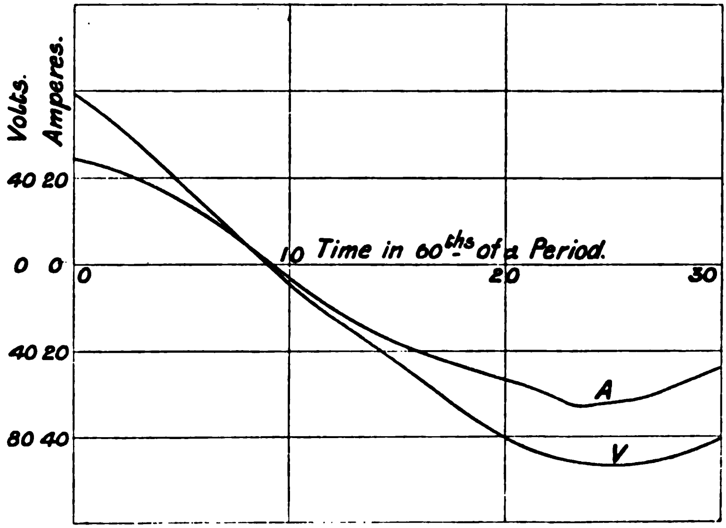

FIG. 7.

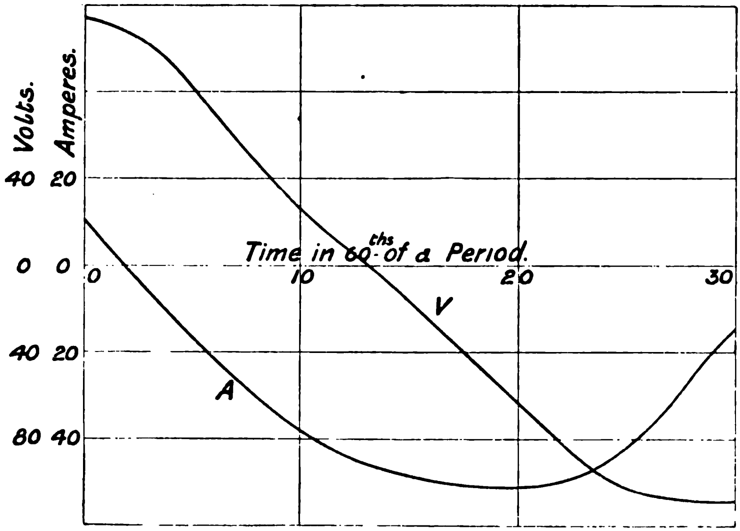

FIG. 8.

ment of the converter armature is increased to 25·2° from 19·8° in experiment 1. In experiment 6, Fig. 7, the excita-

tion of the converter is unchanged, but the alternate current generators have reduced excitation. The result is slightly to decrease the displacement of the armature to 24·6°, and to bring the current and potential into phase. With this load on the converter and with these excitations we have

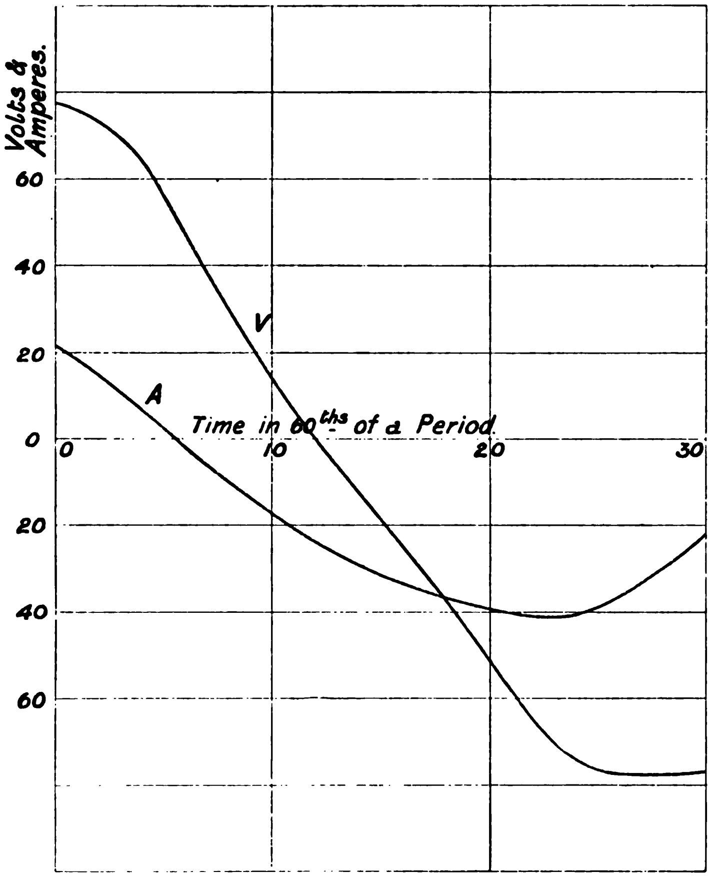

FIG. 9.

maximum efficiency with regard to the intermediate conductors, and the potential difference of the converter is reduced to 91 volts. (Experiments 8 and 9 show the same result, and are simply given again in confirmation.) To maintain a maximum power factor the converter excitation should be *de*creased as the load is *in*creased, or the

generator excitation should be *in*creased as the load is *in*creased, or both variations could be carried on simultaneously. Series coils carrying the main current, or currents proportional thereto, could be used for this purpose. We now come to experiment 7, Fig. 8. In this the converter excitation is increased. This of course increases the potential difference on the direct current side of the converter, and the current was reduced just to give stability, that is to keep the converter from falling out of step. The effect is to give a considerable lead (67·2°) to the current and to increase the displacement of the converter armature to 20·4°. The converter ran out of step when its exciting current was increased to 2·35 amperes. It is interesting to compare this experiment with No. 10, Fig. 9, in which the limiting condition is nearly reached, but by decreasing the generator excitation instead of increasing that of the converter. In the latter experiment the lead of current is 40·8°, and on further decreasing the Alternator exciting current the converter fell out of step. We notice that in this experiment the displacement of the armature is 33·6°, whereas with the stronger field (experiment 7) it is only 20·4°. We may say that loading this particular converter has the effect of increasing mechanical displacement of the armature, since greater armature reaction results; further, that as the excitation of the converter is increased, that of the generator being kept constant or the converter excitation being constant, that of the generator is decreased, the current is accelerated in phase with regard to applied potential difference, this goes on until current and potential difference are in phase, after this the current must be increased if the same power is to be transmitted, consequently greater armature reaction and loss due to current into resistance, and ultimately the converter falls out of step.

The mechanical phase displacement of the armature of a rotary converter due to armature reaction and fall of potential in the armature conductors, has practical importance. Fluctuations in the load on the direct current side of the converter, for instance, will be accompanied by tendencies to accelerate the armature positively or negatively. Imagine two armatures in parallel capable of independent motion. Their respective armature reactions might be

the cause of an interaction, if once started, such that the armatures are alternately positively and negatively accelerated; a positive acceleration in the one being coincident with a negative acceleration in the other. The precaution to be taken in order to render this smaller is to have small armature resistance and strong magnetic field intensity so as to reduce armature reaction. Since

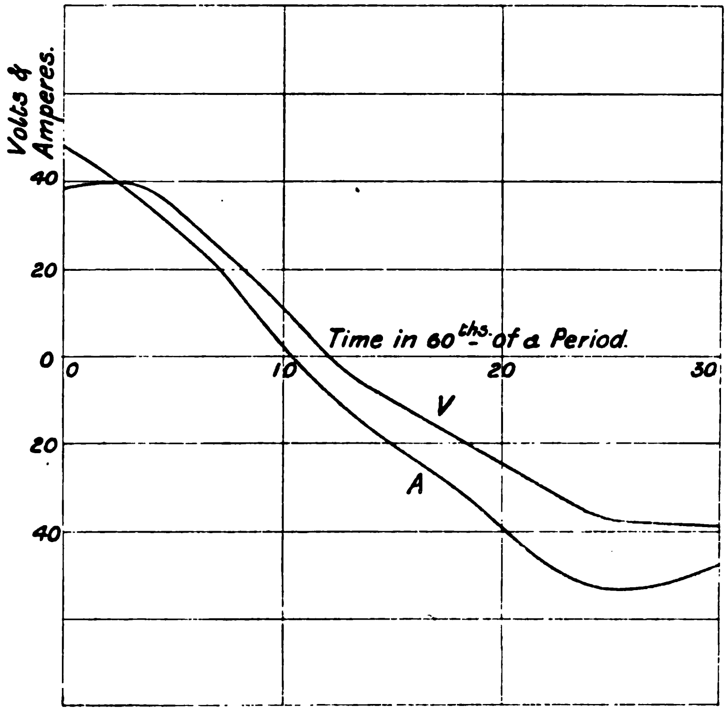

FIG. 10.

armature reaction really amounts to a variation of magnetic induction, it is obvious that conductivity in the magnet cores, or a closed conductor of low resistance round the magnet limb near the armature,[1] will tend to annul such variation and result in greater stability, but decreased efficiency. In order that the effect of compounding coils may be quickly felt, the cores should be

[1] This is a well-known device. See Thompson, *Journal Inst. Elec. Eng.*, November, 1898.

laminated. It has been observed[1] that the reaction brought about by the armature inducing currents in the magnets varies within limits as the square of the armature current. It is possible in a machine provided with considerable conductivity near the pole-piece that the periodic effect might diminish in amplitude with increased load. Increased armature inertia is valuable if sudden fluctuations of load have to be dealt with, the time of the fluctuations being

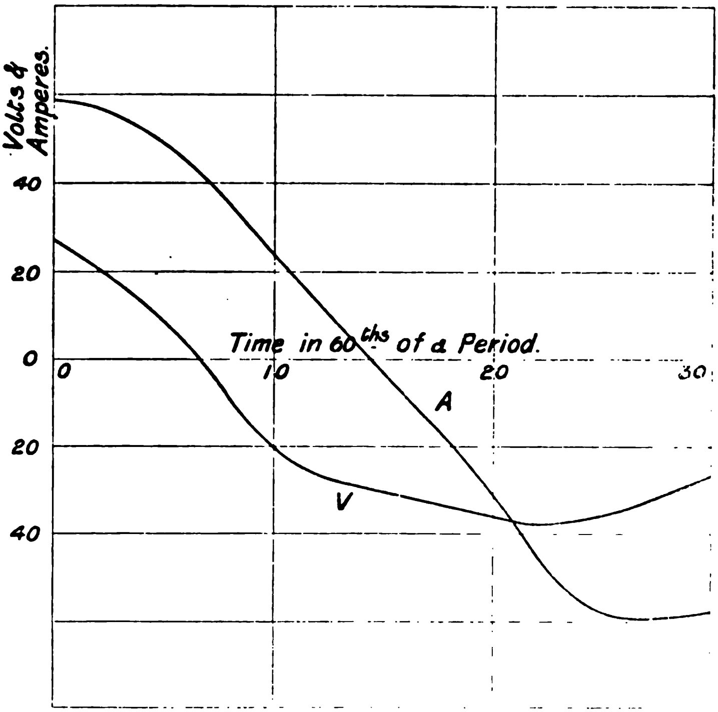

FIG. 11.

small. But equally increased inertia might only increase the periodic time of the interaction between two or more parallel armatures, if once started. Dissimilar inertiæ in the different armatures in parallel might prove beneficial in reducing periodic inter-action.

In a loaded rotary converter, if it is required to deduce the E.M.F. on load from the applied potential difference,

[1] See *Phil. Trans. Roy. Soc.*, vol. clxxxvii. (1896), p. 235.

this latter curve must be corrected for drop of potential due to current into resistance of the armature coils. We have carefully to observe (*a*) the displacement of the armature, (*b*) the phase of current with regard to potential difference actually applied, (*c*) the direction and magnitude of the various currents in the armature conductors the resistance of which is known. By direct experiment it was found that the E.M.F. of alternator B with no current in its armature crosses the axis at 7·5° on its contact-maker scale. In experiment 6, Fig. 7, current and potential difference are in phase. The main brushes 7, 8 carry 24 amperes as shown by a Weston amperemeter, but the load being non-inductive it will vary as the potential difference between the brushes 7, 8, Fig. 2. The curves in Fig. 7 show that current and potential difference are each very nearly zero at position 9 on the alternate current generator contact-maker scale which is plotted horizontally. The consequence is that the maximum current does not occur on the alternate current side of the converter when the main brushes 7, 8 touch the same commutator plates as the rings 1, 3, but maximum current occurs when such commutator plates lag 24 degrees behind the main brushes 7, 8. As a matter of fact, when the brushes 7, 8 *do* touch the same commutator plates as the rings 1, 3, the alternate current generator is delivering 28 amperes, and the other phase, instead of being zero, is delivering 13 amperes. That there must be an excess is obvious, since the power taken to drive the converter has to come from the alternate current generator. In this converter armature the resistance between the brushes 7, 8, Fig. 2, is ·042 ohm at about 15° C; with the currents dealt with the displacement of the armature is almost wholly due to armature reaction. With the generator and converter excited as in experiment 5, but loaded with 11·5 amperes on the direct current side, the potential difference between brushes 7, 8 varied similarly to that shown in curve B, experiment 1, the periodic time being half that of the converter armature. The variation in this case amounted to 11 per cent. on each side of the mean value.

In experiment 11 the excitation of the converter is decreased, that of the generator being such that phase difference 9° exists between current and potential difference. In experiment 12 we have the converter working with no

exciting current externally applied. In this case the brushes sparked on the direct current side of the converter and the phase-difference is 50°, the current lagging. The method of observing the displacement of the converter armature allowed one to examine the fluctuation, if any, in such displacement at one point of a revolution. In all cases except the limiting experiments 7 and 10, the displacement

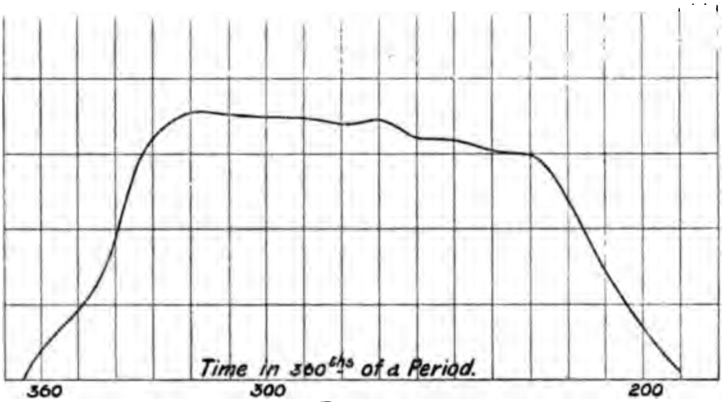

FIG. 13.

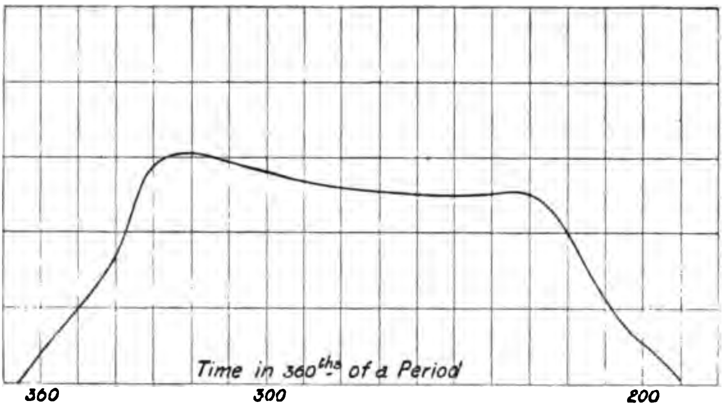

FIG. 14.

was constant under constant load. In experiments 7 and 10 the displacement did not vary more than about 3° on either side of its mean value. Throughout the experiments the main brushes, which are of copper, were fixed on the neutral line and not moved therefrom. To avoid complication, the author has not shown the curves obtained from the circuit of the C alternate current generator. In any

experiment they are the same as in the A machine, but displaced 90°.

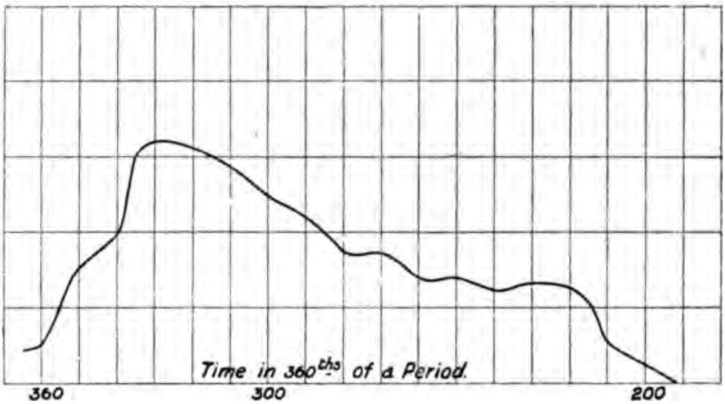

FIG. 15.

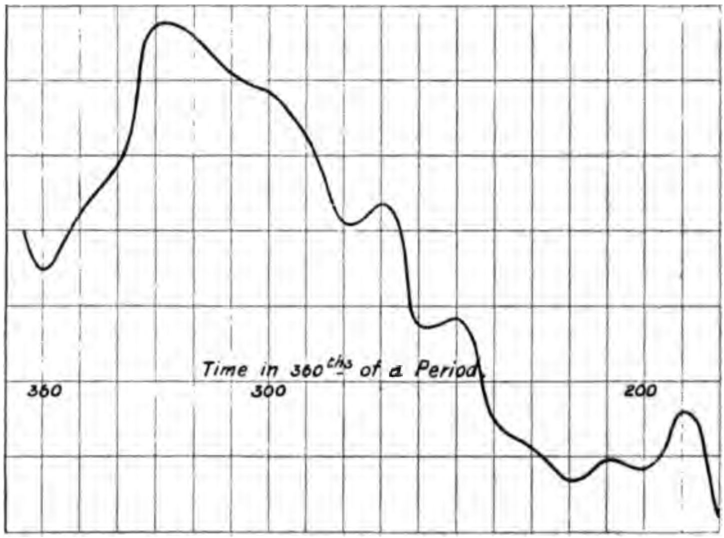

FIG. 16.

DISTURBANCE OF INDUCTION OVER THE BORED FACE OF THE CONVERTER.

The distribution of induction over the bored face of the converter has been observed by noting the potential difference between two small brushes (9, 10, Fig. 2) touching

adjacent commutator plates for different positions round a half circumference. When these brushes are on the neutral line, the reading on the scale is 4°. The commutator at this position is supplied with extra wide insulation material, so that the brush does not short circuit the commutator plates when passing from one to the other. These potential differences were observed on a Kelvin quadrant electrometer. In

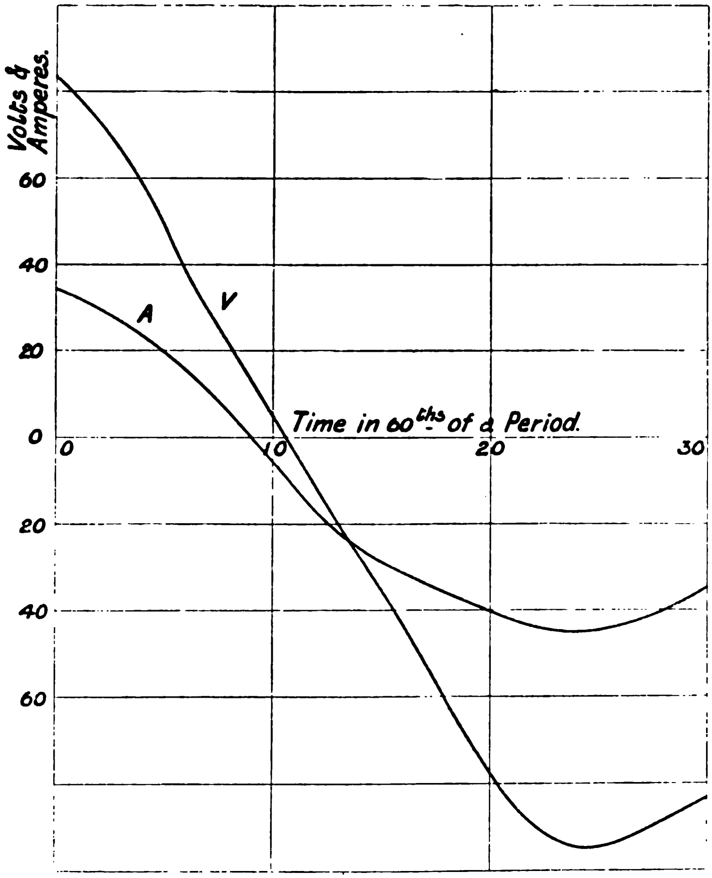

FIG. 17.

the diagrams, time and direction of rotation progress from left to right. It must be remembered that the brushes 9 and 10 give an average effect. In order to investigate what actually takes place between particular adjacent segments at different epochs of a period a contact-maker supplied with two

bars only would be necessary, such bars touching the desired adjacent segments. In Fig. 13 we have just the usual form of curve observed in this same machine when running as a direct current motor.[1] In Fig. 14 we have the converter just before falling out of step. The curve dips at the centre. Fig. 15, experiment 11, shows a marked effect; the converter field is weaker. Fig. 16, experiment 12, is interesting, since the excitation is due to the armature current only. We see that the average field is totally reversed over the leading portion of the surface. Comparing Figs. 15 and 16 with one another, we see that at angles 50° and 70° each curve rises and then falls. These effects are due to the armature currents. It is therefore with weak fields for a

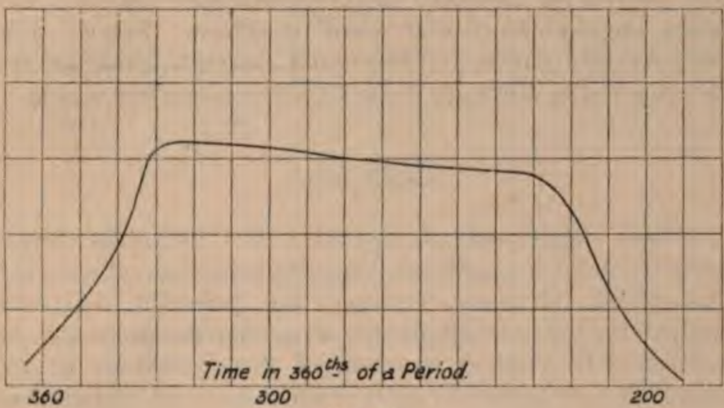

FIG. 18.

given current that violent disturbances take place in the air space of a rotary converter. When working under normal conditions the exciting current in this machine is 5·3 amperes, with which corresponding field the converter armature currents would have to be excessive to cause such disturbances as above observed in Figs. 15 and 16.

In this paper the converter has been worked under abnormal conditions in order to accentuate the effects. The efficiencies, for instance, must not be taken to be what would have been obtained with strong fields and full output in watts. The watts per revolution when this

[1] See *Proc. Roy. Soc.*, vol. li. p. 49; also *Electrician*, vol. xxviii. p. 609; also *Dynamo Machinery*, by Dr. J. Hopkinson, pp. 143–145.

converter is working full load under normal conditions are 12·7.

SINGLE-PHASE EXPERIMENTS.

When the converter was in action single-phase, the brushes 7 and 8 which were on the neutral axis sparked all through. A set of curves is given in Fig. 17, experiment 13, in which the phase difference between current and potential difference is 8°. The converter excitation is 1·25 amperes, and that of the generator 25 amperes. On reducing the latter the converter fell out of step at 18 amperes, whereas in experiment 10 we saw that the generator excitation could be reduced to 12·5 amperes, the converter still keeping in step. The load on the converter direct current side was about half when single-phase of that when two-phase. Fig. 18 gives the field disturbance, and this is not excessive, since for the currents in the armature the field is comparatively strong.

CONCLUSION.

These experiments demonstrate in a two-phase rotary converter used to transform from alternate to direct currents, that phase difference between the potential difference applied to, and current in, the converter armature can be controlled by relative variation of the excitations of the converter and generator, and is influenced by variation of load on the converter: and that maximum economy with regard to the intermediate conductors can be secured by reducing this phase difference to zero.

It is shown that, although synchronising with the alternate current generator, the converter armature lags mechanically behind that of the generator by an amount depending upon armature reaction and current in the converter armature. The experiments suggest an explanation of and means for reducing what has been termed "hunting" of rotary converters.

The disturbance of distribution of induction in the air space of the converter is examined under different conditions, and it is shown that when the converter runs, without excitation due to current in a coil wound on its magnet limb, the armature currents provide a magnetising

No. of Expt.	Figs.	$\sqrt{\text{mean}^2}$ Volts across one phase.	$\sqrt{\text{mean}^2}$ Amperes in one phase.	Average Watts from Curves per phase.	Phase-difference between resultant Volts and Amperes.	Volts across Converter Main Brushes.	Amperes in Converter Main Circuit.	Watts delivered by Converter.	Efficiency of Converter Armature.	Exciting Current in Alternators in Amperes.	Alternator Armature Connections.	Exciting Current in Converter in Amperes.	Speed of Converter in Revolutions per minute.	Phase-difference between Armatures. 360° = 1 period.	Displacement of Generator Potential Difference from zero on open circuit.	Phase-displacement of Converter Armature from zero Potential Difference applied.	Remarks.
1	3	74·4	19	452	ct. lags 72°	105	0	0	...	25	6 series 2 parallel	1·25	1410	12°	− 7·8	19·8	Fig. 1 gives 88 volts max. $\sqrt{\text{mean}^2}$ 62, that is with no current in armature
2	4	104	22	390	ct. leads 77°	150	0	0	...	24	"	2·35	1410	33°	+ 11·4	21·6	Fig. 1 gives 166 volts max. $\sqrt{\text{mean}^2}$ 117, that is with no current in armature
3	5	40	12	360	ct. lags 35°	55	0	0	...	25	3 series 4 parallel	0·674	1427	...	− 3·0	...	
4	...	27abt.	40	...	...	38	0	0	...	24	"	0	1400	...	...	...	Main brushes (7, 8, Fig. 2) sparked.
5	6 & 13	65	23	1430	ct. lags 6°	93	25	2320	81%	24	6 series 2 parallel	1·2	1380	33°	+ 7·8	25·2	
6	7	63·5	22·5	1491	0°	91	24	2180	73%	21	"	1·2	1380	33°	+ 8·4	24·6	
7	8	78	38	1190	ct. leads 67·2°	112	15·5	1740	73%	23·5	"	2·1	1280	54°	+ 33·6	20·4	Fig. 1 gives 137·5 volts max. $\sqrt{\text{mean}^2}$ 97·2, that is with no current in armature. Generator B (Fig. 2) gives 85·7 $\sqrt{\text{mean}^2}$ volts on open circuit.
8	13	64	21	1570	ct. lags 12°	92	24·5	2254	72%	25	"	1·27	1368	30°	...	...	Generator B gave 96 volts $\sqrt{\text{mean}^2}$ at 232 revs. per minute on open circuit. Frequency 23·2.
9	...	63	19·5	1228	0°	88	23·7	2085	84%	20	"	1·25	1370	...	...	...	
10	9 & 14	52	25	1183	ct. leads 40·8°	73	19·5	1424	60%	12·5	"	1·27	1370	60°	+ 26·4	33·6	Generator B on open circuit gave 68 volts $\sqrt{\text{mean}^2}$ at 229 revolutions per minute. Frequency 22·9.
11	10 & 15	27	37	998	ct. leads 9°	34	33	1122	56%	12·5	"	0·704	1360	...	+ 27·0	...	
12	11 & 16	25	43	790	ct. lags 50°	30	10	300	19%	24	3 series 4 parallel	0	1390	...	− 5·4	...	Main brushes (7, 8, Fig. 2) sparked.
13	17 & 18	61	30	2010	ct. leads 8°	85·5	11·6	992	49%	25	6 series 2 parallel	1·25	1380	...	+ 9·0	...	" " "

force such that the direction of the average induction is reversed over a portion of the air space. The effects of armature reaction show themselves at the extremities of the magnetising coil on the limb of the converter. The periodic time of this disturbance is half that of the converter armature. The variation of potential difference between the main brushes on what may be called the uni-directional current side of the converter has also a periodic time half that of the converter armature.

When the converter is run on a single-phase, its exciting current cannot be varied through such wide limits and the converter still keep in step, as is the case when two-phase currents are employed, although the load was only half when single-phase of that when two-phase. When single-phase, the converter is not so satisfactory as regards sparking at the brushes on the direct current side.

In conclusion, I beg to thank Messrs. Renfree and Poole, Student Demonstrators in the Siemens Laboratory, King's College, London. These gentlemen have given me great assistance in the experiments. Mr. Renfree worked out the results of the experiments, and Mr. Poole has made the drawings.

THE EFFECT OF GOVERNORS ON THE PARALLEL RUNNING OF ALTERNATORS.[1]

By LEONARD WILSON, Student.

THE question of the effect of governors on the parallel running of steam alternators is only a small part of the general problem of parallel running, but at the present time it is a question of some importance, and it is the object of this paper to show that in the few cases of modern steam-alternators refusing to operate successfully in parallel, the failure may be ascribed in great measure to the effect of the engine governors. It may be as well before going into details briefly to review the behaviour of alternators when switched in parallel. As an example, take the case of a well-equipped central station with one or more alternators already running on the 'bus bars, fairly well loaded ; and suppose it is desired to put in another alternator. The incoming alternator is first run up to speed, and its field excited until the armature voltage is equal to that of the 'bus bars, it is then connected up to some synchronizer device which indicates when the incoming alternator is in step with the others, and at the right moment it is switched on to the 'bus bars. If this operation is conducted skilfully, there will be no disturbance of the other alternators, and the fresh one will continue running with practically no current through its armature. In order to make it take up its share of the load, its field should first be strengthened proportionately to the load the alternator has to receive, and then the throttle or expansion valve opened up (either by means of the governor or otherwise) until the plant is doing the required load. It must here be remarked that the strengthening of the alternator field does not make the alternator take up any of the load, although it may cause a considerable wattless current to circulate.

With suitable plant, and a good man at the switchboard, all these operations will be carried out without producing even the slightest fluctuation of the station voltage. If, however, the incoming alternator be switched on to the 'bus bars when it is not in the right phase with the others, a disturbance of the current distribution will take place. At

[1] Paper read before the Students' Section of the Institution, January 18, 1899.

the moment of switching in, the ammeter needle of the fresh alternator will swing over, showing a big rush of current through the armature. It will rapidly move back to zero, and continue swinging backwards and forwards across the scale for some seconds or minutes until the vibration is entirely damped out, and the needle settles down at the zero end of the scale. This disturbance of the ammeter is due to the phasing currents necessary to bring the alternators into step, and the effect may be observed on the ammeters of the other alternators, and to a certain extent on the common voltmeter. The foregoing briefly describes what happens in the majority of cases when alternators are switched in parallel. In the exceptional case of some direct coupled plants, the phenomena differ in one important point, and that is, that when the current rushes are once set up between two machines, instead of gradually dying away they go on steadily increasing, each rush of current being a little greater than the previous one. It is extremely interesting to watch the ammeter of such an alternator. If the alternator be switched on to the 'bus bars very carefully, the ammeter will remain quiet for some time, perhaps even for several minutes, but sooner or later the needle will start swinging backwards and forwards with ever increasing amplitude, until at last the rushes of current are so big that the fuses go, or the machines have to be switched out. This phenomenon has been observed in more than one English central station, and unless some cure is found, parallel running in that station is out of the question. The explanation of this behaviour is not at first sight obvious, and no attempt has been made in any of the current technical literature to investigate the theory of the action, although, of course, the practical man knows the necessary remedies to apply.

In the theoretical treatment of alternator problems the most important electrical quantity is the armature re-action, and this needs some little explanation. The effect of the armature ampere-turns in an alternator, like that of the secondary ampere-turns in a transformer, is twofold. It sets up a certain magnetic flux which is interlinked with both the armature- and field-turns, and a second, greater and much more important flux which does not go round the whole magnetic circuit of the alternator, but is interlinked

with the armature-turns only. This latter, the leakage flux, is very easy to deal with, as it is simply the self-induction of the armature coils, and *can be replaced* by an inductance outside the armature. The first mentioned flux is in some cases not so easy to manage, but in coreless armatures like that of the Mordey alternator its value is very small, and, better still, its effect is almost identical with that of armature self-induction, so that it may also be replaced by a simple inductance in series with the armature.

The remaining factor of importance is the armature resistance, and this may be replaced by a simple resistance in series with the "perfect" armature. To be more accurate, this resistance should be a little greater than the ohmic resistance of the armature in order also to replace the energy component of the eddy currents set up by the armature. The alternator is now in a much handier form for calculating purposes; it consists of a "perfect" armature which generates an E.M.F. directly proportional to the field, being independent of armature current, and this E.M.F. is always in phase with the field. In series with this "perfect" armature is an inductive circuit consisting of a simple resistance and inductance, which modifies the phase and magnitude of the terminal voltage in exactly the same way as the armature re-actions would. The practical way to calculate this external resistance and inductance is to run the alternator at constant speed, constant excitation, on a variable non-inductive load, and measure the terminal volts with various armature currents. The same measurements should then be taken with inductive loads of a known power-factor, which should be as low as possible. From these readings the equivalent resistance and inductance which will replace the armature re-actions may be deduced, and also these readings will show whether such a substitution is legitimate. In an iron-cored armature such a simple resistance and inductance cannot be found, but in most coreless armatures, of the Mordey and Ferranti type, the method will be found fairly accurate; and it is only to this type of alternators that the present paper refers, although it will be seen that the same general principles apply to any other type.

One further and very important simplification is necessary before dealing with two alternators in parallel. We

have got in series with the "perfect" armature an inductive circuit of a certain impedance and power factor, and this consists of a resistance in series with an inductance. Now this arrangement may be replaced by an equivalent resistance and inductance in parallel with one another, the condition being that the new combination shall have the same apparent resistance (*i.e.*, impedance) as the former, and shall produce the same lag of current behind E.M.F. From these conditions the following connection between the old and new resistance and inductance is found :—

$$L_P = \frac{R^2 + L^2}{L} \text{ and } R_P = \frac{R^2 + L^2}{R},$$

where R and L are the resistance and inductance for series

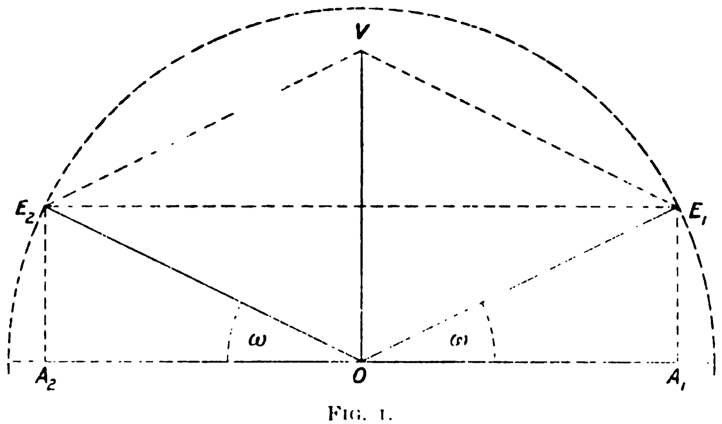

FIG. 1.

connexion, and R_P and L_P are the resistance and inductance for parallel connexion.

It must be noted that the alternator is reduced now to a "perfect" armature, with a parallel connexion of resistance and inductance in series with it. With this arrangement it is a very simple matter to calculate the power which circulates between two alternators when they are any degree out of phase. Take the simple case of two similar alternators running in parallel unloaded, and a certain amount out of phase.

The first part of the problem is to find the E.M.F.

which causes a current to circulate round the two armatures, and to find the phase of this E.M.F. This is shown in Fig. 1, where $O E_1$ is E.M.F. of No. 1 alternator, which lags by an angle ω behind the mean or 'bus bar potential $O A_1$, and $O E_2$ is E.M.F. of No. 2 leading by an equal angle in front of the 'bus bar potential $O A_2$. The difference between or resultant of $O E_1$ and $O E_2$ is O V, and it will be seen that what whatever the angle of phase difference, this resultant O V is always at right angles to the 'bus bar potential. The value of O V is $2 E \sin \omega$.

Now O V is the E.M.F. which causes a current to circulate round the two "perfect" armatures and their attached impedances, and this current may be split up into two parts.

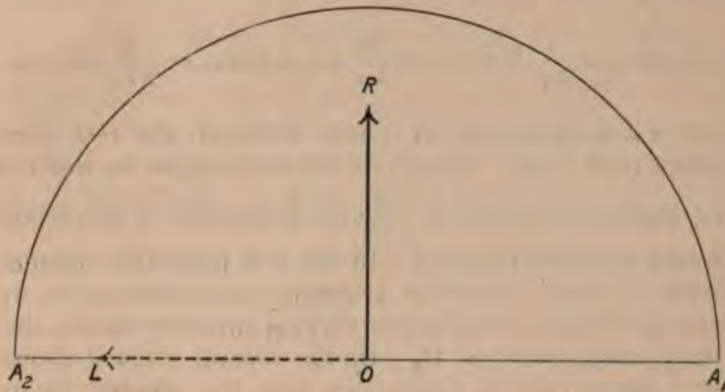

FIG. 2.

One part which flows through the non-inductive resistances will be in the same phase as the E.M.F., O V, and the other part which flows through the inductances will lag 90° behind the E.M.F., O V. These two currents are shown in Fig. 2 in full and dotted lines. On comparing Figs. 1 and 2 it will be observed that the current shown by the full line, which is the current through the non-inductive resistances, is at right angles to the 'bus bar potential, and therefore it does not represent any circulation of energy between the two alternators, being simply an idle current producing C^2R losses, but exerting no torque. In the present problem, therefore, it is of no account. The other part of the circulating current, which is that flowing through the inductances (dotted line), is in phase with the 'bus bar

potential, being in the same direction as No. 2 alternator and in opposite direction to No. 1. This current, therefore, represents the true interchange of energy between the two armatures, No. 2 alternator generating (as the current is in phase with the E.M.F.), and No. 1 motoring (as the current is in opposition to the E.M.F.).

The actual energy output of No. 2 is the current O L × 'bus bar potential difference OA_2. The current O L, is the current caused to flow through the inductance $L_P + L_P$ by the E.M.F., O V ($=2\,E\sin\omega$), and is therefore $= \frac{2\,E\sin\omega}{2\,L_P} = \frac{1}{L_P}\,E\sin\omega$. The 'bus bar potential difference OA_2 is $= E\cos\omega$, and therefore the energy output of No. 2 is,

$$E\cos\omega \times \frac{1}{L_P}\,E\sin\omega = \frac{E}{L_P}\sin\omega\cos\omega = \frac{E}{2\,L_P}\sin 2\,\omega,$$

and $2\,\omega$ = difference in phase between the two alternators (call it δ). Therefore the real output in watts of the leading alternator is $\frac{E}{2\,L_P}\sin\delta$, and this is the fundamental equation required. In the first place this equation shows in what way the armature re-actions affect the phasing torque, this torque varying inversely as L_P and being independent of R_P. In the second place it shows, for a given pair of alternators, how the phasing torque varies with the angle of phase difference. The words "torque" and "power" are for the present taken as synonymous, as with constant speed the torque varies directly as the kilowatts. The above equation therefore may be reduced to the following form for two given alternators, viz. :—

$$\text{Phasing torque} \propto \sin\delta.$$

It will at once strike one that this is the same as the equation for the force acting on a simple pendulum, and it is useful to follow out the analogy. Suppose No. 1 alternator is lagging, both alternators revolving at the same speed, the equation shows that a torque will be set up tending to increase the speed, and gradually bring No. 1 into phase with No. 2. By the time the two are in step,

No. 1 is revolving at a considerably higher speed than No. 2, and consequently begins to lead in phase. This sets up a retarding torque on No. 1 which equalises the speed, but not before No. 1 is considerably ahead in phase. We have now got the two alternators once more revolving at the same speed, but No. 2 is lagging. The process therefore repeats itself, the alternators passing and repassing one another in phase until the swinging action is entirely damped out by friction, and similar forces. It must be noted that the moment when the two alternators revolve at equal speeds coincides with the moment of maximum phase difference, and the moment when the two alternators are in the same phase coincides with the moment of maximum difference of speed. That is to say, the difference of energy between the two alternators continually changes from kinetic to potential and back again to kinetic in precisely analogous manner to the change of energy in a pendulum.

The relation between torque and phase angle being known, and also the moment of inertia of the revolving parts, it is possible to calculate the time taken for a complete double swing of the two alternators. To be more accurate it should be added that this problem cannot be completely solved, but as in the case of the pendulum it can be approximated to. For small phase displacement the time taken for a complete swing is :—

$$\text{T (in secs.)} = \frac{15\sqrt{\sim \text{I} \, \text{L}_{\text{P}}}}{\text{P E}}$$

P = No. of like poles.
I = Moment of inertia.

There is no need to go into the calculation of this formula, as it is simply a matter of more or less elementary mathematics; the question of more immediate interest being to what extent it agrees with practice.

It must not be expected that the agreement will be at all close, as, apart from inaccurate assumptions, the quantity L_P is very difficult to obtain with any degree of accuracy. In order to obtain by experiment the time taken for a complete phasing swing, it is necessary that the two alternators should be driven by a perfectly steady motor or turbine, and this means a very elaborate experiment for which

there is no return (commercially). However, in a rough way it is possible to approximate to the true value by taking the time of a swing of the ammeter between two alternators driven by steam engines. The author has done this in two different cases. In one case the calculated value was only 10 per cent. different from the observed, and in the other case it was 50 per cent.; in both cases the calculated result was less than the observed. This agreement is sufficient to give some support to the foregoing theory. It remains to show the effect of this swinging action on the governors.

For present purposes it may be taken that the function of a governor is to keep the speed of the engine within certain limits for all loads, the regulation being effected by varying the steam pressure in the engine cylinder. At the lower limit of speed the centrifugal force on the governor weights is not sufficient to move them out against the controlling springs, and the steam pressure in the cylinders is a maximum. At a little higher speed the weights move out to a position where the greater centrifugal force is again balanced by the extended springs. As the weights move out they operate some valve mechanism which reduces the mean pressure in the cylinders, and when a certain speed is reached the weights have moved out to the full possible extent, and at this position the steam is entirely cut off. It will be seen, therefore, that under steady conditions a definite speed means a certain definite steam pressure in the cylinders; the difference between the speed for maximum steam pressure and that for minimum steam pressure being called the "drop" of the governor. This quantity varies largely for different types of governor, being as low as ½ per cent. in some cases, and frequently as high as 10 per cent. Now although a change of speed causes a change of steam pressure, it does not do this by any means instantaneously, as there is quite a complicated chain of events between the cause and effect, so that if the speed is suddenly increased, it will be some time before the steam pressure is correspondingly reduced. Suppose now that we have an ordinary governor detached from its engine and capable of being driven at any desired speed. Let two recording mechanisms be attached, one of which records the actual speed of the governor, and the other records what the steam pressure would be if the governor were

actually working with a steam engine. *E.g.*, say the "drop" of the governor is 3 per cent., and that a certain variation of speed takes half a second to cause the corresponding alteration in steam pressure. Now if the speed be *very slowly* varied up and down within the 3 per cent. limit, the two recording pencils will move together, the steam pressure rising as the speed falls and *vice versa*. If the speed is varied up and down sufficiently quickly it will be found that the steam pressure cannot keep time with it, and when the speed is a maximum the steam pressure has perhaps only just started falling, and is also therefore at its maximum. If the variations of speed occur much quicker than this, the steam pressure will not vary at all but will take up a mean position and remain at that. That is to say, there are three cases to be considered. (1) When the periodic variations of speed occur very slowly. In this case the alterations in steam pressure will exactly keep step with the alteration in speed. (2) When the periodic variations of speed occur fairly quickly, in which case the alteration in steam pressure will lag (sometimes as much as half a period) behind the speed variation; and (3) When the periodic variations of speed occur very rapidly, in which case the steam pressure will not have time to respond at all to the speed variations.

This latter is the case with most steam alternators when a swinging action is set up. The variations of speed are too small and too quick to effect a big "drop" slow-acting governor. If, however, the governor is very sensitive to changes of speed, and also quick to respond, the swinging action will have some effect and with a certain lag on the governor, the maximum steam pressure will be reached at maximum speed, and *vice versa* minimum steam pressure at minimum speed. In this case, therefore, the effect of the governor is indefinitely to increase the speed variation, and the swinging action instead of dying down will gather fresh force with each swing. It is this action which causes the difficulty described in the earlier part of the paper.

A quick-acting and sensitive governor is therefore to be avoided where steam alternators are to run in parallel. If a quick-acting governor must be used—and in every other way it is most advantageous—its sensitiveness must be decreased to such an extent that the small variation of

speed due to "swinging" of the alternators does not appreciably affect it. To take a particular case:—Suppose the speed variation during an ordinary swing amounts to $\frac{1}{5}$ per cent., and suppose the governor drop is 8 per cent. Then, 8 per cent. speed variation producing 100 per cent. alteration of steam pressure, therefore, by simple rule of three $\frac{1}{5}$ per cent. speed variation will produce $2\frac{1}{2}$ per cent. alteration in steam pressure, which would not be sufficient to produce any marked effect. Unfortunately, electrical engineers are not to be let off with simple rule of three. On more careful examination it may be found that the "drop" of the governor is not evenly distributed throughout the range of load, and probably the fact is that the drop from full to $\frac{3}{4}$ load is 7 per cent., and from $\frac{3}{4}$ load to no load it is only 1 per cent. In other words a governor may have a very big drop of speed from no load to full load, and still be extremely sensitive over some parts of its range, and this is a point that must always be borne in mind.

In any case of failure in parallel running, the first thing to try is to run the engines on the stop valves with the governors scotched, and unless the alternators are of extraordinarily bad design, it will be found that under these conditions there will be no trouble in running parallel. A great deal has been said about the effect of uneven turning moment on parallel running, one writer having gone so far as to say that it was absolutely impossible to run alternators in parallel if direct coupled to single-crank engines. This has been entirely disproved at Wandsworth, where Mordey alternators direct coupled to Raworth single-crank engines run in parallel without the least hitch. In this particular case the exceptionally well designed steam distribution maintains a much more even turning moment than in most single-crank engines; still, modern experience shows more and more clearly that far greater ill effects are due to the governors than to any small irregularities in the turning moment.

The Three Hundred and Thirtieth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, March 23rd, 1899—Mr. J. W. SWAN, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on March 9th, 1899, were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council :—

From the Class of Associates to that of Members—

Edward G. Campbell Barton.
Henry A. Blackwood Price.
John Peter Nelson.

From the class of Associates to that of Associate Members—

S. Andrews.
W. Corin.
A. C. Cormack.
A. H. F. FitzHerbert.
J. R. W. Gardam.
J. H. Garratt.
G. B. Garvey.
W. McGeoch, junr.
S. A. Mahood.
J. T. Morris.
A. E. Morrison.
W. L. Phillips.
John Pilling.
F. M. Rogers.
W. Routledge.
Sidney Smith.
W. F. Stuart-Menteth.
P. Trackson.
S. N. Wilson.

Donations to the Library were announced as having been received since the last meeting from Messrs. Clark, Forde, and Taylor, and Herr A. Hartleben, to whom the thanks of the meeting were duly accorded.

Messrs. C. C. F. Monckton and E. D. Phillips were appointed scrutineers of the ballot for the election of new members.

The PRESIDENT: I have to announce that the Council have to-night appointed Mr. J. L. W. V. Jensen to be Local Honorary Secretary and Treasurer for Denmark in place of the late Mr. P. Christian Dresing, whose death was reported at the last Meeting.

I now call upon Mrs. Ayrton to read her paper on "The Hissing of the Electric Arc."

THE HISSING OF THE ELECTRIC ARC.

By Mrs. AYRTON.

There are three ways in which any change that takes place in the electric arc may manifest itself: (1) by giving out sounds of various kinds or by becoming silent; (2) by changes in its electrical measurements; and (3) by an alteration in the appearance of the crater, the arc, and the carbons. Only two of the many and varied sounds given out by the *direct current open* arc seem to possess much significance—the hum and the hiss—and the causes of these are evidently connected with one another, for the hum never occurs except when the arc is on the point of hissing or has just been hissing, although it is quite possible to make an arc hiss and become silent again without any hum being heard either before or after.

It is proposed, in the present paper, to discuss the arc as it passes from silence to humming and from humming to hissing, but, as the changes that occur when the silent arc begins to hum make themselves perceptible to eye and ear only, and do not sensibly affect the electrical measurements, silent and humming arcs will be included under one head in the portion of the paper dealing with those measurements. It is, however, to the comparatively unexplored region of the direct current *hissing* arc that I desire particularly to direct your attention this evening.

Some of the electrical measurements of the arc burning between two *solid* carbons 11 mm. and 9 mm. in diameter are shown in Fig. 1, in which the curves connect the P.D. between the carbons with the current for several

OPEN ARC.

Solid Carbons

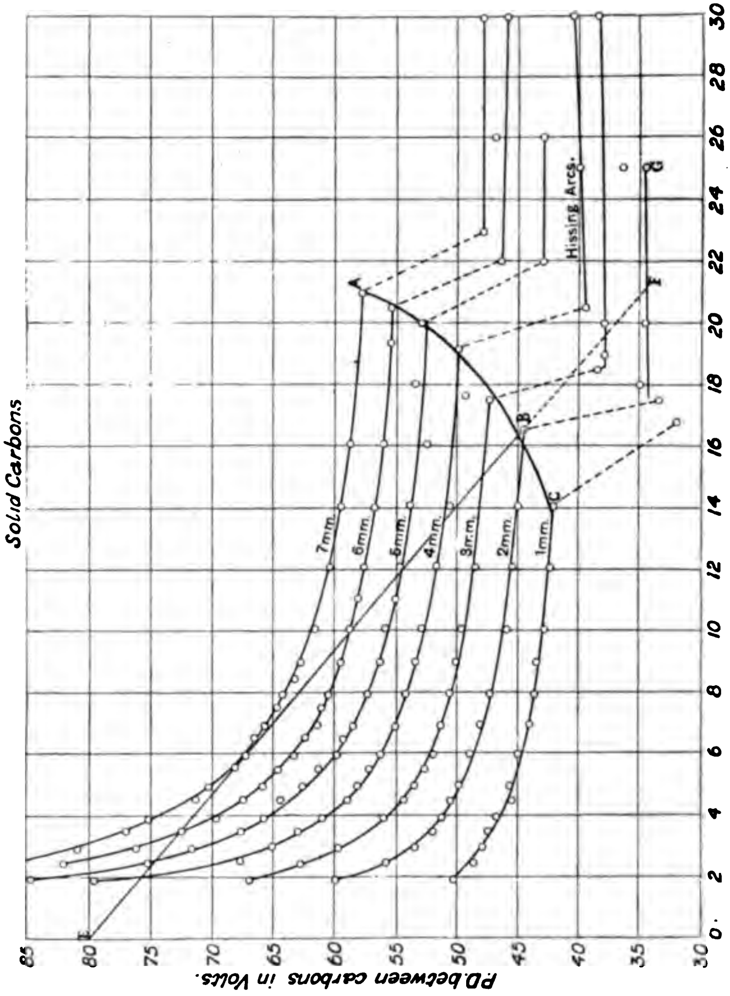

FIG. 1.—Curves connecting P.D. and Current for Constant Length of Arc.

CARBONS.

Positive, 11 mm.; Negative, 9 mm.

constant lengths of arc, both silent and hissing. It is important to bear in mind that before each observation was made the current and length of arc were kept *rigorously* constant for a sufficient length of time for the carbons to take their characteristic shape for that particular current and length of arc, and long enough, therefore, for the P.D. to have become constant also. Such an arc I propose to call a *normal* arc, as contrasted with one arrived at in a haphazard fashion by suddenly giving the current some particular value and the arc some particular length, and making observations without giving the carbons time to acquire their proper forms.

All the lines to the left of the curve A B C represent silent arcs, while immediately to the right of this curve are dotted lines, denoting a period when the arc is in the unstable condition that always divides the silent from the hissing arc; and still further to the right are the lines representing the hissing arc.

As we are only dealing with hissing and humming arcs, and with the silent arcs that are near hissing or humming, we need only discuss that part of Fig. 1 that is to the right of the line representing, say, 12 amperes, for that part includes all such arcs for each of the constant lengths.

An examination of these curves shows that with the carbons used, and with what I have called the *normal* arc, the following results are met with :—

(1) When the length of the arc is constant and the arc is silent, it may be made to hiss by increasing the current sufficiently.

(2) When the current is constant and the arc is silent, *shortening* the arc will make it hiss.

(3) When the arc begins to hiss, the P.D. suddenly falls about 10 volts and the current suddenly rises 2 or 3 amperes.

(4) The largest current that will maintain a *silent* arc is greater the longer the arc.

(5) For the hissing arc, the P.D. is constant for a given length of arc, whatever the current.

It was Niaudet[1] who, in 1881, first observed the fall of about 10 volts in the P.D. between the carbons at the moment that hissing began; and, although, perhaps, there is,

[1] *La Lumière Electrique*, 1881, vol. iii. p. 287.

OPEN ARC.

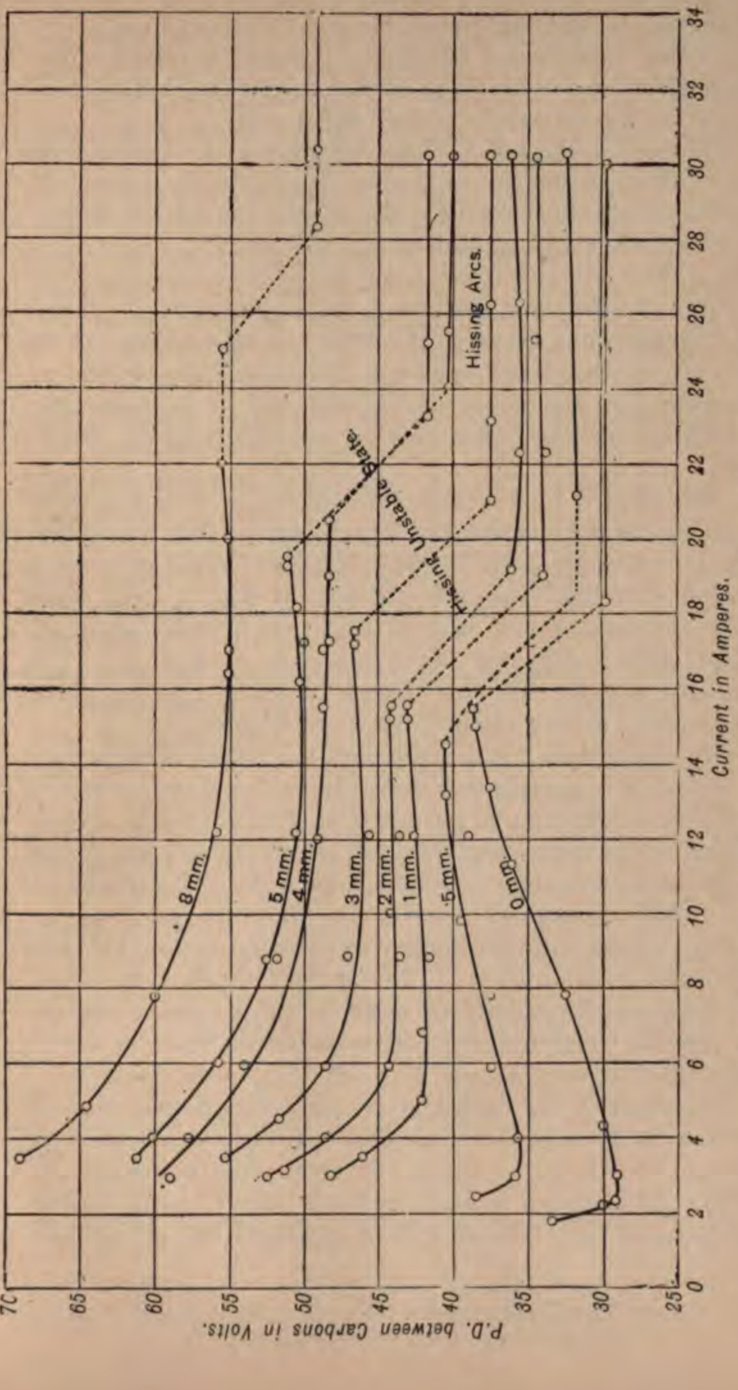

FIG. 2.—Curves connecting P.D. and Current for Constant Lengths of Arc.

CARBONS.

Positive, 9 mm. Cored. ; Negative, 8 mm. Solid.

even yet, a lingering notion that it is only when an arc is short that it can hiss, I find that as far back as 1889 Luggin [1] showed that however long an arc might be, it would still hiss were the current increased sufficiently.

At the Congress at Chicago in 1893 Prof. Ayrton [2] first drew attention to the region of instability, or rather, the region of blankness (notice the dotted portion of Figs. 1 and 2), corresponding with the impossibility of maintaining any *normal* arc with a particular range of current for each length. At the same time he pointed out in Fig. 2, shown at Chicago, that whether the P.D. was descending as the current increased for, say, a 4 mm. *silent* arc, or was ascending for, say, a 0·5 mm. *silent* arc, it became quite constant for wide variations of current with a *hissing* arc.

And, lastly, by a comparison of Fig. 2 with Fig. 3, he brought out the fact that the largest current that would flow silently with any given length of arc was increased by using thicker carbons. For the carbons in Fig. 3 have about twice the diameter of those in Fig. 2, and, while the largest silent current for, say, the 2 mm. arc in Fig. 2 is 15·5 amperes, that for the same length of arc in Fig. 3 is about 49 amperes, or more than three times as great.

Returning now to the subject of the dotted lines in Figs. 1, 2, and 3, it is plain that these divide the curves into two perfectly separate parts, governed by different laws. For to the left of the dotted part the lines are all curved, and curved differently according as solid or cored positive carbons are used, showing that with silent arcs the P.D. varies as the current varies, and that the law of variation is different with solid and cored carbons. To the right, on the other hand, the lines are all straight, and more or less parallel to the axis of current, whether the positive carbon is solid or cored, showing that with *hissing* arcs the P.D. is the same for a given length of arc and a given pair of carbons, *whatever* current is flowing, and that this law is true whether the carbons be cored or solid. In fact, some complete and sudden break-down appears to occur when hissing begins, upsetting all the laws that have governed the arc while it

[1] *Wien Sitzungsberichte*, 1889, vol. xcviii. p. 1192.
[2] *The Electrician*, 1895, vol. xxxiv. pp. 336–7.

OPEN ARC.

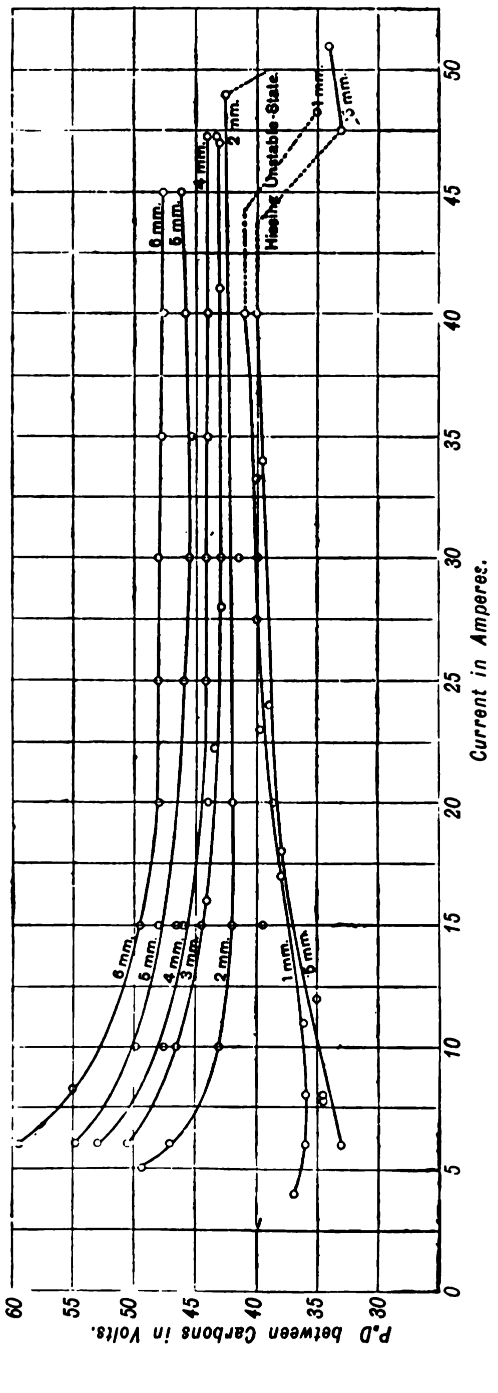

FIG 3.—Curves connecting P.D. and Current for Constant Lengths of Arc.

CARBONS.

Positive, 18 mm. Cored.; Negative, 15 mm. Solid.

was silent, and bringing the behaviour of cored and solid carbons into accord.

Thus, our subject divides itself quite naturally into two distinct portions, the one dealing with the arc when the break-down is imminent, but before it has actually occurred—dealing, that is to say, with the points at which the current is the largest that will flow silently—the *hissing points* as I shall call them; and the other dealing with the arc after the break-down has occurred, and when, therefore, the arc is really hissing.

On examination of Fig. 1, the points of which were obtained experimentally with much care, it is seen that the hissing points lie well on the curve A B C, the equation to which I have shown elsewhere[1] to be

$$V = 40{\cdot}05 + \frac{2{\cdot}91A - 29{\cdot}02}{10{\cdot}54 - 0{\cdot}416A} \; \ldots\ldots\ldots\ldots \quad (1)$$

where V is the P.D. between the carbons in volts, and A is the largest silent current in amperes. Or, expressing the P.D. in terms of l, the length of the arc in millimetres, instead of in terms of the current at the hissing points, we have

$$V = 40{\cdot}05 + 2{\cdot}49\,l \; \ldots\ldots\ldots\ldots \quad (2)$$

which shows that at the hissing points any given increase in the length of the arc causes an increase in the P.D. between the carbons that is simply proportional to the increase of length. That is to say, for every millimetre that is added to the length of the arc, 2·49 volts is added to the P.D. between the carbons at the hissing point.

From the above two equations I deduced the third, viz:

$$l = \frac{1{\cdot}17A - 11{\cdot}66}{10{\cdot}54 - 0{\cdot}416A} \; \ldots\ldots\ldots\ldots \quad (3)$$

and pointed out that, since l was infinite when

$$10{\cdot}54 - 0{\cdot}416A = 0, \text{ or when } A = 25{\cdot}3 \text{ amperes,}$$

no current greater than 25·3 amperes could maintain a normal *silent* arc, however long it might be made with the particular carbons used. Hence we may gather that *for each pair of carbons the current that will sustain a normal silent arc has a maximum value, and that any current greater than this will make the arc hiss, however long it may be.*

[1] *The Electrician*, 1896, vol. xxxvi. p. 541.

To turn, now, to the arc when hissing has actually begun. It has already been shown that *when the arc is hissing, the P.D. between the carbons is constant for any given length of arc, and is therefore independent of the current;* but no law has yet been given connecting the P.D. between the carbons with the length of the arc when hissing. This can now be found from Fig. 1, by plotting the mean P.D. between the carbons for each length of arc when it was hissing, with the corresponding lengths of arc. In this way we get a straight line, the equation to which is

$$V = 29{\cdot}25 + 2{\cdot}75\,l \ldots\ldots\ldots\ldots \qquad (4)$$

How far equation (4) really sums up the facts may be seen from Table I., which gives the mean value of the observed P.D. between the carbons for each length of hissing arc, the P.D. calculated from equation (4), and the difference between the two.

TABLE I.—HISSING ARCS.

MEAN P.D. BETWEEN CARBONS FOR DIFFERENT LENGTHS OF ARC COMPARED WITH SAME P.D. CALCULATED FROM EQUATION (4).

Carbons: Positive, 11 mm. Solid; Negative, 9 mm. Solid.

Length of Arc in Millimetres.	Mean P.D. between Carbons in Volts.	P.D. calculated from Equation (4).	Difference in Volts.
1	32	32·0	0
2	34·4	34·75	−0·35
3	37·8	37·5	+0·3
4	40·0	40·25	−0·25
5	43·0	43·0	0
6	46·5	45·75	+0·75
7	48·0	48·5	−0·5

Equation (4) shows that, *with the hissing as with the silent arc, a straight line law connects the P.D. between the carbons with the length of the arc.*

There is, however, this vast difference between the law for silent and that for hissing arcs, viz., that, for silent arcs, the law only holds for constant currents or for the currents at the hissing points, whereas for hissing arcs it holds *whatever the current may be*. Thus, while for silent arcs the constants which correspond with the terms 29·25 and 2·75 in equation (4) are constant only for each separate current, and change when the current changes, with *hissing* arcs they remain the same *whatever* the value of the current may be. For instance, the equation equivalent to equation (4) for a normal silent arc with a current of 4 amperes is

$$V = 41{\cdot}79 + 4{\cdot}71\, l,$$

and with a current of 12 amperes it is

$$V = 39{\cdot}85 + 2{\cdot}95\, l;$$

but with the hissing arc the equation is

$$V = 29{\cdot}25 + 2{\cdot}75\, l,$$

whether the current be one of 20 amperes or of 50, *and whether the arc be normal or not.*

This brings me to the reason for the great importance of distinguishing between arcs that are normal and those that are not. We have seen that, with normal arcs of any given length, hissing only starts when all the silent arcs have been used up, as it were; that is to say, when the current is *greater* than it can be with any silent arc of the same length. But with a *non*-normal arc of 2 mm. I have been able to produce hissing with a current of 11 amperes, and to have a silent arc burning with a current of 28 amperes, the same carbons being used in each case. This apparent anomaly will be fully explained later, when we go into the causes that produce hissing.

We are now in a position to find the law that connects the length of the arc with the change that takes place in the P.D. between the carbons when hissing begins. For, if we call V the P.D. between the carbons at the *hissing point*, with any given length of arc l, and V′ the same P.D. when the same length of arc is hissing, then from equations (2) and (4) we get—

$$V - V' = 10{\cdot}8 - 0{\cdot}26\, l \quad \ldots\ldots\ldots\ldots \quad (5)$$

which shows that *the longer the arc the less does the P.D. between the carbons diminish when it changes from silence to hissing.*

In 1889, Luggin[1] found, by measuring the fall of potential between each carbon and the arc, that the principal part of the diminution of P.D. caused by hissing took place at the junction of the positive carbon and the arc. About three years ago, not then having come across any account of Luggin's experiments, I made some of the same sort myself and obtained the same result. I used two solid Apostle carbons 11 mm. and 9 mm. in diameter, and the third carbon for placing in the arc was 3 mm. in diameter. This last was somewhat thick, but it burnt well to a point in the arc, and thinner carbons burnt away too rapidly with the current I used—25 amperes—to give good measurements. The P.D. between the positive carbon and the arc was found by placing the third carbon in the arc as close as possible to the positive carbon, and measuring the P.D. between the two with a very high resistance voltmeter. This was easily done when the arc was hissing, but was impossible when the largest silent current was flowing, for then the mere insertion of the third carbon was sufficient to make the arc hiss. Accordingly the P.D. between the positive carbon and the arc when the largest *silent* current was flowing has had to be calculated from the formula I gave at the meeting of the British Association[2] last year for calculating that P.D. with *any* silent current, viz.:

$$V = 31{\cdot}28 + \frac{9 + 3{\cdot}1\,l}{A}$$

In Table II. two sets of currents are dealt with, viz., the largest silent current for various lengths of arc, and a hissing current of 25 amperes; and, for each of these sets of currents and lengths of arc, two P.D.'s are given, viz., the P.D. between the main carbons, and the P.D. between the positive carbon and the arc itself.

[1] *Wien Sitzungsberichte*, 1889, vol. xcviii. p. 1192.
[2] *Report of the British Association*, 1898, p. 805.

TABLE II.

P.D. BETWEEN CARBONS, AND P.D. BETWEEN POSITIVE CARBON AND ARC WITH LARGEST SILENT CURRENT AND WITH HISSING CURRENT OF 25 AMPERES.

Carbons: Positive, 11 mm. Solid; Negative, 9 mm. Solid.

Length of Arc in Millimetres. (1)	Largest Silent Current. P.D. between Carbons in Volts. (2)	Largest Silent Current. P.D. between Positive Carbon and Arc in Volts (calculated). (3)	Hissing Current of 25 Amperes. P.D. between Carbons in Volts. (4)	Hissing Current of 25 Amperes. P.D. between Positive Carbon and Arc in Volts. (5)
1	42·2	32·1	32·1	24·4
2	44·5	32·2	34·6	25·2
3	47·5	32·3	37·0	25·7
4	49·4	32·4	40·5	25·7
5	53·0	32·5	43·9	27·9
6	55·5	32·6	45·9	27·2

Now, in order to compare the change in the P.D. between the main carbons caused by hissing, with the corresponding change in the P.D. between the positive carbon and the arc, we must subtract column (4) of Table II. from column (2), and column (5) from column (3), and compare the differences. These differences are given in Table III.

TABLE III.

DIMINUTION OF P.D. BETWEEN CARBONS DUE TO HISSING COMPARED WITH CORRESPONDING DIMINUTION OF P.D. BETWEEN POSITIVE CARBON AND ARC.

Carbons: Positive, 11 mm. Solid; Negative, 9 mm. Solid.

Length of Arc in Millimetres. (1)	Diminution of P.D. between Carbons due to Hissing. (2)	Diminution of P.D. between Positive Carbon and Arc due to Hissing. (3)
1	10·1	7·7
2	9·9	7·0
3	10·5	6·6
4	8·9	6·7
5	9·1	4·6
6	9·6	5·4

Thus, for the lengths of arc dealt with, hissing causes a mean fall of about 9·7 volts in the total P.D. between the main carbons, and a mean fall of about 6·3 volts in the P.D. between the positive carbon and the arc. Hence of the whole diminution of the P.D. between the carbons caused by hissing, about two-thirds takes place at the junction of the positive carbon and the arc.

Further, my experiments showed that very little of the remainder of the diminution, if any, was due to a fall of the P.D. between the arc and the negative carbon; therefore this remaining diminution must be attributed to a lowering of the resistance of the arc itself. We may sum up these results as follows:—

Of the total diminution of the P.D. between the carbons caused by hissing, about two-thirds takes place at the junction of the positive carbon and the arc, and the remaining third seems to be due to a lowering of the resistance of the arc itself.

From Fig. 1 it might be supposed that, given the length of the arc, the increase of current that abruptly occurs as the arc starts hissing was as definite for that length of

arc as the diminution in the P.D. And this, for a long time, I imagined to be the case. But, while trying to find out what law connected the smallest hissing current with the length of the arc, I saw that the value of that current really depended on the circuit *outside* the arc.

For, let E be the E.M.F. in volts of the generator, which we will assume to be constant and independent of the current;

" r " resistance in ohms of the whole circuit *outside* the arc;

" l " length of the arc in millimetres;

" A " largest silent current in amperes;

" V " corresponding P.D. between the carbons in volts;

" A′ " smallest hissing current in amperes;

" V′ " corresponding P.D. in volts,

$$\text{then } E = V + Ar$$
$$\text{and } E = V' + A'r,$$

$$\therefore \frac{A' - A}{V - V'} = \frac{1}{r}$$

that is, the sudden increase of current when hissing begins equals the product of the sudden diminution of the P.D. into the conductance of the circuit outside the arc.

$$\text{Again, } \frac{A'}{A} = \frac{E - V'}{E - V},$$

$$\text{or } A' = \frac{E - V'}{E - V} A,$$

But for a given hissing point, for example, B (Fig. 1), V, V′, and A are all constants; therefore for such a point A′ depends simply on E.

In fact, for a fixed point B and a fixed line F G, the position of the point F merely depends on the slope we give to the line E B F; that is, on the point on the axis of P.D. we select for E. And a consideration of the figure shows that the distance between this point E and the axis of current measures E, the E.M.F. of the dynamo. Consequently it now appears that the dotted lines in the unstable region constitute records of the particular E.M.F.'s

the dynamo was made to give on the various days when the experiments were made with the different lengths of arc, several years ago.

Hence, when the largest silent current changes to the smallest hissing current for the same length of arc, the value of that smallest hissing current depends on the E.M.F. of the generator only.

Thus, it is possible, by choosing suitable E.M.F.'s, to make the sudden smallest hissing current have any value greater than that of the largest silent current for the same length of arc. It is evident from Fig. 1 that the smaller the E.M.F. of the generator, the larger will be the value of the smallest hissing current, for the lower down will E be on the axis of P.D., and therefore the farther will the point F be along the line F G. This explains a circumstance that puzzled me greatly when it happened, but which is now perfectly comprehensible. Some years ago, I was using accumulators to maintain an arc, and employing as small a number of cells as possible. I was able to have quite a moderate current as long as the arc was silent, but as soon as it began to hiss, the current rushed up to some huge value which would inevitably have ruined the cells, if I had not had a cut-out arranged to break the circuit. Why the first hissing current should be so much greater than I was accustomed to find it with the dynamos I ordinarily used, I could not imagine, but the reason is now perfectly obvious. The hissing current was so great simply because the E.M.F. of the cells was so small, and, had it been possible to maintain a silent arc without any resistance in the outside circuit except that of the cells, which is what I was trying to accomplish, I might, except for the cut-out coming into operation, have had practically an infinite current when the arc began to hiss.

We now pass from the consideration of the electrical measurements of the arc to the appearance of the crater, arc, and carbons.

Every alteration of the current and of the distance between the carbons naturally produces a corresponding modification of all parts of the arc, but, until the value of the current attains a certain magnitude, which depends only on the length of the arc with a given pair of carbons, this

change is one of degree merely, and not of character. A greater current simply produces a larger crater, a larger arc, and longer points to the carbons. When the special current is reached, however, a change which is no longer merely one of degree, takes place in the crater. Instead of presenting a uniformly bright surface to the eye, this becomes partly covered with what appear to be alternately bright and dark bands in one or more sets of concentric circles, moving round different centres in opposite directions. The directions of rotation and the entire positions of the images change continually, and the motion grows faster and faster as the current is increased.

It is impossible that these figures, which do not move too fast to be clearly seen by the eye in an image of the arc magnified only ten times, should have escaped the observation of all those who have made a study of the arc, and yet, after a careful search, I can find no mention of them anywhere.

When the current is so much increased that the motion becomes too fast for the eye to detect, the arc begins to hum, and then, as Mr. Trotter[1] first showed in 1894, it rotates at the rate of from 50 to 450 revolutions per second. These rapid revolutions, which the unaided eye is incapable of observing, he discovered by the use of a disc having alternate arms and spaces, and kept in rapid rotation; but, in his account of his observations, he makes no reference to the relatively slow rotations which *precede* the hum. In fact, the rotations observed by Mr. Trotter appear to begin just where those I have been describing become too quick for the eye to see unaided, and end just as the arc begins to hiss, for he mentions that at 450 revolutions per second the arc breaks into a hiss.

As soon as hissing begins the whole appearance of the crater changes again; a sort of cloud seems to draw in round a part of it, moving from the outer edge inwards, and varying continually in shape and position. Sometimes but one bright spot is left, sometimes several, but always the surface is divided into bright and dull parts, giving it a mottled appearance, as is slightly indicated in (*b*) Fig. 4. If, then, the current be diminished, so that the arc becomes silent again, the whole surface of the crater grows dark

[1] *Proc. Roy. Soc.*, 1894, vol. lvi. p. 262.

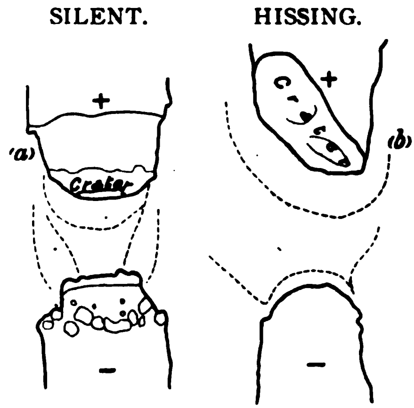

FIG. 4.

CARBONS.

Positive, 9 mm. Cored ; Negative, 8 mm. Solid.

LENGTH OF ARC.

(*a*) 5 mm., (*b*) 8 mm.

CURRENT.

(*a*) 3·5 amperes, (*b*) 34 amperes.

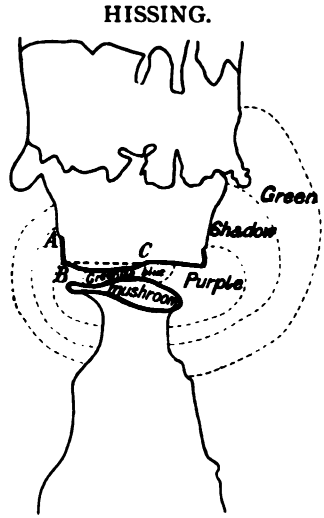

FIG. 5.

CARBONS.

Positive, 11 mm. Solid ; Negative, 9 mm. Solid.

LENGTH OF ARC, 1·5 mm. CURRENT, 28·5 amperes.

for an instant, then brightens in spots, and finally becomes bright again all over.

The vaporous arc itself undergoes fewer modifications ;

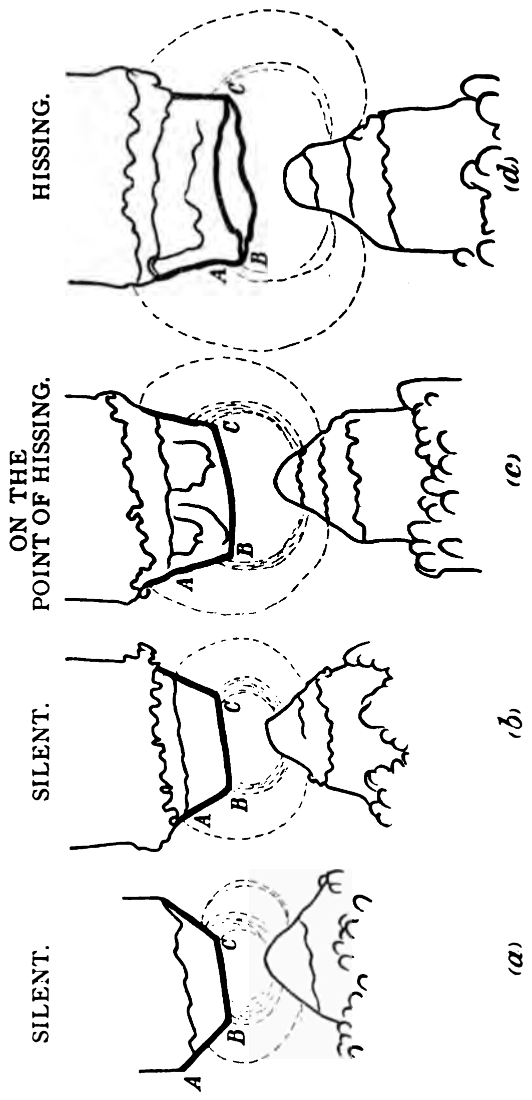

Fig. 6.

Carbons.

Positive, 11 mm. Solid ; Negative 9 mm. Solid.

Length of Arc, 2 mm.

Current.

(a) 6 amperes, (b) 12 amperes, (c) 20 amperes, (d) 30 amperes.

it preserves the ordinary characteristics of the silent arc while dancing circles hold possession of the crater, but, when humming begins, a green light is seen to issue from

the crater, and with hissing this becomes enlarged and intensified, till the whole centre of the purple core is occupied by a brilliant greenish-blue light, as is indicated in Fig. 5. The *shape* of the arc now alters also. While it is silent or humming, no great difference can be observed in its form. With solid carbons it is rounded or pear-shaped according to its length, and has an appearance of great stability. But as soon as hissing occurs the arc seems to suddenly dart out from between the carbons and to become flattened out, as if under the influence of a centrifugal force acting at right angles to the common axis of the two carbons. In Fig. 5 this flattened appearance is well marked, as it is also in (*d*) Fig. 6; and indeed these figures show that every part of the vaporous arc itself is involved in this flattening—the purple core, the shadow round it, and the green aureole—as if they were all revolving with great rapidity round a common axis. And what more likely than that this should be the case, since, as has already been mentioned, the arc is revolving at the rate of 450 revolutions per second *at the moment that it starts hissing?*

As regards the carbons themselves, the only important modification of the *negative* carbon that appears to be due to hissing is the formation of the well-known "mushroom" at the end of that carbon with a *short* hissing arc. This mushroom, of which a good example is seen in Fig. 5, is well named, not only because of its shape, but also because of the rapidity of its growth, which is so great that, while it is forming, the carbons often have to be *separated*, instead of being *brought together*, to keep the length of the arc constant.

And now we come to the most important of all the changes that take place when the arc begins to hiss, viz.: the alteration in the shape of the *positive* carbon.

During the course of his 1889 experiments, Luggin[1] observed that the arc hissed when the crater filled the whole of the end of the positive carbon. He was thus the first to call attention to the fact that there was a direct connection between hissing and the relation between the area of the crater and the cross-section of the tip of the positive carbon. My own observations in 1893 led to a conclusion somewhat

[1] *Wien Sitzungsberichte*, 1889, vol. xcviii. p. 1192.

similar to Luggin's, but yet differing in an important particular. It seemed to me that with hissing arcs the crater always *more* than covered the end of the positive carbon—that it overflowed, as it were, along the side. How far this is true will be seen from an examination of Figs. 4, 5, 6, and 7, which show the shaping of the carbons under various conditions with silent and hissing arcs. These figures have all been made from tracings of the images of actual normal arcs, burning between carbons of various sizes. For Fig. 4 the diameters of the carbons were the same, but the currents and lengths of arc were different. Fig. 5 is the image of a

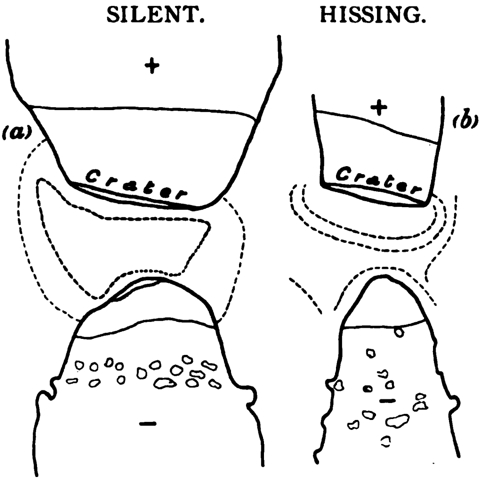

FIG. 7.

CARBONS.

(*a*) Positive, 18 mm. Cored. Negative, 15 mm. Solid. (*b*) Positive, 9 mm. Cored. Negative, 8 mm. Solid.

LENGTH OF ARC, 5 mm. CURRENT, 25 amperes.

short hissing arc. For Fig. 6 the positive carbons were all of the same size, and the arcs of the same length, but the current had four different values, while for Fig. 7 the current and the length of the arc were the same for both (*a*) and (*b*), but the diameter of one of the positive carbons was twice that of the other. The figures were carefully chosen with special reference to the shaping of the positive carbons. For with normal arcs the shape of the end of a positive carbon, even taken quite apart from that of the negative carbon and of the vaporous arc itself, is capable of

revealing almost the whole of the conditions under which the arc was burning when it was formed. It is possible, for instance, with a normal arc, to tell, from a mere drawing of the outline of the positive carbon and of its crater, whether the arc with which it was formed had been open or enclosed, short or long, silent or hissing, burning with a large or with a small current for the size of the carbon.

Take, for example, Fig. 4, and note the difference in the shape of the positive carbon with a current of 3·5 amperes as in (*a*), and with one of 34 amperes, as in (*b*). In the first case the tip of the positive carbon is rounded, so that the crater lies in its smallest cross-section; in the second, the tip would be practically cylindrical for some distance, but that the crater has burnt away a part of the cylinder, making the tip look as if it had been sheared off obliquely. Comparing now the tips of the positive carbons when the arc is silent and when it is hissing in all the four figures, 4, 5, 6, 7, we find the same difference. With all the silent arcs the tips are more or less rounded, and the crater lies in the smallest cross-section, and consequently is less in area than any but the smallest cross-section. With all the hissing arcs, on the other hand, the tip of the positive carbon is practically cylindrical for a short distance at least, or would be but that it is sheared away by the crater; consequently the area of the crater is *greater* than the smallest cross-section of the tip, or indeed than the cross-section of the tip for some little distance along its length.

We have now arrived at the real, the *crucial*, distinction between a silent and a hissing arc. When the crater occupies the *end* of the positive carbon only, the arc is *silent;* when it not only covers the end, but also extends up the *side*, the arc *hisses*. Hence, the arc must be at the *hissing point* when the smallest increase in the area of the crater will make it begin to cover the *side* of the positive carbon, and this can only be when the tip of that carbon has very nearly the same cross-section for some little distance from its end —in other words, when its sides are nearly vertical.

I shall now proceed to show that the extension of the crater up the side of the positive carbon is not the *effect* but the *cause* of hissing; that, in fact, *hissing is produced by the crater becoming too large to occupy the end only of the positive carbon, and by its, therefore, extending up its side.*

This seems an absurdly simple and inadequate cause to produce such complicated phenomena as those belonging to the hissing arc, but it is the true one nevertheless. Before I proceed to prove this, I will show how the laws for the largest silent currents with normal arcs, which have been already obtained from the electrical measurements on pages 370, 372, and 374, may be deduced on the above hypothesis from Figs. 6 and 7.

In Fig. 6 we have a series of four normal arcs of the same length, burning between solid carbons of the same diameter, but in (*a*) the current is 6 amperes, in (*b*) 12, in (*c*) 20, and in (*d*) 30 amperes. The bluntness of the tip of the positive carbon may be measured by the obtuseness of the angle A B C. In (*a*) the tip is very blunt, and the area of the crater is certainly less than any but the smallest cross-section of that tip; therefore the arc is certainly silent. In (*b*) the tip is less blunt, but the arc is still evidently silent; in (*c*) the angle A B C is much more nearly a right angle, and it is plain that a very small increase in the area of the crater would cause it to burn up the side of the tip, therefore the arc is near the hissing point. In (*d*) the angle A B C is practically a right angle, the tip of the positive carbon is cylindrical, and the crater has evidently burnt partly up its side. Thus, keeping the length of the arc constant and gradually increasing the current must gradually bring us to a hissing point.

Next, I have shown elsewhere[1] that with a constant current the end of the positive carbon becomes rounder and blunter, and occupies a larger portion of the entire cross-section of the carbon rod the more the carbons are separated. Hence, the longer the arc, the greater must be the area of the crater, and consequently the greater must be the current, before the crater extends up the side of the positive carbon. Consequently, the longer the arc, the greater is the largest silent current.

Thirdly, it follows that when the current and the length of the arc have been increased to such an extent that the tip of the positive carbon occupies the whole cross-section of the carbon rod itself, no further increase in the size of the crater is possible without a part of it extending up the side of the positive carbon. Hence the largest silent current

[1] *The Electrician*, 1895, vol. xxxiv. p. 614.

for a positive carbon of a particular diameter cannot exceed a particular value, however long the arc may be made. And lastly, similar reasoning used in conjunction with Fig. 7 tells us that the thicker the positive carbon the greater must be the largest silent current for a particular length of arc, which was one of the results deduced from the curves in Figs. 2 and 3.

Consequently, the fact that hissing occurs when the crater covers more than the end surface of the positive carbon and extends up its side, combined with our knowledge of the way in which the positive carbon shapes itself in practice, is sufficient to enable us to deduce *all* the laws given on pages 370, 372, and 374, which govern the largest current that will flow silently with the *normal* arc under given conditions.

It is also now obvious why, when the arc is *not* normal, it may be made to hiss with small currents or be silent with quite large ones. For suppose, for instance, the end of the positive carbon were filed to a long fine point, then a very small current would make a crater large enough to extend up the side of the point, and produce a hissing arc. Whereas, on the contrary, if the end were filed flat, so as to have as large a cross-section as possible, quite a considerable current could flow silently even with a short arc, for, in that case, it would require the current to be great for the crater to be large enough to fill up the whole of the end of the positive carbon.

We come now to the question, why should the arc hiss when the crater burns up the side of the positive carbon—what happens then that has not happened previously? In pondering over this question, the possibility occurred to me that, as long as the crater occupied only the end surface of the positive carbon, it might be protected from direct contact with the air by the carbon vapour surrounding it, but that, when the crater overlapped the side, the air could penetrate to it immediately, thus causing a part at least of its surface to *burn* instead of volatilising. Many circumstances at once seemed to combine to show that this was the true explanation. The dancing circles I had observed, and Mr. Trotter's stroboscopic images, how were they caused but by draughts getting into the arc? Then

the humming noise, which I long ago described as sounding like the wind blowing through a crack, was not this probably caused by the air rushing through a slight breach in the crater already getting near to the critical size? This air, pouring in faster and faster as the breach widened, would cause the arc to rotate faster and faster, sometimes in one direction, sometimes in another, according as the draught was blown from one side or the other. Then finally the air would actually reach the crater, burn in contact with it, and the P.D. would fall and the arc would hiss.

In the open arc, whether silent or hissing, the outer envelope of the vaporous portion is always bright green. With the hissing arc the light issuing from the *crater* is also bright green, or greenish blue. What so likely as that the two green lights should have a common origin, viz.: the combination of carbon with air? For the outer green light is seen just at the junction of the carbons and carbon vapour with the air, and the inner one only appears when air can get direct to the crater.

Again, why does the arc always hiss when it is first struck? Is it not because a certain amount of air must always cling to both carbons when they are cold, so that when the crater is first made its surface must combine with this air?

The cloud that draws in round the crater when hissing begins would be a dulness caused by the air cooling the part of the crater with which it first came into contact, the bright spots being at the part where the crater and air were actually burning together. In fact, everything seemed to point to the direct contact of crater and air as being the cause, perhaps not of the hissing *sound*, but of the diminution in the P.D. between the two carbons, which is the important part of the hissing phenomenon.

One easy and obvious method of testing this theory immediately presented itself. If air were the cause of the hissing phenomena, exclude the air and there would be no sudden diminution of the P.D. between the carbons, however great a current might be used. Accordingly I tried maintaining arcs of different lengths in an enclosed vessel, and increasing the current up to some 40 amperes. *No* sudden diminution of the P.D. could be observed with any of the currents or lengths of arc employed, although when

the same carbons were used to produce *open* arcs, the sudden diminution of 10 volts in the P.D. between the carbons occurred with a current as low as 14 amperes for a 1 mm. arc.

Indeed, so far from there being any sudden diminution in the P.D. when the current through an *enclosed* arc is raised to higher and higher values, the P.D. appears to slightly increase for large currents.

It was, of course, impossible, in these experiments, to avail myself of an ordinary enclosed arc lamp, since a current of some 5 or 8 amperes is all that is used with

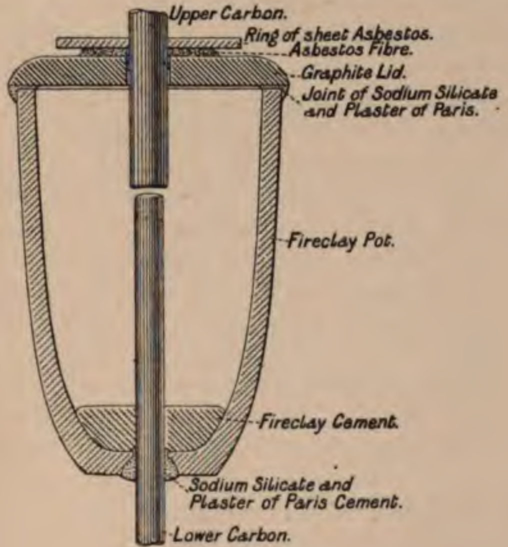

FIG 8.

such a lamp, whereas to test my theory it was necessary to employ currents up to 40 amperes, although my carbons were of smaller diameter than those fitted in ordinary commercial enclosed arc lamps. Accordingly, I constructed little electric furnaces, some made out of fire-clay crucibles with lids of graphite sealed on, as in Fig. 8; some moulded out of fire-clay with mica windows inserted, so that the image of the arc could be projected on to a screen and its length kept constant; some constructed of iron lined with asbestos. Some had tubes inserted in them through which the air could be admitted when required, &c.

It was found that when the vessel was entirely enclosed, the pressure in it was so great, on the arc being first started, that occasionally the lid was blown off. Consequently the space between the positive carbon and the asbestos ring was left open till the arc was well started, and then was tightly closed. This sudden increase of pressure probably took place when the carbons were first *brought into contact*, for Mr. Seaton,[1] while conducting some experiments for Messrs. De la Rue and Müller in 1879, observed that when the arc was completely enclosed the increase of pressure when the carbons were first brought into contact was far greater than could be accounted for by the rise of temperature of the gas in the vessel, and that the pressure fell, the moment the carbons were separated, almost to what it had been before contact was made. This fact was confirmed by some experiments made by Stenger,[2] in 1885. This first great rise of pressure may, of course, be partly caused by the gases occluded in the carbons being expelled on the current being started, but a complete investigation of this phenomenon has not, as far as I am aware, yet been made.

Some curves connecting the P.D. between the carbons with the current when the arc was completely enclosed in the crucible (Fig. 8) are given in Fig. 9. The carbons were solid, the positive being 11 mm. and the negative 9 mm. in diameter, similar to those used with the open arc experiments (Fig. 1). As this crucible—the first one made—had no window, the length of the arc could not be kept quite constant, but the distance by which the carbons were separated was noted at the beginning of the experiment, and they were then allowed to burn away, without being moved, till the end, when the distance the positive carbon had to travel in order to bring it tightly against the negative, was noted. Measured in this way, the length of the arc was 1·5 mm. at the beginning and 2 mm. at the end of the experiment. The current was started at 6 amperes, and gradually increased to 39 amperes; then as gradually diminished to 6 amperes again, increased to 36 amperes, and diminished to 5 amperes, when the arc was extinguished. The P.D. between the carbons for a given current seems to have increased as the length of time during which

[1] *Philosophical Transactions*, 1879, p. 179.
[2] *Wiedemanns Annalen*, 1885, vol. xxv. p. 31.

ARC ENCLOSED IN CRUCIBLE.

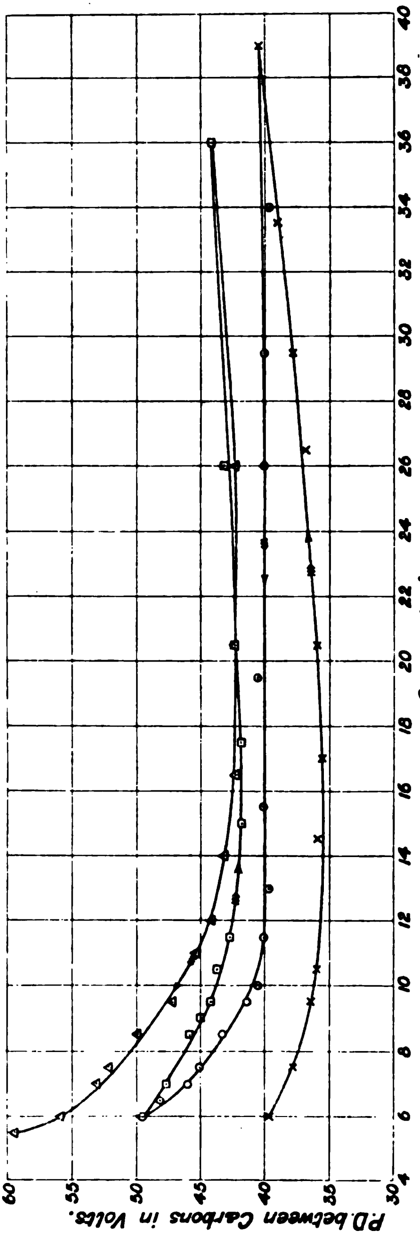

FIG. 9.

Curves connecting P.D. and Current for nearly Constant Length of Arc of 1·5 mm. to 2 mm. The Arrows show the Direction in which the Current was varied.

CARBONS.

Positive, 11 mm. Solid; Negative, 9 mm. Solid.

the arc had been burning increased; this was undoubtedly partly due to the lengthening of the arc, but was probably also partly due to the whole of the air in the crucible having been gradually burnt up, or driven out between the slag wool and the asbestos ring, by the pressure of the carbon vapour.

Many other sets of curves were obtained, but all with the same result, viz., that, when once the crucible had been freed from air, no sudden diminution in the P.D. could be observed on increasing the current far beyond the value at which this diminution occurred on lifting up the lid and allowing the air to have access to the arc.

The next thing to do was to try if an open arc could be made to hiss and the P.D. to suddenly diminish by blowing air against the crater, when the current was so small that the crater remained well at the end of the positive carbon—in fact, to bring the air in contact with the crater artificially, when a much smaller current was flowing than would usually produce hissing. I first tried inserting a carbon tube in the arc and blowing through it, but this almost invariably blew the arc out. Then Mr. Phillips, one of Professor Ayrton's assistants at the Central Technical College, suggested using a tubular positive carbon, and blowing the air down it. This plan answered admirably, for, when a current of 10 amperes was flowing with an arc of about 3 mm., so that the arc was quite silent, each puff of air blown down through the positive carbon was followed by a hiss and the characteristic diminution of the P.D. between the carbons. With a current of 6 amperes, however, I could get no hiss, but simply blew the arc out each time, probably because, with such a small current, the arc was cooled sufficiently to be extinguished before the action could take place.

Oxygen was next tried, still with the open arc, and again each puff produced a hiss and diminution of the P.D., the latter being exactly the same in amount as when air was used, namely, about 10 volts. As my idea was that the diminution of the P.D. was due to the chemical combination of air with carbon at the temperature of the crater, the fact of oxygen producing the same diminution of the P.D. as air seemed to show that nitrogen would produce no effect, and that all the effect produced by air was due to

the oxygen in it. Accordingly I tried blowing nitrogen down the positive carbon of an open arc, and found that *no* change in the P.D. followed if the nitrogen was blown through gently, but that, beyond a certain pressure, the arc was blown to one side, and thus lengthened, so that the P.D. *rose*, and, if the pressure continued, the arc went out.

This experiment proved two things—firstly, that it is the *oxygen* in the air that causes the diminution in the P.D. with hissing; secondly, that this diminution in the P.D. is not due to cooling, for nitrogen would cool the arc as effectually as oxygen or air.

To make assurance doubly sure on this point, carbon dioxide was blown down the tubular positive carbon, with the same result as when nitrogen was used, viz., no change was produced in the P.D. between the carbons unless the pressure of the gaseous stream were large enough to blow the arc on one side, and then an increase and not a diminution in the P.D. was observed.

If, however, the current was *very near* the value that made an open arc of the particular length used start hissing, blowing either nitrogen or carbon dioxide through the positive carbon sometimes started hissing; but this was due, *not* to any direct action of the stream of gas on the carbon, but to the arc being deflected by the gaseous stream and burning obliquely up the side of the carbon and thus allowing the air to come into contact with the crater. The proof of this was that this diminution in the P.D. had the same value as if air had been employed, and that the hissing phenomena did not disappear on stopping the stream of nitrogen or of carbon dioxide.

This was not the case with hydrogen, however. When that gas was blown down the positive carbon in the open air, the arc would start hissing if the current were large enough, *and stop hissing the moment the hydrogen was shut off.* Not only this, but the diminution in the P.D. had a different value from that produced by air, being only about 6·5 volts instead of 10 volts. Table IV. gives the current and the P.D. between the carbons just before the hydrogen was turned on, just after it was turned on, just before it was turned off, and just after it was turned off.

TABLE IV.

Effect of Blowing Hydrogen down a Tubular Positive Carbon.

Carbons: Positive, 11 mm. Tubed; Negative, 9 mm. Solid; Length of Arc, about 3 mm.

P.D. between Carbons in Volts.				Current in Amperes.
Before H. was turned on.	After H. was turned on.	Before H. was turned off.	After H. was turned off.	
52	46	46	53	14
52	45	47	52	12
52	45	45	52·2	12
52·5	46	46	53	12
52	45	46	52	9

Thus, the mean diminution of P.D. accompanying the hissing caused by hydrogen being sent down the positive carbon of an arc burning in the air was about 6·6 volts, or about 3½ volts lower than when the hissing was caused by air alone.

In order to exclude all possibility of doubt as to the effect of the various gases, I repeated the experiments with the arc entirely enclosed, so that the only gases that could reach it were those blown down the tubular positive carbon. The current was distinctly below the hissing point, being only 10 or 11 amperes, and the arc was from 2 mm. to 3 mm. long.

When air was blown down the positive carbon, each puff lowered the P.D. by about 10 volts, and the moment the puff ceased the P.D. rose again. Next, oxygen was tried, with the same result. Thirdly, nitrogen with *no* result, or with the result that the arc was blown out if the pressure was too great. Carbon dioxide had the same effect as nitrogen, and lastly hydrogen was tried.

This gas, however, gave a totally different result with the

enclosed arc from that already obtained with the open arc. For whereas, as has been previously stated, hydrogen produced a distinct hissing of its own when blown down the positive carbon in the *open air*, it produced *none* when used in the same way with the *enclosed* arc.

To prove that, in order to produce the sudden diminution of P.D. under discussion, it was necessary for the active gas to actually touch the crater, a tubular *negative* carbon was used, and each gas was blown up through it in turn, gently enough not to force the gas directly against the crater.

In *no* case was there any sudden diminution of the P.D., whatever was the gas blown through the negative carbon, and whether the arc was open or enclosed. On the contrary, there was generally a small increase, probably due to the lengthening of the arc by its being blown on one side. If oxygen or air were blown *very hard* up the negative carbon, they would either produce hissing, or blow the arc out, or both; for in that case some of the gas got to the crater uncombined with the carbon vapour, and acted exactly as if it had been blown down the tubular positive carbon.

The case, then, stands thus:

(1) When the arc begins to hiss in the ordinary way, the P.D. between the carbons diminishes by about 10 volts.

(2) If the air is excluded from the arc, this diminution of the P.D. does not take place, even when the current is nearly three times as great as would cause hissing in the air.

(3) If, however, while the air is excluded, puffs of air are sent against the crater, the diminution of the P.D. *does* occur, even with currents much *smaller* than would cause hissing in the air.

(4) If, instead of air, *oxygen* is sent against the crater, the P.D. is diminished to exactly the same extent as when air is used.

(5) If, on the other hand, *nitrogen* is sent against the crater, *no* diminution of the P.D. is observable.

(6) If air or oxygen is gently blown through the *negative* carbon, so that it cannot get direct to the crater, *no* diminution of the P.D. follows.

Thus there can be no shadow of doubt that *the sudden diminution of P.D. that accompanies the hissing of the open arc is due to the oxygen in the air getting directly at the crater and combining with the carbon at its surface.*

This explanation, like many another in science, solves certain difficulties, only to create others which may, perhaps, be yet harder to deal with. Directly we know exactly how the diminution of P.D. accompanying hissing is brought about, we ask, "How does this combination of gas and carbon, at the temperature of the crater, act in producing a diminution of P.D. so sudden, so certain, following such definite laws as those we have been considering? Why do certain gases produce it and not others? Why does hydrogen act in the presence of air, but not when air is absent?" &c.

These are questions that have been occupying me for some time past, and which will, I hope, be completely answered by the series of experiments on which I am still engaged. These latter experiments, although suggested by the investigation on the Hissing of the Electric Arc, have already opened up a different field of inquiry, and the further results may therefore with advantage be reserved for a future communication.

It only remains for me to express my high sense of the honour done me by the Institution of Electrical Engineers in asking me to read this paper, and to thank the many people to whom I am indebted for very kind assistance.

My most sincere thanks are due to Professor Ayrton for much criticism, for advice as to the methods of conducting the experiments, and for invaluable help in the literary portion of the work; to Mr. Solomon for constructing with much ingenuity the first enclosing crucible, and for making the earlier experiments with it; to Mr. Phillips for assisting in the experiments and for drawing most of the diagrams, especially the admirably executed coloured drawings of the hissing and silent arcs; to Mr. Carter for supervising, and to Messrs. Manuel & Pletts for very carefully constructing the apparatus for the experiments of this evening; and lastly, to Messrs. Arundell and Spencer for helping with most of the experiments for the paper, and for conducting many of them alone, with great skill and patience, as well as for carrying out so admirably all those that have been shown to you this evening.

The President.

The PRESIDENT: If we followed our ordinary procedure we should at once go to the discussion of the paper to which we have been listening. But I feel that this is not an ordinary occasion, and that the last words spoken by Mrs. Ayrton require me to depart from usage in that respect. Mrs. Ayrton said she felt honoured by the Institution in having been allowed to make this communication. I am sure that we on our part feel more than equally honoured that Mrs. Ayrton has chosen this Institution as the medium of its publication. It is a communication of unusual merit. Based upon elaborate, painstaking, exact experimental observation and upon the clearest reasoning, it forms, I think, quite a model of scientific method in research.

Perhaps before we go to the cold-blooded business of the dissection of the paper I may be allowed to say that I feel astonished at the great progress that has been made, and the perfection that has been realised, in the arc method of electric lighting. I remember the time when the arc light was a new thing, a thing attended with frequent splutterings and hissings and extinctions, not entirely due, as we now know, thanks in a great measure to the research of Mrs. Ayrton, to the defects of the lamps, but due in some considerable measure to defects in the carbons themselves. Carbons at that time were always cut square or octagonal, and out of some hard form of carbon such as we find lining gas retorts. Arc lighting has now reached a perfection that, considering the many circumstances and conditions which tend to the instability of the arc (many of which have been very beautifully illustrated to us to-night), seemed at first impossible of attainment. A degree of perfection has been reached that seemed almost finality until Mrs. Ayrton showed us that probably we may advance still further. I think amongst other practical uses of the discovery—for I can speak of it in no other terms than as a discovery—Mrs. Ayrton has made of the cause of the hissing of the electric arc, we shall probably no longer have those flutes or grooves in the carbons used in connection with lighthouse illumination, and which seem, now that we know the cause of the hissing of the arc, designed with the object of preventing the use of the large currents suitable for such lighting. Furrows in the sides of the positive carbon would seem to be highly provocative of the hissing arc. I am not at this stage going to move a formal vote of thanks: that comes later. I am only going to express, for myself, and I am sure I may also express for you all, our very high appreciation of the honour done us by Mrs. Ayrton in coming here to-night and giving us this most valuable contribution to electrical engineering procedure, the first that we have had placed before us by a lady. I know that she will not desire any departure from ordinary routine on that account, but I must say it has added interest to the paper to know that the difficult and laborious work it sums up has been done by a lady who has many duties and occupations besides those incidental to highly abstract scientific research.

Professor Thompson.

Professor SILVANUS P. THOMPSON: I had hoped that Professor Ayrton would have opened this discussion; but as I do not wish to remain silent on this occasion, I must be allowed to join in the congratulations which you have already voiced, that our Institution

Professor Thompson.

has been the channel through which this very important paper has been given to the scientific world. I am quite sure that Mrs. Ayrton would be the last to desire that we should put her upon any platform of amateurishness by declining to discuss what is so excellent a professional contribution to our particular science, and therefore I propose to take up a point or two in the paper that has been read to us.

In the first place, it is clear that we have now got practically a new definition of the term "hissing arc." Apparently there are several things that a hissing arc may do. It makes a noise. It shows a green colour—that was also a discovery of Mrs. Ayrton's, I think, a few years ago. It spurts; and occasionally, when its spurting becomes extreme, produces mushrooms. It covers the surface of the crater for the time being with a kind of cloud or nebula. And it is accompanied, when produced, by that very distinct drop of potential which has been so large a feature of the electrical measurement.

Now, which of these things is to be taken as the criterion of hissing? I always supposed the noise was to be so regarded, and I am not quite sure it is not so. Suppose an arc to have all, or most, of the characteristics I have enumerated. I am not quite clear from this paper whether it is not still a hissing arc, even although you cannot hear it. Yet, after all, the drop of potential appears to be the important indication, in Mrs. Ayrton's mind, because again and again she pointed out to us how in certain cases it dropped, and in other cases it did not. For example, we had the images of two arcs thrown side by side on the screen. The one we were told was an open arc, and the other an enclosed arc. Presently the current was increased through both of them, and we heard a hissing sound, and we were told it was the open arc which was hissing. But to all appearances it was the enclosed arc that was doing so; it went green, it spurted violently, and, if we had not been told, I should have supposed the drop of potential belonged to that one and not to the open arc. So that the dropping of the potential apparently constitutes the hissing arc; and the turning green, the production of the cloud, and the forming of the mushroom, even if they occur, have nothing to do with the hissing arc *per se*. I hope Mrs. Ayrton will make it quite clear in her reply what is the definition henceforth to be adopted for the hissing arc.

Quite early in Mrs. Ayrton's paper there was a remark made which recalled to me some of the discussions which have taken place upon the physics of the arc in times past. We were asked to look at diagram No. 1, at the lowest line belonging to an arc 1 mm. in length, and Mrs. Ayrton pointed out that if we had it in the silent state we might increase the current up to 14 amperes and no more. Then the instability would set in, and after that we might have a hissing arc of 17 amperes or any amount more; but we could not possibly have an arc either silent or hissing at 15 amperes. It is not the first time that the fallacy —for it certainly is a fallacy—has turned up in the physics of the arc, and Mrs. Ayrton herself has now supplied the antidote. I have had occasion before now—I think it was when Professor Ayrton was bringing the physics of the arc before us—to point out that there has

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been in the past a fallacy in some of these matters. With reference to the same diagram, taken from Mrs. Ayrton's earlier publications, we used to be told (and my friend Professor Ayrton will no doubt be able to reply to me on this point, if he has any reply by this time) that if you have an arc and keep the potential difference constant, and then lengthen the arc (which would apparently put in an additional resistance) the current increases rather than decreases. The diagram appears to indicate this. We have, let us say, an 8-ampere current at 55 volts, and the arc little more than 4 mm. long. Lengthen the arc, that is to say, go to one of the higher lines in the series of curves, and at the same time keep the potential difference constant, and, with a 5 mm. arc, we are told that the current increases to 11 amperes. I ventured to point out when that was told to us that it was a fallacy. It was putting the cart before the horse. I pointed out that when you lengthen the arc, you, at the same time, must do something to the external circuit to prevent the potential falling. What you do is to increase the current in order to keep the potential up, and the result is that you have a larger current with the larger arc. At the time the explanation was entirely doubted, and Professor Ayrton would not believe that the argument was that way round; but I am glad to find that Mrs. Ayrton has supplied to-night the missing point, namely, that the behaviour of the arc depends very largely on what is going on in the external circuit; that if you work with a larger electromotive force, putting in resistance so as to limit the current, you may control your arc in various ways. You can get a 1 mm. arc, with a current that would not give an arc of that length with the particular electromotive force on the circuit illustrated in Fig. 1. You can have a 1 mm. arc with 15 amperes, although in the circuit there illustrated you must go from 14 to 17 amperes. Mrs. Ayrton has shown that the slope of the line running down from C depends on the electromotive force and on the resistance of the rest of the circuit. The point C was reached by using a certain definite current, namely, 14 amperes, on an arc 1 mm. long and with a potential difference of about 42 or 43 volts. If you had reached the same point by using a dynamo with double the electromotive force and with pretty nearly double the resistance in the circuit, or with treble or with ten times the electromotive force, and increasing the resistance to get back to the same voltage, with the same length of arc, you would still be at the point C; but the line running down through C would drop through a different angle. The steeper the angle, the greater the electromotive force and external resistance to arrive at that point. If this was an arc in a circuit of nearly constant current with a very large electromotive force and a very large resistance, you might have that line going perpendicularly down, and the end of the horizontal line would then be drawn out further to the left; and you might then have an arc 1 mm. in length with a current of 15 amperes. It is really one of the most interesting points in this whole research, that it has been shown by Mrs. Ayrton herself how the behaviour of the arc is affected by the circuit which controls the voltage at the terminals.

Professor Thompson. I would like to refer to the very important researches Mr. Trotter made some years ago. I think Mrs. Ayrton underrates one point, viz., the observation Trotter made about the rotation of that curious comma-shaped body. He observed it first with the aid of stroboscopic slits, but he pointed out that it could also be seen without any stroboscopic slits, revolving slowly. The phenomenon of the revolving circles of the arc shown by Mrs. Ayrton is practically the same that Trotter has described as a thing which might be seen with the eye.

Professor Ayrton. Professor AYRTON : Where did he say it ?

Professor Thompson. Professor THOMPSON : He certainly showed it to me in his own laboratory, but I cannot refer to any paper where it has been mentioned.

I had intended to refer also to the question of the shapes of carbons. Mrs. Ayrton's remarks absolutely prove now that the hissing of the arc is intimately connected with the crater creeping over the edge of the carbon, and this obviously bears the inference that square carbons are to be deprecated.

There remains, of course, a great deal yet to be discussed. For example, though it is evidently proved, thanks to Mrs. Ayrton's researches, that the hissing of the crater is connected with the access of oxygen, complicating the effects at the crater surface with direct chemical action, yet there is a good deal more behind that to explain ; namely, why that action should of itself set up instability, the hissing sound, and the drop of potential. Some years ago I likened this phenomenon of the hissing of the arc to that which occurs when you attempt to cause evaporation to take place from a liquid by putting heat into it at a rate too great for the available amount of surface, when you have the phenomenon of boiling with, or without "bumping." The contrast between the evaporation from the top layer of surface pure and simple and the production of vapour, not only at the top surface, but a little below, heaving up the surface with bubbles, or in the case of solids like camphor or sal-ammoniac producing a disintegration of the surface, is so like that which happens physically in the spurting of the arc, that I cannot help thinking there is something of that nature taking place, and that the access of oxygen is responsible for the disintegration going on below the top surface layer of the crater.

Let me conclude by again expressing my very high appreciation of the remarkable piece of work which Mrs. Ayrton has presented to us, and which obviously represents many months, if not years, of laborious and truly scientific work.

Mr. Mordey. Mr. W. M. MORDEY : The paper under discussion is, I think, a record of a thoroughly philosophical investigation which will have important results both practically and theoretically, affecting the physics of the arc, not only from the point of view of the student of pure physics, but also from that of the electrical engineer. After all we, as electrical engineers, want arc lamps to give light, and the interesting physical questions that have been dealt with by Mrs. Ayrton are interesting to us particularly because of their bearing on the light-giving power of the arc.

I would ask Mrs. Ayrton to make it clear whether instability is always and necessarily an accompaniment of hissing. If it is not,

then, of course we need not always mind if an arc does hiss. As to the references just made to the fluted carbons that are used in lighthouses, we must remember that these fluted carbons, although they may hiss, add to the useful effect of the arc by allowing the light to get out horizontally. One of the drawbacks in arc lights is that the light is given off mainly from the surface of the crater, and that the surface is only to a small extent usefully directed to the object under illumination. On that account Sir James Douglass, a former member of this Institution, introduced the fluted form of carbon when he was Engineer to the Trinity House, and I believe it is now always used in the lighthouses round our coast. I have been in a lighthouse when the light was on, and the arc was hissing the whole of the time; but yet there was a good, steady, useful beam. Mr. Mordey.

These lighthouse arcs, so successful for so many years, are alternate current arcs. To-night we have listened to a work on direct current arcs. There is a large field to be explored (and, as I hope, to be explored by Mrs. Ayrton), in connection with the alternating current arc. I hope that, some day, Mrs. Ayrton will give us a paper here not only on the hissing, but also on the physics of the alternating current arc and on its light.

If I may be allowed to go beyond the scope of this paper, I would draw Mrs. Ayrton's attention to a peculiarity I have noticed in connection with the dark space corresponding with the middle of the arc with alternate currents. It is seen, for instance, on an ordinary 14-inch globe as a dark band about 1½ or 2 inches wide. Now, with a low-period alternate current arc, with, say, 50 periods per second, there is a steady pulsation that is not connected, as far as I can tell, with the periodicity. The periodicity, of course, is far too high to be observed by the eye, but if that dark band is observed, it will be seen to increase and decrease in breadth with a regular beat or pulsation at a rate of about 400 times a minute. But, as there are with a 50-period current 6,000 currents a minute, there is no obvious connection whatever between that slower beat and the periodicity itself. I may say that I have tried by various experiments to ascertain the cause of it, but without success. It has nothing to do with the mechanism—it occurs when the carbons are clamped perfectly steady. It has nothing to do with the electro-magnet in the circuit—I have tried it when there was no electro-magnet in the circuit.

Might I ask Mrs. Ayrton to explain the cause, if it is known, of the more granular appearance of the luminous surface of the carbons in the enclosed arc? It is very marked, and I should like to know what the explanation is. I wish, in conclusion, to repeat my very great appreciation of Mrs. Ayrton's results, and of the admirable way in which she has put them before us.

The PRESIDENT: I have to announce that the scrutineers report the following candidates to have been duly elected:— The President.

Associate Members:

James Alexander Bell.	Major P. R. Burn-Murdoch, R.E.

Foreign Member:

Paul Fournier.

Associates:

Henry Thornton Hazzledine.
Harry Henderson.
Alfred George Jackson.
John Daniel Murphy.
Ernest Hugh Rainbow.
Charles Walter Torrens.
Archibald Wright.

Students:

John Frederick Avila.
Kenneth Chas. Horton Newman.
John Albert Ross.
Thomas Gregory Smith.

The Three Hundred and Thirty-First Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, April 13th, 1899—Mr. J. W. SWAN, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on March 23rd were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council :—

From the class of Associates to that of Members—

Graham Thomas Walters Olver. | Hector Douglas Munro.

From the class of Associates to that of Associate Members—

John Bailey.
Captain W. P. Brett, R.E.
C. R. Cormac.
E. C. Cox-Walker.
C. J. Cummins.
C. F. Heywood.
H. B. Maxwell.
F. W. Mills.
T. M. F. Tamblyn-Watts.
G. E. V. Thomas.
E. Thompson.

From the class of Students to that of Associates—

Wilfred Henry Ward.

Messrs. H. J. Tomlinson and V. H. Gregory were appointed scrutineers of the ballot for new members.

Donations to the Library were announced as having been received since the last meeting from Lord Armstrong and Señor Joseph Fola Igúrbide, to whom, on the motion of the PRESIDENT, the thanks of the meeting were unanimously accorded.

The PRESIDENT : I have to announce, on behalf of the Council, that the Annual General Meeting of the Institution will take place on Thursday, May 25th. We will now continue the discussion on Mrs. Ayrton's paper.

Professor Fleming.

Professor J. FLEMING: Mrs. Ayrton's paper brings before us a large amount of matter for discussion, in addition to the particular problem which gives its title to the paper. There are many points in connection with the arc which, it seems to me, are only suggested for investigation in the light of some theory as to what is happening in the arc. It is a remarkable thing, considering the fact that physicists have been familiar with the electric arc for the greater part of a century, that our ignorance concerning nearly all the processes taking place in it is so very great. It is impossible to handle a large number of the questions which arise unless one can form in the first place some conception as to the molecular processes in the arc.

When we look at the continuous current arc, I think the first thing that attracts our attention is the fact of its want of symmetry. You have on one side the crater carbon at a very high temperature, from which the greater part of the light and heat comes out. Then you have the column of incandescent carbon vapour, and on the other side the cooler negative terminal. Essentially the phenomena of the arc consists in the conveyance of a current through a column of vapour, and if we ask by what process that is effected, I think we must bear in mind the conclusion to which most investigators who have studied the propagation of currents through gases have been brought, viz., that it appears to be a process closely similar to that of electrolysis in liquids. In fact one theory which may be put forward concerning the arc is that it consists in electrolysis going on in a gaseous column. Suppose we start with that as a provisional hypothesis.

In the electrolysis of liquids two chief phenomena are presented to us: first of all the fact that the molecule of the electrolyte is broken up into two ions which are urged in opposite directions, and then, according to the majority of researches on the subject, these ions move with very different velocities. Supposing it be accepted merely as a working hypothesis that in the arc we have electrolysis of carbon vapour going on, and the electrolysis consists in the splitting up of complex carbon molecules into carbon ions, positive and negative, these being moved rapidly in opposite directions. I wish to suggest that the velocity of the negative ion may be much greater than that of the positive ion, and trace the consequences of that assumption. The result of that would be that we should have upon the positive pole an accumulation of negative carbon ions which would cover up the crater with what I may call a cushion of negative carbon ions, which would have the effect of creating a resistance.

Let us next consider the distribution of potential in the arc. The effect of these accumulated negative ions round the positive electrode would be that the step in potential in passing from the positive carbon to the vapour would be greater than the step in passing along the vapour, or from the vapour to the negative. And that is exactly what is found to be the case. In 1890 I exhibited as a lecture experiment at the Royal Institution the effect of introducing a third terminal into the arc, and showed that the difference of potential between that third terminal and the positive carbon was about 35 or 40 volts, whilst a very much less difference in potential existed between that third carbon and

Professor Fleming.

the negative pole. I did not know at that time that Luggin, in 1889, had observed the same thing.

Then with regard to the behaviour of the arc when air gets access to it; as Mrs. Ayrton has shown us, that produces the phenomenon of hissing. We know perfectly well the intense affinity that carbon vapour has for oxygen at a high temperature. The result of access of air or oxygen to the crater would be, as it were, to destroy by chemical combination these accumulated negative ions near the crater carbon, and that would immediately produce a diminution in resistance of the arc. That process would be an intermittent process because the effect would be then to produce a greater current, and that again would produce a further delivery or rush of the ions. This view seems to point to the fact that the hissing arc must be an intermittent arc. In the last few days, since the reading of the paper, I have had the curiosity to examine the hissing arc in a revolving mirror to see if the light is intermittent. I do not know whether Mrs. Ayrton has tried this experiment. I found that the light of the hissing arc *is* an intermittent arc. I find it has already been noticed that in a hissing arc the current is intermittent. It is mentioned in the paper by Messrs. Frith and Rogers, in which they put forward the notion of a negative resistance.

The process, therefore, going on in the normal arc, according to my view of the case, would be something of this kind: the electrolysis taking place in the carbon vapour of the arc provides a means for the transfer of electricity, and the rush of the negative ions towards the crater is the cause, partly, of the excavation of the crater; the bombardment of these ions would cause a high temperature of the crater, and the consequent volatilisation of the carbon would provide the necessary fuel or material for the arc by the production of carbon vapour. Hence the arc feeds itself by this process. In addition to this, I think that that theory would explain in some way the formation of the mushroom on the negative carbon at the same time that the hissing begins, because the rapid rushes that take place of the negative ions proceeding towards the crater must be accompanied by an equal number of positive ions in the other directions, and there would be a continual and very rapid delivery at that time of the carbon ions on the negative carbon which would build up the mushroom. This hypothesis would also explain at the same time why it is that hissing is started apparently by oxygen and by air, but not by nitrogen, because, in order to have any hissing, there must be an intermittency, and there must be something, therefore, that removes this cushion, as I have called it, of carbon ions, and that something must be a material capable of combining with carbon. The hissing that is produced by hydrogen may perhaps be explained by the affinity of hydrogen for carbon at high temperatures, producing, as we know, acetylene. The carbon ions would not be removed by any neutral gas. I may venture, perhaps, with diffidence to make the suggestion that any other gas or any other vapour which has the property of combining with carbon at a high temperature will produce hissing. I would suggest that it is worth while to try chlorine, bromine, iodine, and see whether they, too, do not produce the hissing arc.

Professor Fleming.

Mrs. Ayrton has not entered into the discussion of all those points, such as negative resistance and E.M.F., which belong to the region of hypothesis. With regard to the so-called counter-electromotive force in the arc, I have long held the opinion that it does not exist, but that the voltage which has to be applied before any finite length of arc can be created is merely one of the factors in the work which has to be done on the arc to effect the volatilisation of the carbon and supply the vapour which is necessary for the electrolytic process.

I will conclude by adding my congratulations to Mrs. Ayrton for the very philosophical and careful paper which she has brought before us, and the very interesting conclusions to which she has been led by her experiments.

Captain Abney.

Captain W. de W. ABNEY: Although I have done a good deal of work with the arc, I cannot say that I have entered at all into that field which Mrs. Ayrton has so ably filled. The communication before us is an ideal paper. The investigation is carried on from point to point and reasoned out in the most admirable way, and I think that anybody who has worked long with the arc cannot but come to the conclusion that the deductions Mrs. Ayrton has made are perfectly right.

The hissing of the arc has long been a phenomenon of very sad interest to me, because it has interfered many times with experiments in which I had to use the electric arc extensively. I may say that it was by long observation that I came to the conclusion that certain conditions were requisite to cause the hissing; and also that, long before Mr. Trotter described the phenomenon of the rotation of the arc to which Mrs. Ayrton has alluded, I had been infinitely bothered with it in my experiments. The way in which I was troubled was this: I threw an image of the crater of the positive pole upon the slit of a spectroscope, and having by means of prisms produced a spectrum on a focussing screen, recombined that spectrum by means of a lens and formed an image of the first surface of the prism upon a distant screen. By passing a slit along the spectrum any desired monochromatic colour could be obtained. In a very great many cases it has happened that the patch of light which was formed had a regular flicker. I then found there was a definite rotation taking place, and that something was evidently happening in the arc light. To investigate that still further, the slit of the spectroscope was diminished to a point, and then I had the whole of the phenomena in the proper colours very beautifully exemplified upon the screen. By using a pin-hole an image of the carbon point was obtained upon the prism itself, and the image formed in my collecting lens was that of the surface of the prism. I hesitate to say whether the rotation was as much as 450 times a second. All I can say is that, at the time immediately before the hissing, that phenomenon which Mrs. Ayrton describes so well in her paper was to be seen, and this led one to the conclusion that perhaps it might be due to an oscillation in the arc, such as Professor Fleming has pointed out. I did not investigate that at all. At the same time the phenomenon of hissing certainly was accompanied most frequently with a rotation. The rotation may occur always, but it did not always interfere with my observations. I knew when the hissing began that I was liable to get a

rotation by which I could see a sickle-shaped and bright band going over my colours. This was excessively annoying in the experiments which I was making. I have purposely alluded to this rotation to-night because, several years ago, when Mr. Trotter brought it forward at the Royal Society, it was very late in the evening, and I contented myself with saying that I had met with the phenomenon before. Now I have liberated my mind, and I can only say that the phenomenon is particularly interesting and annoying. Captain Abney.

If Mrs. Ayrton will throw the monochromatic image of the carbon points upon the screen and watch it, I think she will be rewarded. This is done in a very easy way, somewhat as I have indicated already. By placing a lens close against a slit and throwing a sharp image of the carbon point upon the prism, the lens which recombines the spectrum gives an image in white light of the carbon points upon the screen. Then if you take a slit in the focus of the spectrum and pass it through, you get for every colour an image of the carbon points in that colour. It is very interesting to find that when the hissing begins a flame is seen surrounding the points when the slit is in the yellow of the spectrum. Simultaneously with the commencement of hissing the spectrum becomes quite different. There is observable, as said, surrounding the carbon point a yellow flame, which is evidently something very different from the ordinary violet flame which we know to be due to a combination of carbon and oxygen—probably a higher compound of carbon, and oxygen, perhaps with hydrogen. At all events there is quite a different phenomenon which may be watched in all its beauty upon the screen; and the flame may be made to appear and disappear at will by causing the arc to hiss or to be silent. It may also be made to appear almost immediately before the hissing but not very readily. I have photographed a good many of these monochromatic images and they form a very interesting study. I have also photographed the sickle-shaped bands to which I have referred.

Professor W. E. AYRTON: At the beginning of the discussion before Easter, Dr. Thompson criticised the statement, which I had made on a previous occasion, that if you are dealing with normal arcs and keep the P.D. between the carbons constant, then when you draw apart the carbons slowly so as to lengthen the arc you increase the current, and do not diminish it as you might *a priori* have been led to expect. To this Dr. Thompson took exception, and stated that what I ought to have said was that, with the normal arcs, if you draw the carbons apart—if you lengthen the arc in fact—and wish to keep the P.D. between the carbons constant, it is necessary to alter the outside conditions so as to increase the current. That was his criticism. It appears to me, as I said, or implied, on the previous occasion when he made this same criticism elsewhere, that it is a distinction without a difference. His criticism that I put the cart before the horse is like a question as to whether, in dealing with an electric motor car, the motor is in front of the car or the car in front of the motor. As a matter of fact, the criticism he made might have been made just as well with reference to an ordinary statement about the flow of electricity through a wire. Though I do not Professor Ayrton.

Professor Ayrton.

for a moment wish to be regarded as considering that a current through an arc is exactly the same as that through a wire, it is perhaps worth noticing that his objection, if a valid one, will apply equally well. Supposing you have two arcs of different lengths and the same kind and thickness of carbons as Mrs. Ayrton has used, and you wish the P.D. between the carbons in each case to be the same, say 55 volts. With an arc of 4 millimetres there will be 7 amperes, and with an arc of 5 millimetres you have some 11 amperes. Now there are millions of outside conditions, of electromotive forces and resistances which will enable you to have these 55 volts with the shorter arc, and there are millions of other conditions of electromotive forces and resistance which will enable you to have the same P.D. with a longer arc. If you have the two together you must no doubt select outside conditions which will enable you to have your 55 volts in *each* case. But just the same thing applies to a wire. Suppose you have a wire of a certain length and wish to maintain a certain P.D. between the terminals, there are millions of ways in which you can do that, millions of electromotive forces and outside resistances. But the important thing is this : if you stretch the wire and by some proper arrangement of outside circumstances keep the P.D. constant, the current goes down, while in the case of the arc when you do the same thing the current goes up. That is the point to which I wish to draw attention.

I do not know that Professor Thompson quite grasped in his remarks last time that what Mrs. Ayrton told us about that region of instability was really quite distinct from what he referred to, and also distinct from what I have just referred to where the length of the arc is altered. What she was putting forward in this paper was what happens when the length of the arc is *not* altered, what happens, in fact, in the region of instability, or of blankness, as she called it, in which, until two or three months ago, I certainly was myself floundering—a region of both mental and electric blankness. I cannot help stating that I think I observed another form in that region, remarkably like my critic who was also holding up his hands groping for light. What Mrs. Ayrton pointed out was, that if you take a point on the curved line A B C (Fig. 1), the arc is in a state of instability ; it is ready to tumble down, so to say, along one of the inclined lines shown in the figure, and, as Mrs. Ayrton explained to us, along other lines which may not be drawn there. The five-millimetre arc, for example, is in a position of unstable equilibrium, ready to tumble over. Whether it tumbles down the dotted line of the figure or along a steeper or down a less steep and longer line, depends, as Mrs. Ayrton first explained to me this year, entirely upon the E.M.F. of the generator and consequent outside resistance, and that is a fact which one could not deduce from anything that I mentioned some time ago and which Dr. Thompson criticised.

Another criticism made by Dr. Thompson upon which I would break a lance with him was in reference to the slow rotation of the bands which may be seen on the carbon crater before the arc hums even, and certainly before it hisses. Dr. Thompson said last time that he thought that had been pointed out by Mr. Trotter. The curious part about it is that, in Mr. Trotter's paper before the Royal Society, and

in the account given by him in the *Electrician*, of which he was then editor, there is no reference whatever to this slow rotation. Professor Ayrton.

Professor S. P. THOMPSON : I think I can explain exactly. What I said was that Mr. Trotter had shown it to me. It is published in my Cantor Lectures at the Society of Arts as having been shown to me by Mr. Trotter. Professor Thompson.

Professor W. E. AYRTON : I am very glad to hear that, because the curious thing I was going to point out is that, if the fact were commonly known, it is extraordinary that the idea of using a square millimetre of the surface of the crater as giving the standard of light should ever have been put forward by various people, I think even by Dr. Thompson. Professor Ayrton.

Professor THOMPSON : That is quite true, and I abandoned the idea when Mr. Trotter showed me the rotation in question. Professor Thompson.

Professor AYRTON : I may make one more remark on that point. Mr. Trotter was kind enough to invite me to go to his house to see the rapid rotation, that rapid spinning which could only be seen with the rotating sectors, and I have no recollection whatever of his drawing my attention to the slow movement of those dark bands which could be seen without any sectors at all. I have thought about the subject a good deal in the last couple of weeks, and I can call nothing to mind of being shown anything of that kind. Professor Ayrton.

In conclusion, there is one peculiarity in Mrs. Ayrton's method of working, which I recommend to the notice of students—we are all more or less students—but especially to younger students, and that is the method she adopts of studying the known by searching among the unknown ; that is to say, not acquiring knowledge from books—she always says she is profoundly ignorant of the contents of scientific books —but believing that more can be learned from a careful examination of small phenomena, by observation and reflection in fact, than by passing any number of examinations or getting up any number of text-books.

Captain ABNEY : I should like to say that although Professor Thompson has abandoned that unit, I still adhere to it. I think it is a very good unit when it is properly used. Captain Abney.

Mr. H. G. COTSWORTH [*communicated*] : During the reading of Mrs. Ayrton's most interesting and instructive paper, a problem presented itself to my mind, which no doubt the authoress can throw some light upon. Suppose the crater end of the positive carbon is surrounded with a tight-fitting cylinder of some refractory material such as kaolin or lime so as to prevent the crater from extending up the side of the carbon, what now will be the effect of increasing the current to what would have been before the hissing point ? One would suppose that as now the crater was prevented from extending up the side, some new phenomenon would present itself ; of course I am aware that under the high temperature that the kaolin would be subjected to, it would become a conductor, but of sufficiently high resistance, I think, not materially to interfere with the experiment. Mr. Cotsworth.

As regards the remarks of Professor Fleming, it is with some diffidence that I venture to differ from such a well-known authority, but I cannot see how he reconciles the fact that no change takes place in

Mr. Cotsworth. the arc when hydrogen alone has access, as shown by Mrs. Ayrton, with his view that at a high temperature hydrogen combines directly with carbon, causing the arc to hiss; is it not rather that the hissing is still due to the action of the oxygen on the carbon, but in a less degree owing to the presence of hydrogen, which, as it were, averts the attack on the carbon by combining with the oxygen itself, oxygen having a greater affinity for hydrogen than for carbon.

Mr. Trotter. Mr. A. P. TROTTER [*communicated*]: The interest and importance of Mrs. Ayrton's explanation of the hiss of the electric arc is sufficiently obvious, but I regard it as a privilege that owing to the attention which I have given to the appearance of the crater, I can appreciate the ingenuity of her discovery and the completeness of her solution far more keenly than if I had not spent considerable time on the border of the subject. My attention was confined to the phenomena which immediately precede the hiss, and though I have observed enlarged images of a hissing crater on very many occasions, I did so, for the most part, unintentionally. The appearances of the cloud, the mottling, the bright spots, and the colours are most accurately described, but I do not remember the "dark bands in one or more sets of concentric circles moving round different centres in opposite directions" as a common phenomenon of a long arc. This condition of the arc was the other limit of my study, and although in my account [1] of the periodic phenomenon it was stated that it is more marked in a short humming arc, it was suggested that it is always present, In Professor S. P. Thompson's Cantor Lectures (1895) he stated:—"This patch is more luminous than the surrounding surface of the crater, over which it moves in a capricious way. Sometimes you can catch a glimpse of this patch revolving, flickering round and round; sometimes it goes one way, and sometimes the other. You cannot readily observe it, however, unless you resort to artificial means, because of the rapidity of its motion." I considered that before the steady rotatory motion was set up, the phenomenon was in an unstable and ill-defined state, and I paid but little attention to it, and thus missed these bands and figures which Mrs. Ayrton has been the first to describe, and which I look forward to examining when I resume, as I hope to do, my study of the humming arc.

My collection of notes on the rotatory phenomenon made since the date of the paper referred to, contains little that is ready for publication, but I have grounds for commenting on Mrs. Ayrton's statement that "with solid carbons the arc is rounded or pear-shaped according to its length, and has the appearance of great stability. But as soon as hissing occurs the arc seems to suddenly dart out from between the carbons and to become flattened out, as if under the influence of a centrifugal force acting at right angles to the common axis of the two carbons." At the end of Professor S. P. Thompson's account of my work, in his Cantor Lectures, he alludes to my observation of the rotation of the whole arc—that is, of the coloured flame. By stroboscopic methods it may be shown that the pear-shaped flame of the steady arc is the figure of revolution of an arched flame which revolves at the

[1] *Proc. Roy. Soc.*, vol. lvi.

period of the hum (the hum may be too low to be easily audible). With a long arc this can be stopped by a draught of air or by a magnet. This arched flame, held stationary by the up-draught of air with a long horizontal arc, suggested the name arc. I cannot concur with the view that the sudden flattening out of the hissing arc is due to a centrifugal force, because (1) I have carefully examined the hissing crater for periodicity and found no trace of any continuous motion within wide limits, and the change from the hum to the hiss suggests that the periodic phenomenon is at an end, and that an irregular condition has commenced; and (2) because I think that there is reason to suppose that the specific gravity of the arc (the flame) is less than that of the surrounding gases. Mr. Trotter.

Mrs. Ayrton forestalls me in her suggestion of the cause of the rotation, namely, the inrush of the surrounding air, but "a slight breach in the crater" is not very clear, and forms no part of the explanation which I had in view.

That Mrs. Ayrton's explanation of the cause of the hiss seems to her "absurdly simple," so far from removing it from the category of important discoveries, ranks it with many others that enjoy the same distinction, and the proofs are as convincing as the explanation is lucid.

Mrs. Ayrton [*in reply*]: I have to thank all those who have criticised me for the very kindly manner in which they have done it, and for the extremely warm way in which they have welcomed my paper. I have been much struck by, and have received great pleasure from, the appreciation which my work has gained from the scientific world. I was hardly prepared for it. Mrs. Ayrton.

Dr. Thompson asked what I meant by the hissing arc—did I mean the green arc, or an arc in which the P.D. had fallen, or an arc that was making a noise? The arc makes a great many noises; it hisses in all sorts of ways and under all sorts of conditions. But there is only one kind of hiss that is associated with a *sudden* change from purple to green of a part of the arc vapour, and as sudden a diminution in the P.D. between the carbons. It is to this hiss, and this alone, that my explanation refers.

Mr. Mordey has asked if the hissing arc always gave out less light than the silent arc? I believe it does, but it is difficult to say. Less light for what? For the power supplied, I suppose. I have never tried. M. Blondel, in 1893, said, in a paper which he wrote in the *Electrician*, that the intrinsic brilliancy of the hissing arc was always 18 or 20 per cent. less than that of the silent arc. It is true the arc always looks as if it were duller when it begins to hiss, but I have found, by drawing curves, that the power supplied when hissing first begins is generally less than the power which was being supplied when the largest silent current was flowing; and therefore it is difficult to say, without very careful experiment, whether, for the amount of power supplied, the amount of light given out is less when the arc is hissing than when it is silent. As regards the unsteadiness of the arc when hissing, that only lasts while it is at the *hissing point*—that is, while the current is such that the arc is on the borderland between hissing and

Mrs. Ayrton. silence. An extremely steady hissing arc can be maintained when the current is so great that it would have to be *very much* diminished in order to reduce the arc to silence. With regard to the dark band in the alternating current arc, as I have not made experiments on the alternating current arc, I am sorry I cannot help Mr. Mordey on that point.

Letters about my paper have reached me from various people. One suggested that the ends of the carbons in the enclosed arc which I showed were of a very different form from those of the ordinary enclosed arc. I think I made it quite clear that the arc I used was not an ordinary enclosed arc at all; it was an arc enclosed in a crucible arranged in such a way that the air could be let in in different quantities, or entirely excluded. For the purpose of showing that air was necessary to cause hissing, I excluded the air entirely, while leaving sufficient clearance between the positive carbon and the crucible to allow vapour to escape from within. Thus the arc was not what is technically called "enclosed." The shapes of the carbons when the air is entirely excluded are unlike their shapes both with an open arc and with what is usually called the enclosed arc, or a semi-enclosed arc, as it really should be called. The carbons are not quite flat, as they are in a semi-enclosed arc, for which, it may be remembered, rather small currents are used, but they are very much flatter than with the open arc. When the current is greater than about 12 amperes the crater is very much pitted —it is full of tiny holes. How those are caused I do not know. You get the same pitting, but to a much smaller degree, with the open arc when it is hissing. Thinking that perhaps this pitting was caused by the carbon at the end becoming soft and bubbling, I tried bringing the carbons sharply together when a very large current was flowing, and then turning off the current immediately. I expected them to stick together, even if only slightly, but there was no sign of their either sticking or moulding themselves upon one another, as Professor Elihu Thomson[1] found in 1890. I think, therefore, there can be no doubt that the pitting is not caused by any sort of bubbling.

M. Blondel, who has done a larger amount of original work on the arc than any other single individual, and whose opinion is, therefore, of the greatest value, has written to remind me of a theory of hissing that he published in an article in the *Electrician* in 1893.[2] He says:—

"J'ai été frappé d'un fait auquel vous ne faites pas allusion, à savoir que l'arc est transparent dans le régime silencieux ou bourdonnant, et devient brusquement *opaque* dès que l'arc siffle. Cette différence se constate en observant le cratère à travers l'arc sous un angle de 45 dg. J'ai signalé en même temps la production de la lumière verte dont vous parlez.

"J'ai conclu de ce phénomène qu'il y a une différence essentielle entre la constitution de l'arc sifflant et celle de l'arc ordinaire: c'est que le premier est formé de *particules solides* et constitue une vraie décharge disruptive, c'est à dire produite par *arrachement des particules* de charbon,

[1] *The Electrical World*, 1891, vol. xvii. p. 176.
[2] *The Electrician*, 1893, vol. xxxii. p. 170.

tandis que le second est formé de *vapeur* proprement dite, produite par ébullition ou *vaporisation* de carbone. J'ai montré plus récemment que c'est de l'évaporation et non de l'ébullition. Mrs. Ayrton.

"Il n'y a aucune contradiction entre cette théorie et celle que vous venez d'exposer si brillamment; je crois même qu'elles se compléteraient fort heureusement l'une l'autre: car, si je n'ai pas indiqué, faute de la connaitre, la cause du changement de nature de l'arc, votre théorie de son côté n'explique pas, il m'a semblé, *pourquoi* l'introduction de l'oxygène fait que l'arc siffle.

"Je suis porté à croire que la présence de l'oxygène n'agit pas en vertu de quelque action mystérieuse, mais simplement en rendant impossible la vaporisation régulière. Votre expérience semble exclure une influence de refroidissement; il faut donc chercher autre chose. Ne serait ce pas que la vapeur de charbon est brulée aussitôt que formée et que l'oxyde carbone ne serait pas conducteur? On comprendrait ainsi trés bien que l'arrivée de l'air sur le cratère empêche la vaporisation et ne permet que l'arrachement disruptif des molécules.

"En tout cas il y a un point sur lequel j'ai une foi absolue, c'est que l'arc sifflant est disruptif; il est confirmé par les photographies que j'ai faites autrefois en même temps que j'étudiais l'arc alternatif. Je connaissais bien l'influence de la densité de courant pour déterminer le sifflement, et je pensais seulement que si le cratère est trop petit pour le débit qu'il a à fournir, il faut que la vaporisation lente du charbon soit remplacée par un arrachement plus rapide, trop rapide pour que les molécules arrachées passent à l'état de vapeur transparent.

"Vos expériences me font voir que cette dernière idée est sans doute fausse, mais elles ne démontrent pas la fausseté de la précédente."

I have noticed an increase in the opacity of the arc vapour when hissing begins, but have found it neither so great nor so invariable as has M. Blondel. This may be because we burned the arc under different conditions, for I have found that, if the carbons are placed so that the whole crater is in view, the crater will sometimes be partially hidden by the vapour, but sometimes it can be seen as clearly as if the arc were silent. This is, therefore, a point that requires further investigation.

This variation in the opacity of the hissing arc would appear to show that, if M. Blondel's theory of a disruptive discharge of solid particles is correct, the particles must vary either in size, or in number, or in both, according to conditions yet unknown. M. Blondel suggests that when the air reaches the crater, and the arc, therefore, begins to hiss, it is because the carbon vapour is burnt as soon as formed, thus producing carbonic oxide, which, not being a conductor, stops the vaporisation of the carbon, only permitting a disruptive discharge of the particles. It seems hardly possible that the diminution in the P.D. between the carbons when hissing begins can be caused in this way, for the formation of a non-conducting material near the crater, such as M. Blondel supposes the carbonic oxide to be, would surely tend to *increase* the resistance of the arc, and thus to *increase* the P.D. between the carbons rather than to diminish it. Further, this increase of resistance would take place just where the greatest *diminution* actually takes place, namely, near the crater.

Mrs. Ayrton. I entirely agree with Prof. Fleming as to the hopelessness of attempting to explain any single one of the numerous and complicated phenomena of the arc without having previously formed a perfectly definite conception of the action of the arc. Such a conception I have naturally formed, one which, while differing greatly from Prof. Fleming's, makes a very good working hypothesis. As, however, the truth of the hypothesis depends on the existence or non-existence of a large back E.M.F. in the arc, and as I do not consider that the existence of such an E.M.F. has been either proved or disproved, I hold myself in readiness to change my hypothesis whenever any fact bearing upon this back E.M.F. shall be brought to light, and prefer, therefore, to keep my theory at present for home use.

The intermittency of the current with a hissing arc, first noticed by Messrs. Frith and Rodgers in 1896, can, I think, be easily explained, without the intervention of a cushion of negative ions near the crater, thus:— When first the air gets to the crater it burns some of the carbon, thus forming a gaseous covering over the part burnt. This, till it is dispersed, protects the crater from the air, just as the carbon vapour protects the crater of a silent arc. As soon as the products of combustion are dispersed, however, the air gets direct to the crater again, and so on. Hence, if this explanation is correct, it is the intermittence of the air current that causes the intermittence of the electric current. That the air current *is* intermittent, I proved experimentally some time ago, by fastening one end of a single asbestos fibre to the edge of the hole in the cover of the fireclay crucible in Fig. 8, in such a way that its free end stretched out horizontally in the space between the positive carbon and the lid of the crucible. The space was wide enough to allow air to enter the crucible, so that the arc could be made to hiss. It was then found that while the arc was silent, the fibre remained fairly motionless, but that as soon as hissing began, it vibrated rapidly up and down, instead of being sucked into the crucible as it would if there had been a steady inward current of air. This intermittent current of air would naturally cause an intermittent electric current, because the crater would be alternately acted on by the air and not acted on by it. The vibration thus set up probably also causes the sound given out by the hissing arc.

Prof. Fleming's suggestion that *any* gas that would combine with carbon at a high temperature would cause hissing is one that naturally presents itself, and one that I had in my mind when I tried the effect of hydrogen; but hydrogen does *not* cause hissing except in the presence of oxygen; hence, either something more is required to produce hissing than a gas which unites readily with carbon at a high temperature, or the temperature of the crater is *too* high to allow carbon and hydrogen to unite, except in the presence of oxygen.

It is not at all surprising that Captain Abney should long ago have been troubled by the slow rotation and oscillation of bright and dark bands over the crater that I have described. Considering the nature and the closeness of his investigations, he must have noticed many interesting phenomena, which, if published, would materially assist those who are attacking the physics of the arc from a point of view different from his own. His observation about the yellow flame which springs

up simultaneously with the beginning of hissing is very valuable, and will, I hope, lead to a further unravelling of that still mysterious phenomenon. Mrs. Ayrton.

I have never covered up the end of the positive carbon with lime or kaolin, in the manner suggested by Mr. Cotsworth, but I have tried a jacket of fireclay with very unsatisfactory results. When the arc had been started some little time, the fireclay began to boil and bubble, the P.D. dropped to about 20 or 25 volts, and the arc became pale green and gave out very little light, besides being very unsteady. I think, however, it would be well worth while trying other substances, especially lime.

The very interesting discoveries that Mr. Trotter has made about the humming arc, together with his long and close study of that phenomenon—one bordering so closely on hissing—give a peculiar value to his contribution to this discussion. As an addition to the first part of this contribution, I may quote the following words from a letter I received from him on May 22nd :—

"I am contributing to the discussion. Since posting this contribution, it has struck me that it is perhaps because I generally used an inclined, and often a horizontal (Crompton projector) arc, that I missed seeing the pre-rotatory bands and figures that you describe. The lateral draught of air may have dragged the flame to one side."

With regard to what Mr. Trotter says about the hissing arc, there is every reason to believe that he is right in thinking that the hiss is an irregular and not a regular periodic phenomenon, such as he has shown the hum to be. I still retain the idea, however, that the flattening out of the hissing arc is due to centrifugal force, but it now seems to me likely that when hissing begins the arc splits up into various portions, each whirling round a common centre at a different rate. I shall look forward with the greatest interest to the publication of Mr. Trotter's further observations on the humming arc, which cannot fail to throw more light on the hissing phenomenon.

I have here carbons which were taken with the open and the enclosed arc under the same circumstances. The arc was 2 mm. in length in each case, and the currents were 6, 12, 20, and 30 amperes ; the current was kept constant for some time, and then suddenly shunted into another circuit, so that there should be no period during which the current was less or different from what it had been while the carbons were formed. Hence the permanent shape taken by each of the carbons under the given conditions can be plainly seen. It will be noticed that with the arc enclosed in the crucible, the end of the positive carbon is nearly cylindrical with all currents, and has a wider, deeper crater than with the open arc ; also that the crater is very deeply pitted with currents over 6 amperes, and that it is covered with a sort of light grey skin which readily peels off. It may be mentioned that, whether the arc be open or enclosed, the ends of both carbons are composed of graphite, with which you can write as readily as with a lead pencil, and that the depth of the layer of graphite appears to increase as the current increases. Also that the mushroom formed with a short hissing arc is entirely composed of graphite.

The President.

The PRESIDENT: I have now the pleasure of asking you to pass a vote of thanks to Mrs. Ayrton for her excellent paper. It is the first paper we have had the pleasure of receiving from Mrs. Ayrton; I sincerely hope that it will not be the last. I think we are all the more indebted to her for communicating this most valuable paper to us as she is not a member of the Institution. We have not the honour of numbering amongst us any lady members, but I do not know of any legal disability against ladies becoming members. If not, I hope we may look forward to the pleasure of numbering Mrs. Ayrton among the members of the Institution before long. I am sure we shall all welcome her as an associate. I ask you to pass a hearty vote of thanks to Mrs. Ayrton.

The motion was carried by acclamation.

The PRESIDENT: We now pass to the discussion of the elaborate paper of Messrs. Duddell and Marchant which occupies a large portion of Part 138 of the Journal. In connection with the discussion we are to have presented to us some very interesting experiments in illustration of the paper. I will ask Mr. Duddell to be kind enough to proceed with that part of the paper which requires the experimental illustrations.

EXPERIMENTS ON ALTERNATE CURRENT ARCS BY AID OF OSCILLOGRAPHS.[1]

By W. DUDDELL and E. W. MARCHANT, Associates.

[Mr. Duddell here demonstrated the use of his Oscillograph, projecting on the screen the curves traced by the spots of light reflected from the mirrors of the instrument.]

Mr. Andrews.

Mr. LEONARD ANDREWS: I am delighted to have had the opportunity of seeing Mr. Duddell's most fascinating experiments. They have been particularly interesting to me, because during the past two or three years I have also been experimenting in somewhat the same direction, though I cannot pretend to have obtained curves of anything like the same accuracy of shape as the Authors'. The apparatus I first used consisted of an optical lantern, from which rays of light were projected on to concave mirrors attached to the suspended coils of a modified form of Ayrton and Mather galvanometer. From these mirrors the light was reflected back on to a slot, behind which a rapid photographic plate was dropped as in the Author's experiments. With this arrangement I succeeded in obtaining curves, the amplitude of which showed approximately the strength of the current at the time. As, however, I could only get about 10 curves on the plate, and I required a record of at least 100 consecutive curves, I tried mounting a number of plates on a drum and caused them to revolve behind the slot. This arrangement was too heavy and cumbersome to be a success, and was consequently replaced by a Kodak film mounted on a very light wooden drum; but as I was unable to obtain films as rapid as the plates, I

[1] The paper is printed *in extenso*, on pp. 1–100 of this volume.

Mr. Andrews.

experienced some difficulty in obtaining a photographic record on these films; in fact, I have never been able to do so when using the optical lantern as a source of light. The arrangement I have used for my more recent experiments is shown in Fig. A.

a a_1 are the carbons of a simple hand-fed arc lamp, P is a counter-sunk pinhole in a brass plate about a ¼ inch thick; with this I have obtained a better result than with lenses. T is a rectangular tunnel, and S_1, S_2, S_3 are stops for cutting off all light except the direct rays between the arc lamp and the mirrors. The reflecting galvanometers referred to above are now replaced by telephone receivers, A, B, C, the concave

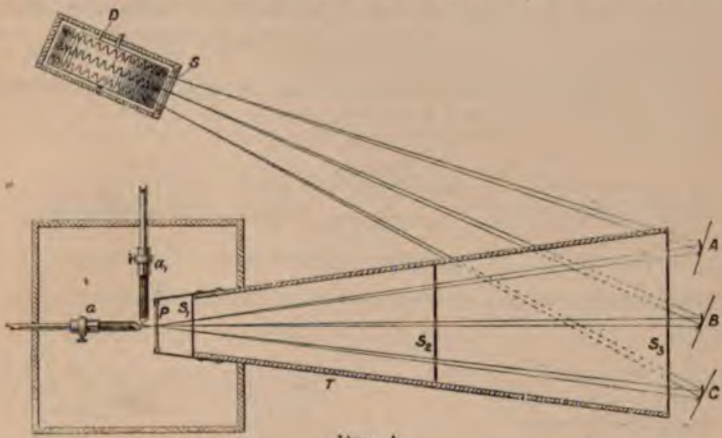

FIG. A.

mirrors being attached eccentrically to the diaphragm. For obtaining records of very small currents or for tracing E.M.F. curves the ordinary telephone winding is used; but for currents of from 10 to 50 amperes the fine coils are removed and one turn of wire is wound round the magnets. The Kodak film is fixed on a light drum D, about 8 inches in diameter. This is mounted in a light tight box, provided with an electrically controlled shutter S.

With this somewhat crude apparatus I have been able to obtain records of the rapid changes of phase differences and variations of current strength that occur during a break-down which have been of great assistance to me in designing alternate current automatic cut-outs.

Professor Ayrton.

Professor AYRTON: May I ask Mr. Andrews whether he has made any experiments to see whether these curves are accurate? The essence of the communication we have had this evening is the fact that the authors have shown, by those curves projected on the screen, that the Oscillograph method gives the same results as would be obtained by Joubert's point by point method, supposing the waves lasted quite steady for a sufficient length of time, half-an-hour or whatever it may be, to enable the point by point method to be employed. All such methods as the one which we have just heard described fail, I am afraid, in accuracy. I think you will find that the natural vibrations of the

Professor Ayrton.

telephone diaphragm itself superimposed on those which you desire to record, will render your curves not true ones. It must not be forgotten that nine years ago Frölich tried this same sort of method, but had to abandon it entirely on account of the inaccuracy of its results. It will give waves and so on, but the membrane would not allow the waves recorded to be the true waves which were wanted. Just as the telephone does not give you the speech of a person with perfect accuracy, but superimposes harmonics of its own, and the phonograph does the same, so all these early methods failed for the same reason. I end by again saying that the essence of this method, which has been worked at for years, is to obtain an instrument which is sufficiently modest to record what it is asked to record, and not put itself forward and interpose its own vibrations.

The President.

The PRESIDENT : I have to announce that the scrutineers report the following candidates to have been duly elected :—

Members :

Henry Martin Peikert.
Frederick James Satchwell.

Associate Members :

Francis James West Ashlin.
Sidney George Brown.

Associates :

Louis Emile Aulagnier.
John Shaw Barnes.
Norman Ellison Beves.
Lieut. Francis Morton Close, R.E.
George Richard Drummond.
William John Ferguson.
Stephen Arthur Hardstone.
William Richardson Protheroe Hobbs.
Victor Martos.
John B. McIndoe.
Kenric James McMullen
Edward Sheppard.
Alfred Sims.
Frank Heathcote Wilson.

Students :

George Hildebrand Burgmann.
Charles Fitzroy Farlow.
E. A. Rosenheim, B.Sc.
George Stevenson.
George Nugent Merle Tyrrell.
F. R. Webb.

NOTICE.

1. The Institution's Library is open to members of all Scientific Bodies, and (on application to the Secretary) to the Public generally.

2. The Library is open (except from the 14th August to the 16th September) daily between the hours of 11.0 a.m. and 8.0 p.m., except on Thursdays, and on Saturdays, when it closes at 2.0 p.m.

An Index, compiled by the late Librarian, to the first ten volumes of the Journal (years 1872-81), and an Index, compiled under the direction of the late Secretary, to the second ten volumes (years 1882-91), can be had on application to the Secretary, or to Messrs. E. and F. N. Spon, 125, Strand, W.C. Price Two Shillings and Sixpence each.

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NOTICES.

In future, the Volume of the Journal of Proceedings will commence with the report of the first meeting of the Session, in November.

This part (No. 141) is therefore the last number of Vol. XXVIII. and, as usual, there will be no further issue of the Journal until the end of the year.

The Index to Vol. XXVIII. is issued herewith.

Attention is specially directed to the letter, placed within the cover of this number of the Journal, relating to PREMIUMS AND SUBJECTS OF PAPERS.

INSTITUTION OF ELECTRICAL ENGINEERS,
***August*, 1899.**

Walter M. Arnot.	Francis Harrison.
Nigel Harrington Balfour.	J. Ranald Macdonald.
James Douglas Dallas.	H. E. Moul.
Sydney Evershed.	Allen Francis Scott.
Arthur H. French.	Oliver Tibbits.
S. H. Gowdy.	Robert Wardell.

Arthur Wright.

Mr. C. H. W. Biggs and Mr. Hilton Johnson were appointed scrutineers of the ballot for new members.

Donations to the Library were announced as having been received since the last meeting from Mr. A. H. Gibbings and Mr. C. H. Wordingham, to whom the thanks of the meeting were duly accorded.

The PRESIDENT: In accordance with No. 45 of the Articles of Association I have to announce the names of the Members, Associate Members, and Associates nominated by the Council to fill the vacancies occurring in the Council at the date of the Annual General Meeting:—

MEMBERS NOMINATED BY THE COUNCIL FOR OFFICE DURING THE TWELVEMONTH 1899–1900.

As President.

New Nomination.	Professor SILVANUS P. THOMPSON, D.Sc., F.R.S.

As Vice-Presidents (4).

Remaining in Office.	Professor JOHN PERRY, D.Sc., F.R.S. W. E. LANGDON. JAMES SWINBURNE.
New Nomination.	ROBERT KAYE GRAY.

As Ordinary Members of Council (15).

Remaining in Office.	S. L. BRUNTON. Professor J. A. EWING. W. P. J. FAWCUS. P. V. LUKE, C.I.E. E. MANVILLE. W. M. MORDEY. J. S. RAWORTH. A. A. CAMPBELL SWINTON. HERBERT TAYLOR. CHARLES HENRY WORDINGHAM.
New Nominations.	Major PHILIP CARDEW, R.E. JOHN GAVEY. ROBERT HAMMOND. A. J. LAWSON. R. P. SELLON.

As Associate Members of Council (3).

Remaining in Office.	SYDNEY EVERSHED.
New Nominations.	R. W. WALLACE, Q.C. ARTHUR WRIGHT.

HONORARY OFFICERS NOMINATED BY COUNCIL FOR THE TWELVEMONTH 1899–1900.

As Honorary Auditors.

For Re-Election. { F. C. DANVERS. / E. GARCKE.

As Honorary Treasurer.

For Re-Election. Professor W. E. AYRTON, F.R.S., Past-President.

As Honorary Solicitors.

For Re-Election. Messrs. WILSON, BRISTOWS, and CARPMAEL.

I have further to announce that the Council have had under consideration a visit of members of the Institution to Switzerland. There is much that is interesting in connection with electrical engineering to be seen there, and it was, I think, unanimously felt that such a visit would be both agreeable and useful if it could be carried out. With the view to ascertaining the feelings of the members on the point a letter has been drafted and will be duly sent by post to the members of all grades, giving a general outline of the project. The idea is that this Swiss meeting, if carried out, should begin on the 1st of September and terminate on or about the 10th of that month ; but, as the letter will inform you, there are certain possibilities of making the time elastic and within the discretion of individual members.

We now proceed to the renewed discussion which was broken off at the last meeting. I shall be glad if Mr. Andrews will commence the discussion and complete what he had to say.

Mr. L. ANDREWS : Professor Ayrton inquired at the last meeting if I had taken any steps to ascertain the degree of accuracy of the curves of which I have spoken. I have done so, and am satisfied that the curves I have obtained are by no means so accurate as those shown by the authors of the paper. Mr. Andrews.

I understand, however, that one of the advantages claimed for the Authors' apparatus is that it shows the shape and *position of each curve relatively to other curves* throughout any particular experiment, and that I think is almost, if not quite, as important as getting the actual shape of the curves. In central stations things are very liable to occur which completely disarrange the whole working of the station, and they generally happen so rapidly that no one has any idea of what actually

Mr. Andrews.

causes them, or how they may be prevented. There was, for instance, much correspondence as to the cause of the Brighton breakdown, when all the lights were put out, a few months before Christmas. Several theories were suggested; but I believe no one has any knowledge of the real cause. Mr. Raworth suggested to me a few years ago that it would be interesting to get a photographic record of the current curve and the the E.M.F. curve during a breakdown. That is what I endeavoured to do; and the accompanying diagram (Fig. B.) is a record of what happened during the space of one second of time, when the field-circuit of one Mordey alternator, working in parallel with another, was suddenly opened. A and B are the current curves of two alternators connected in parallel; C is the E.M.F. curve taken across a transformer excited off the 'bus bars. When the experiment was made, the two machines were run on the circuits, but we had only about 8 amperes on at

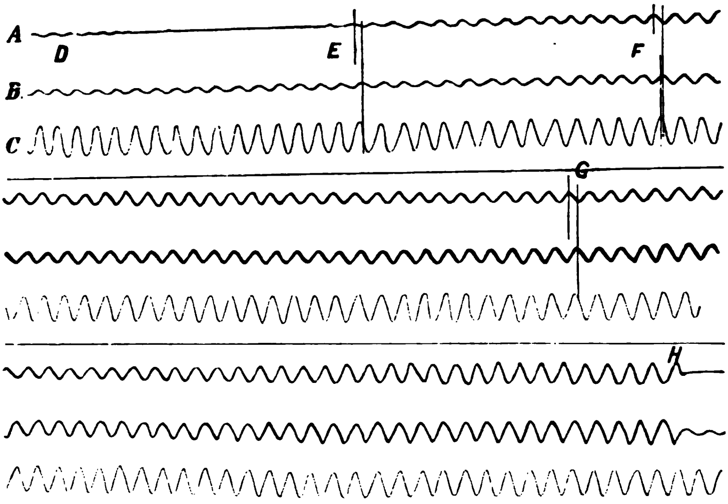

Fig. B.

the time, chiefly exciting transformers. At the commencement of the record, viz. at D, neither of the machines was doing very much; there is very little current shown, but both the curves are exactly in phase. Three or four hundredths of a second later I practically opened the field—I did not actually open it, but switched in with a quick-break switch a resistance which reduced the field-current by 50 per cent. Again, a few hundredths of a second later, the current in the machine in which we opened the circuit gradually increased, and the current of the other machine also increased correspondingly. The current in the machine which was opened then began to lag behind. At E, where the current is beginning slightly to increase, it is about 100° behind the current in the generating machine. At F and G the top curve A is lagging about 140° behind the middle curve C. Throughout the remainder of the record this lag remains approximately the same, viz., about 140°.

The lag of the shunt curve behind the current curve B is approximately 45° throughout the entire record. At H, when the current in A and B had increased to about 30 and 35 amperes respectively, the discriminating cut-out protecting the generator connected to A operated and so disconnected this machine; this caused the current in A to fall to zero and that in B to the 8 amperes on the circuits. Mr. Andrews.

Mr. W. M. Mordey: I intervened at the close of the last meeting in favour of an adjournment of the discussion not because I wished to discuss the paper, but because I felt that many would be glad of a fuller opportunity for expression of appreciation of what we had all heard and seen with so much pleasure. For my own part I may say I keenly enjoyed the experiments—it was the most beautiful demonstration I have ever seen. It was full of interest from beginning to end. Mr. Mordey

The authors have given us an excellent instrument, beyond comparison superior to any apparatus previously introduced for the purpose of examining these curves. It must have been the outcome of most painstaking study and experiment, and the authors are to be congratulated on the result. If they had done nothing more than put that almost perfect instrument before us, they would have deserved our thanks. But they gave us also a very interesting demonstration of the use of the instrument in the study of arcs, and, in addition, we have the excellent paper they have written. I should like to congratulate Mr. Duddell, who explained the diagrams, on the excellent and clear way in which he did it. It was evident that he loved his subject, and that he was speaking with a very full knowledge of it.

I hope to see this instrument used at some future time on other matters of equal interest, and perhaps of greater importance; for example, an examination into the reactions between alternators in parallel with different kinds of wave forms, with different kinds of loads, and with different varieties of driving. We now have in the instrument under discussion a means for the examination of some of these phenomena which are so important in alternate current work. The practical problems pressing for solution some years ago had to be solved, and were solved more or less satisfactorily, by rough methods and painful experimenting. There is now provided a means for going over the ground in detail, and I doubt not the gleaner's harvest will be a good one.

In conclusion, I would like to congratulate Professor Ayrton and the Central Technical College on having turned out this excellent piece of work.

Dr. J. A. Fleming: Messrs. Duddell and Marchant have made, in this method of delineating and projecting on the screen alternate current curves by their beautiful apparatus, as great an advance on all older methods as the power loom made on the hand spinning-wheel. I can speak feelingly on this subject, because some three or four years ago, in conjunction with Mr. Petavel, I projected a campaign which was originally intended to cover very much the same ground. We were able to work out a fairly satisfactory point-by-point method of describing these curves, but we found there was often little to show for a whole day's work. Hence, at the end of about three months, we Dr. Fleming.

Dr. Fleming relinquished the experiments. But now by this apparatus the authors of the paper under discussion have been able to do more in an hour than could have been done by the point method in a week. One of the great advantages which it seems to me their method possesses, is that they are able to exhibit and to delineate the evanescent curves which last only a moment or two, and which flitter about, as those curves on the screen did last time, from one form to another. Of the experiments shown last time, the experiments with the arcs formed with one pole of carbon and the other of metal interested me most. The authors have shown that with the metal-carbon arcs the current only flows when the metal is the positive pole; and when it forms the negative pole the current is interrupted, except when the arc is very short; in other words, the arc under those conditions possesses a kind of unilateral conductivity. In 1890 I showed experiments with the incandescent lamp which had a close analogy to these effects. The lamp was worked with continuous currents. I found that, if a galvanometer and a single cell were connected between a middle insulated metal plate and the negative terminal of the lamp, the current could only flow across the vacuous space when the negative pole of the cell was in connection with the hot carbon, the positive pole being in connection with the metal plate. Under these circumstances it will flow across the space and give an indication on the galvanometer. If the connection is reversed, it will not do so unless the plate is red-hot. I also showed that if the galvanometer and a single cell are placed in series with a vacuum tube having carbon electrodes it will not send the current if the electrodes are cold; but if the negative electrode is made hot, then the current will flow through. That has a close analogy with this unilateral effect which the authors of the paper have found. I think I may venture to predict that, if in their unilateral zinc-carbon arc they will heat the metal pole with an oxy-hydrogen blow-pipe, so as to keep it hot, they will then find the arc will not be unilateral. The unilateral arc is, I believe, due to the very rapid cooling of the metal which takes place when the metal is about to be negative. Take a case when the metal is the positive pole of the arc, then the current flows. As the current reduces to zero and is going to reverse, the metal cools off, and by the time the E.M.F. has reversed, the metal is too cold to pass the current, and the unilateral effect is found because of the much greater conductivity of the metal pole. It would be worth while to pay particular attention to the non-arcing metals in this respect. It is well known that Wurts found—and that is the principle of the well-known lightning arrester—that an arc cannot be formed between certain non-arcing metals. It is a curious thing that those are the very metals which chemists tell us have monatomic molecules.

With regard to the electrolytic theory of the arc, I assume that something of this kind is going on: the carbon molecules are broken up into carbon ions, probably by their collisions in the vaporous condition, and that these ions are drawn in the opposite direction by the electric force, and in order that that process may continue it is necessary that the molecules should be capable of being split up into ions. If they are monatomic metals the process cannot go on, and that may be the

reason why the monatomic metals, zinc, cadmium, mercury, magnesium, cannot have electric arcs between them when the surfaces are close together. Dr. Fleming.

I am glad to see from the paper that one of the conclusions supported, to which Mr. Petavel and myself were led some four years ago, viz., that the luminous efficiency of the alternating current arc is increased by lowering the frequency, is confirmed. Another thing which we pointed out (which I do not think is referred to), is that there is a distinct lag between the light curve of the alternating arc and the current; they are not in step with one another; the moment of maximum illumination of the crater carbon happens later than the moment when the current reaches the maximum by a very marked interval. The study of the alternate current arc, I venture to think, is the most probable avenue of approach to a full comprehension of the phenomena which are taking place in the continuous current arc, and therefore I think that the interesting experiments which have been described in this paper afford very valuable material indeed for a discussion of those processes. And in particular I am inclined to think that a more extended examination of the effects produced by non-symmetrical arcs of different metals or of carbon and metals will be exceedingly useful in bringing to light the phenomena which underlie the electric arc in general.

Professor SILVANUS THOMPSON: I need hardly say that I have the most profound admiration for the demonstration which was made last time, and for the very beautiful instrumental appliances with which that demonstration was made. Professor Ayrton in the first place, even more than the authors of the paper, and the City and Guilds of London Institute as a whole, are to be congratulated on such admirable work having been produced in the laboratories of the Central College. I have given a good deal of time to reading through this elaborate research, and every time I resume that reading I am the more impressed with the admirable piece of work which has been done. So far as the appliances are concerned I regard it as an instrumental triumph. I seldom see work directed so carefully, and evidently so patiently, rewarded with such a complete and beautiful result. Things which would have taken months and years to work out by the appliances which were available until only a few months ago, can now be examined in the course of a few moments. That alone would justify us in regarding this as a very important paper, and in being glad that our Institution has been the means of publishing it to the world. Professor Thompson.

The paper raises a number of exceedingly interesting problems besides those which it settles; and amongst the points which one naturally looked to was this. It must be known to everybody who has worked with an alternating current arc that the current through the arc does not entirely keep step with the voltage. Directly one begins to try to measure the power through an arc by the three-voltmeter method, for instance, one finds that the voltmeter indicates a dislocation of phase. Then one asks one's self, Is this arc acting so as to produce a lag, as a self-induction would, or to produce a lead in the current, as a capacity would do? We turn to Mr. Duddell's diagram and make

Professor Thompson.

the interesting discovery that there is no lag in a great many cases, that the curve of potential difference, rising and falling, starts off at absolutely the same place as the current curve and ends at the same place as the current curve on the horizontal line; so that at the beginning and end there is neither lag nor lead; yet, in spite of that, the arc acts so that in some way or other it is out of phase. The peak of the voltage curve and the peak or peaks of the current curve, and not the place where the curves cross the zero line, are clearly

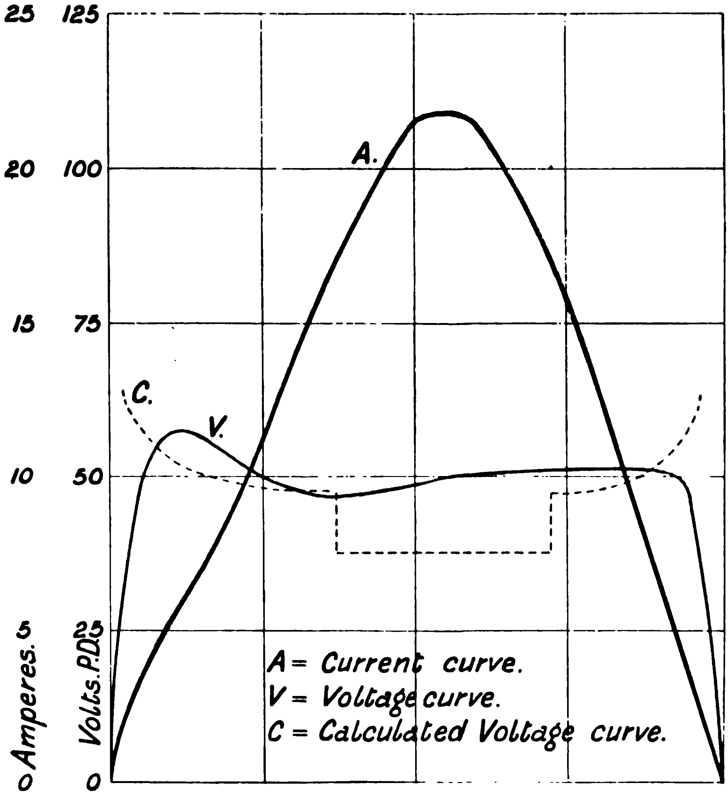

FIG. C.

responsible for the phenomenon of the difference in phase. This is interesting, because I remember, not many years ago, seeing in Major Cardew's laboratory in Whitehall an instrument which I was told was to measure very simply the amount of lag of a current behind the E.M.F. But on examination I discovered that all that it did was to measure the difference in point of time between the place where the voltage curve and the current curve crossed the line. If that be regarded as lag, then in a great many cases there is no lag at all. When one follows this a little deeper, one finds there

Professor Thompson.

is further information forthcoming. Then the question naturally arises :—Ought one to be able, from what one knows about continuous current arcs, to predict any of those curves which have been examined and produced experimentally? Messrs. Duddell and Marchant have worked very carefully on solid carbons known as Apostle carbons, and it so happens that Mrs. Ayrton's experiments on the continuous current arc were also to a large extent conducted with carbons of the same kind. Therefore, always allowing for possible differences in the conditions, we have at any rate a possible basis for examining whether the phenomena of the alternate current arc can be deduced from the experiments with the same kind of carbon. I have attempted to do that. Now, taking the first sheet of diagrams in the paper under discussion, there is an example given—I think it is is marked No. 6—of experiments made with Apostle carbons, current 14·8 amperes, arc length 3 millimetres. The curve, as a matter of fact, I have reproduced (Fig. C) on a diagram on the wall. The curve of the current rises with a fairly simple form, almost a sine curve; but the curve of voltage across the terminals of the arc rises much more abruptly and much more steeply, and has a small front peak; it runs down and is nearly flat for a considerable distance, and then rises slightly and bends down again. That is an experiment with carbons of the same kind, I presume, but not of the same size as those which Mrs. Ayrton used in some of her experiments. Taking Mrs. Ayrton's standard set of experiments, the accuracy of which is beyond dispute, and looking to see what voltage corresponds to the different values of current around that first half-period for arcs of the same apparent length, namely, 3 millimetres, one finds that, plotting the corresponding voltages out, one gets a curve which is marked C on the diagram; whereas the observed curve is that marked V. The calculated one from Mrs. Ayrton's results begins a little higher and comes a little lower. But Mrs. Ayrton was using 11-millimetre carbons instead of 13. If she had had a 13-millimetre carbon we should not have had the sudden drop which is due to hissing, but the curve would have gone very nearly straight across; in fact the shape of the curve would, so far as the middle part of that diagram is concerned, have almost exactly coincided with that of the observed curve. But at the ends there is a very great difference. Where the current begins it is very small, and at the end of the half-period it has also become very small. To argue from the continuous current observations an enormous voltage would be required to send that current through a 3-millimetre arc, and the two ends of the calculated curve would go up, I suppose, to infinity, which clearly shows that the alternate current arc, both at the beginning and at the end of the half-period, conducts a great deal better than a continuous current arc with a similarly small current. At the beginning of the half-period there is carbon vapour left behind, and at each end of the half-period it is easier for the current to pass through, and consequently the voltage does not rise to those heights. You do not have a sort of inverted hyperbola, you have a peak rising steeply and falling steeply at the end. To a certain extent one can evidently predict from the

Professor Thompson.

continuous current experiments the form of the potential difference curve for a given current. I have taken that case, in particular, because it seemed to me to be about the most suitable case for comparison, as it is one of the few cases where, by the insertion of self-induction in series with the arc, the current was itself rendered comparatively invariable; that is to say, by the self-induction in the circuit the current itself was to a certain extent rendered less flexible. It would be less liable to have this curve altered or perturbed by any reaction in the arc itself, and therefore I particularly chose that.

If, instead of No. 6 in the series, one takes curve No. 2 where there was no such self-induction, where there was merely a resistance in series, and therefore where any reaction in the arc itself could more easily affect the current, the potential difference rises to a higher peak and rises much more suddenly, and the current finds much more difficulty, apparently, in starting. In fact, there is less E.M.F. propelling it, owing to the fact that the induction in the circuit is no longer helping to keep the current to constant wave-form. One thing comes out very clearly, that the little front peak to which Mr. Duddell drew our attention to a very large extent depends upon the resistance of the E.M.F. in the rest of the circuit, that that is responsible for forcing the current up, and that therefore, finding an arc in the state of hardly having begun with a bad conductivity in it, the potential is forced up so that the current may begin. Now, the rise and fall of resistance in the arc may also of course be obtained by comparison of the curves, and it is exceedingly interesting to see how very closely--I am not speaking of those peculiar things with metal at one end and carbon at the other, but with an ordinary carbon arc—if we work out the resistance of the arc from the relation between the current curve and the potential difference curve, the resistance of the arc seems to be almost inversely as the current through it; that is to say, as the current passes through the arc, it raises up a column of vapour having a cross-section which increases with the current, and therefore the resistance goes down as the current becomes large. It is not so at the very beginning or the very end—the curves have fallen away there.

Now all this brings one back to the question whether the arc acts as a self-induction or as a capacity. A self-induction would tend to make the voltage lead, or in other words would force up a peak in front. A capacity would tend to make the voltage lag, or would force a peak at the back. Both effects in fact occur. But, owing to the time lag of the temperature, the former is predominant. In other words, the "flame"—the hot column of vapour—takes time to grow and takes time to die away; hence the dissymmetry. The conductance is greater during the dying of the arc than during its growing, and the quasi-self-induction which makes the front peak exceeds the quasi-capacity which is responsible for the hind peak.

There is a remark on page 89 of the paper which I think deserves a little attention: "If a resistance in any alternate current circuit is replaced by an arc which transmits the same root-mean-square current, the wave forms of P.D. between its terminals and current *through* it are no longer the same; and the root-mean-square

value of the P.D. between its terminals is increased." I do not know whether attention is drawn to that as though it were something unexpected, but there is no explanation of it given. I want to suggest that the explanation of that is really very simple; that it depends on the circumstance that you are no longer dealing with constant resistance. The resistance of the arc is large when the current is small; because with a small current you have a thin column of vapour, and therefore a larger resistance. When the current is large you have a smaller resistance. Now if you have during the time of a small current a large resistance, and during the time when there is a large current a small resistance, and you are passing that current through the resistance, the voltage will have to vary in an inverse way—it will have to vary directly as the resistance: and consequently the mean square value of the voltage, and therefore the square root of the mean square, is bound to be a larger quantity than if the resistance had been constant. Though you may be apparently working with the same voltage at the terminals, and the same current through the arc, yet necessarily the root-mean-square value of the potential difference between the terminals will be increased if the resistance is changing in that way. If you could produce a case when the resistance was large when the current was large, and small when the current was small, then I suppose the converse ought to hold good.

Professor Thompson.

There are a number of points which are not altogether clear, and I would ask Mr. Duddell if, in his reply, he would kindly throw a little more light on one or two of the things which I have found some little difficulty in understanding. He says at the beginning what was to be the plan of the experiments: "In every experiment, a certain length of arc and current having been selected, the speed and excitation of the alternator were kept constant, and the distance between the carbons was regulated so as to keep the P.D. between them constant during the whole course of the experiment." That having been set down as the plan, we begin to look at the diagrams. Take the very first sheet. We discover a certain size and quality of carbons, current, 14·8 amperes, frequency, resistance in series, and so forth, and it is not at all clear which of these things is the dependent variable, and which is the independent one. For example, in the second diagram, which is given to compare the solid arc with the resistance. The current there, I presume, is what is allowed to vary and take its chance, the alternator being at a certain speed and excitation. There is no attempt to keep the current constant in shape, but it is set down to be 14·8 amperes, so that apparently it is limited in quantity. Then, if I understand the plan of campaign, the arc length is to be varied until you keep the voltage reading constant. Then the length is the dependent variable. You are continually shifting the length backward and forward in order to keep the voltage constant. And yet it is stated that the length is 3 mm., as though this were invariable. I want to know which it is that is altered to keep th[e] other constant, in order to observe something else varying. They [cannot] not all be varying, and they cannot all be fixed. I want to know [which is] the cause and which is the effect, in order that I may not, i[n] these remarks, be putting the cart before the horse. T[h]

Professor Thompson.

tions incidentally that M. Blondel in one of his experiments endeavoured to keep the current constant. What did M. Blondel try to keep constant? The mere number of amperes as measured by the ampere meter, or the shape of the curve? If he had put in an alternator, for instance, either with a large amount of self-induction or with high E.M.F., with very big resistance in the rest of the circuit, he would have been almost independent of reactions, and the current would then be constant not only in apparent amount but in its wave-form. Now in trying to understand exactly the method in these experiments it is very difficult to see what it is that is being kept constant. Is it the root-mean-square value, or are conditions being introduced to try to keep the form constant? Is it an invariable quantity which is being sent through, or is it an invariable shape which is being impressed? One cannot in all cases gather what is the thing which it has been attempted to keep constant, and what is the thing which has been allowed to vary. But although I ask for this information, I would not for one moment let it be thought that I wish to undervalue the enormous amount of work which is represented here.

Professor Ayrton.

Professor AYRTON: It will be out of place for me to add to the praise of the laboratory which I happen to look after; I can, however, praise very much those two students whose names are attached to this paper for the indefatigable way in which they have worked at the subject for some years continuously. And perhaps the greatest compliment that could have been paid them has been paid by M. Blondel, who has done more work on the arc than any other person living. He is also, I think I am right in saying, the father of oscillographs—the very name, in fact, is due to him. He tried to make an oscillograph with a double strip, the current going up the one and down the other close together, but he was only partially successful, and he therefore temporarily abandoned that form of oscillograph. In a paper on oscillographs which he published last year at the meeting of the French Association for the Advancement of Science he says that since the exhibition of the Physical Society in France in 1897, Mr. Duddell has constructed an oscillograph with a double strip, and has obtained such remarkable results that he (M. Blondel) has been led to take up that method again. But what perhaps is even more complimentary to the authors of this paper is the fact that not only since their work, so far as the paper now before the Institution is concerned, was finished, but since it was in the hands of the secretary of this Institution, M. Blondel has contributed two papers to the French Academy on some of the very points to which the authors have themselves drawn attention in connection with alternate current arcs as studied by means of the oscillograph. One of these papers was presented by M. Blondel in December, 1898, and deals with the effect on an alternate current arc of changing the nature of the carbons as well as of introducing resistance or self-induction into the circuit [1]; while the second, dated March, 1899, goes into the question of an arc between the carbon and metal, and shows the enormous value of the oscillograph for studying such problems. As Dr. Fleming has told you,

[1] This latter subject was also treated by M. Blondel in 1893.

Professor Ayrton.

and as you saw for yourselves last time, the point-by-point method not only fails to give the results so accurately, but fails to give even an idea of them in consequence of the variations that are constantly taking place with an alternate current arc.

I will not now stop to predict what results may be attained from a study of the curves given by the authors. It is, however, obvious that what the authors have aimed at is to change only one of the many variables that it is possible to change at a time. There have been many investigations on the alternate current arc, but as a rule several things have been varied; whereas in the experiments of the authors of this paper one of the conditions only has been changed at a time, such as the length of the arc, everything else being kept as far as possible constant. The value of that I need hardly impress upon you; it is what one endeavours to teach in connection with every scientific investigation—that where it is possible to vary many things, only one should be varied at a time. Results should not be confused by varying things haphazard, and making it impossible to ascertain how much of the change in the result is due to the variation in any of the variables.

There is one investigation to which very little attention has been drawn, viz., that of a Brush arc lamp with a Mordey transformer. The authors say on p. 62 of their paper, "The transformer and the arc lamp are those in general use for the street lighting of an important English town, and our experiments were arranged to produce the ordinary conditions of working." They have modestly omitted to mention that the engineer of that particular English town, being in considerable difficulty about his alternating current arc lamps, and knowing that the authors possessed this particular apparatus, had the whole of the arrangement transmitted to my laboratory for them to carry out an investigation and give him an explanation of the difficulty. This is an actual example of the employment of oscillographs to solve a commercial question and to aid a municipal electrical engineer of one of our towns to get over a difficulty which was troubling him very seriously.

With reference to the suggestion made this evening by Dr. Fleming that the unilateral effect of an arc between carbon and metal is due to the cooling action of the metal, he possibly has not observed that that is the explanation advanced by the authors. On page 80 you will find in italics, "*This suggests that the more rapid cooling of one electrode facilitates the flow of the current from it, and resists the flow towards it.*"

With regard to the very interesting deduction to which Dr. Thompson has drawn attention, I may mention that there is not only the difference, when the current is small with an alternating arc, that the space is found full of vapour, which would not be the case if you were dealing with a direct current arc, but also that the alternate current arc is not what Mrs. Ayrton calls a normal arc—that is to say, the carbons are not shaped for each particular value of the current. Those particular curves which were hung up on the screen on the previous occasion only give you the connection between the P.D. and current for a particular length of arc when the current and the length

Professor Ayrton.

of arc have been kept constant for a sufficient length of time for the normal condition to be arrived at. It is absolutely impossible in the case of an alternating current arc where the current is varying rapidly from 0 to 20, and down to zero again, and then reversing, for the

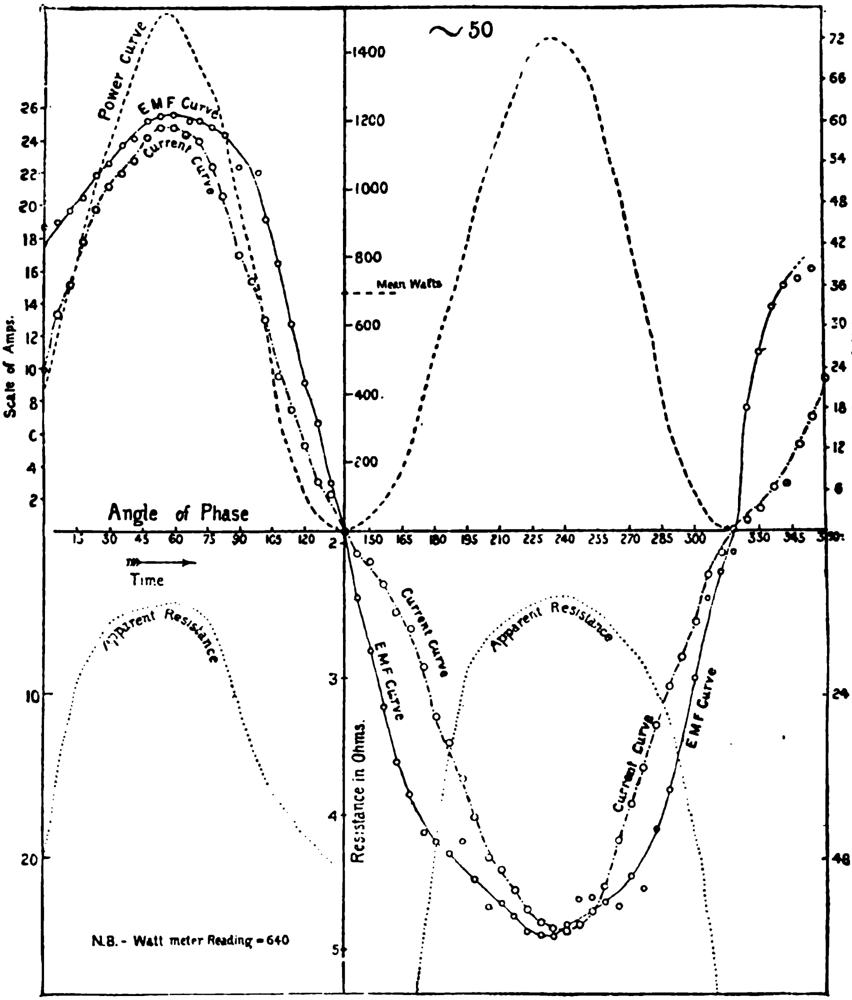

Fig. D.

carbons to be shaped as they would be shaped for each particular value of the current, because in many cases the shape takes a quarter of an hour or half an hour to be attained with the direct current arc. Hence it follows that, while Messrs. Duddell and Marchant's curves for alter-*nating* current arcs can be deduced from those of Mrs. Ayrton's for

direct current arcs when the current is fairly large, this is not the case when the current is small. The other points which Dr. Thompson and others have raised I will leave the authors to reply to. Professor Ayrton.

Mr. A. A. C. SWINTON : I should like to ask the authors what is the limit of periodicity with which this very admirable instrument can be used, because if the limit is sufficiently high it appears to me that it might be applied to the elucidation of the Wehnelt contact-breaker. Mr. Swinton.

Professor AYRTON : They have so applied it and obtained a curve. Professor Ayrton.

Mr. A. A. C. SWINTON : That is what I was going to ask. There are various points with regard to that which are still obscure, and which it appeared to me the instrument might help in elucidating. Mr. Swinton.

Mr. J. E. PETAVEL : Professor Silvanus Thompson has just alluded to the fact that the changes in the resistance of the arc are among the most important factors in determining the shape of the current and electromotive force curves. That is a point which struck me when I was working with Dr. Fleming some four or five years ago. It is evident that as the current decreases the temperature decreases also ; the resistance therefore will rise as the current approaches the zero point. This periodical increase in resistance causes the arc to take a larger proportion of the total electromotive force of the circuit at the instant when the electromotive force is falling to zero. The electromotive force curve will therefore be squarer than the normal curves given by the alternator on a non-inductive resistance ; whereas the current curves, on the contrary, will show a flattening at the zero point. These conclusions have been borne out by the more complete experiments of Messrs. Duddell and Marchant. The dotted curve (Fig. D[1]) is the curve of resistance of the arc as deduced from the instantaneous electromotive force. The original curves of the alternator were very nearly sine curves, and you see the electromotive force curve has become square, whereas the current curve is scooped out at the points where it approaches zero. Although I did not start by thanking the authors for the very ingenious instrument which they have devised, I recognise the usefulness of it in a very high degree, having at different periods spent months in working with the contact-breaker. Mr. Petavel.

Professor AYRTON : I notice there is an extreme difference in positive and negative between the potential difference and current curves ; what circuit was that? Professor Ayrton.

Mr. PETAVEL : It was an ordinary Kapp transformer with a resistance in series. Mr. Petavel.

Mr. L. GASTER : I am also one of those who have had to deal with the alternate current system, and I thoroughly appreciate the very valuable work of Messrs. Duddell and Marchant. One observation that agrees with a result attained by myself is with regard to the conductivity of the arc when potassium salts are used. Soda salts also diminish the potential difference, but the potassium salts are more used for the carbon cores. Then the purity of the carbon has very much to do with the stability and ease in the burning of the lamp. The impregnation of the carbon is a very important point, and one which ought to be further discussed. I think that a paper on the Mr. Gaster.

[1] From the *Philosophical Magazine*, April, 1896, p. 353.

Mr. Gaster. differences in carbon-making and the different results obtained by the various substances used for impregnation would be very useful, and would be very likely to explain some of the things that, taking place in the arc, alter the efficiency of the light. If it should be so desired, I would be willing to give a paper on carbon-making, and try to explain some of the results that take place when different makes of carbon are used. The whole process and the different stages through which a carbon pencil has to pass before it is available for use greatly affect the results. I thank the authors very much for their instrument, which will save much time and trouble.

Mr. Tandy. Mr. F. TANDY: May I ask whether this apparatus has been applied to the study of the telegraphic current. Most of the speakers have dealt with the question of arc lights and alternating currents, but it seems to me that we still have much to learn with regard to telegraphic currents. For instance, we know very little about the shape of the current curve in relation to high-speed telegraphy, and in connection with the recent application of sine currents, in the case of the synchronograph. I should be very glad to know if the periodicity of the instrument is such that it will also record this very high-speed telegraph current with accuracy.

Mr. O'Gorman. Mr. M. O'GORMAN: I should like to point out, in connection with the interesting remark of the last speaker, that there is being manufactured a telephone transmitter (the Randall Telephone), which in a trial on a submarine cable between England and Ireland gave the unexpected result of a rapid movement of the galvanometer spot in response to the word "hallo" shouted at the distant end.

I have been studying this transmitter for Mr. Adams-Randall, and Messrs. Duddell and Marchant's remarkable instrument suggests to me to obtain a photographic record of these extremely rapid impulses through a submarine cable. This might lead us to obtain legible records of a telephonic conversation which would be too much distorted by the capacity of the cable to produce recognisable sounds.

Mr. Ackerman. Mr. E. ACKERMAN: There is one point which I should like to bring out, and that is, that the whole of the apparatus that we saw here last time, with the exception of one piece made by the Cambridge Scientific Instrument Company, was made by Mr. Duddell.

The President. The PRESIDENT [*communicated*]: The question put by Mr. O'Gorman is suggestive of a new kind of phonograph, in which speech can be recorded graphically. The record of this new phonograph would be a kind of writing somewhat resembling that of the siphon recorder, but, like the record of the phonograph, the characters would be phonetic.

It appears to me only necessary to apply the same general principles so exquisitely used in the instrument of Messrs. Duddell and Marchant, with the difference in detail that the luminous pencil would be actuated by the vibrations of the diaphragm of the telephone instead of by the vibrating wires of the galvanometer, and that the record would be imprinted on a travelling strip of photographic paper instead of being produced on the screen as we have seen it to-night.

Mrs. Ayrton. Mrs. AYRTON [*communicated*]: It is difficult to over-estimate the importance of the instrument so ingeniously devised and so perfectly

made by Messrs. Duddell and Marchant. Such an instrument can hardly fail, in time, to lay bare all the most intimate secrets of the arc —to tell us what is its true resistance, what is the real value of its back E.M.F., if it exists at all, and to settle, once for all, all those burning questions that have ravaged the breasts of succeeding generations of investigators for the greater part of a century. To see the curves actually forming themselves under your eyes, to be able to photograph them under every imaginable set of conditions—that must give an accuracy to the study of the arc never before obtainable. Mrs. Ayrton.

The curves that interest me most are those for long metal-carbon arcs (pp. 76–79), which lead to some important conclusions. For instance, it may be deduced from these curves that with such arcs the central vaporous portion must be entirely metallic. For these curves indicate that no appreciable current flows while the positive electrode is of carbon, consequently there is no arc during that period. Now, I think no one disputes that, in the arc, volatilisation takes place at the *positive* electrode *only*: therefore, since in long metal-carbon arcs the arc exists only while the *metal* is positive, only *metal* can become volatilised, and hence the vapour of the arc must be entirely metallic.

Now, since metal volatilises at a much lower temperature than carbon, metal vapour, under atmospheric conditions, must be much cooler than carbon vapour. Hence, while, when the metal electrode begins to be positive, it is in contact with its own vapour, and is therefore very near its own temperature of volatilisation; when the *carbon* rod, on the other hand, begins to be positive, it is in contact with a vapour very much cooler than its own, and is therefore at a temperature much lower than its own temperature of volatilisation. Hence it requires more heat—that is, more electric energy, to volatilise it than either the metal rod in contact with metal vapour, or than a carbon rod would, in contact with carbon vapour.

This is only one of many conclusions of great significance that may be deduced from Messrs. Duddell and Marchant's curves, but it is sufficient to show how great are the results that we may expect from the application of their beautiful instrument to the unravelling of the mysteries of the arc.

Professor E. WILSON [*communicated*]: In 1892 the late Dr. John Hopkinson and I commenced a research in connection with alternate current arcs, one object being to confirm or not a theory set forth by Dr. Hopkinson [1] based upon conclusions drawn by Joubert. A short notice in the *Electrical Review* [2] called our attention to the work of M. Blondel, and on examination of his published results,[3] which dealt so fully with the matter, we decided to postpone the research, as we had other important work in hand. I however published an account of some of the experiments,[4] and pointed out the violet colour which accompanies long arcs. The frequencies tried were 72 and 8 periods per second. The curves of current and potential difference between Professor Wilson.

[1] See *Journal of Inst. Elec. Eng.*, November, 1884.
[2] See *Electrical Review*, December 16, 1892.
[3] See *Électricité*, November 17, 1892.
[4] See *Electrical Review*, December 30, 1892.

Professor Wilson.

the carbons, in the case of arcs $\frac{1}{4}$-inch and $\frac{1}{16}$-inch long, cross the axis of time at the same moment, so that the power factor depends upon the wave-form. The alternate current machine used had no iron in its armature, and the extra resistance in series with the lamp was non-inductive.

Since the date of the above experiments I have made occasional experiments with alternate current arcs and recently I have worked with the Inclosed type. My experiments with a Davy Inclosed Lamp, when working with a choking coil, confirm the results obtained by Messrs. Duddell and Marchant. It is not necessary for me to give any curves. The lamp appears to burn best with a higher potential difference between its terminals in the case of direct currents than with alternate currents. For instance, with 8·5 amperes it requires a little over 80 volts with direct currents, and 70 volts $\sqrt{\text{mean}^2}$ with 8·4 amperes $\sqrt{\text{mean}^2}$ alternate current at 83 periods per second. The arc had a length of about $\frac{1}{4}$ inch in each case, but was not measured very accurately. It appears to be best to have both carbons solid with direct currents and both cored with alternate currents; 13 m/m carbons were used in the experiment.

I wish to congratulate the authors on having perfected a beautiful instrument, and on having presented such a valuable paper. Those who have worked with an ordinary contact maker on this subject, presenting as it does so many variables, know full well how disheartening it is, since the time taken is so great and conditions change.

Messrs. Duddell and Marchant.

Mr. W. DUDDELL and Mr. E. W. MARCHANT in reply [*communicated*]: We are very interested in the curves for two alternators in parallel which Mr. Andrews has obtained by the telephone method; and we have no doubt that the two *current* curves are fairly accurate, for, presumably, the two telephones and their circuits, which were used to record the curves, were exactly alike; in which case, the *relative* phase-differences and amplitudes of the two approximately sinusoïdal currents would be accurately given by the curves. The E.M.F. curve C will not, however, necessarily occupy the correct position with respect to the current curves A and B unless specially adjusted to do so.

It is with a certain satisfaction that we hear from Prof. Fleming that, after working out a satisfactory point by point method, he was obliged to give it up when working with the arc wave-forms; as it was the difficulty of applying the point by point method to these wave-forms that led us to develop the oscillograph, and not the oscillograph which led to the investigation of the arc.

We were rather shy of publishing the wave-forms of the metal-carbon arcs when we first obtained them, just two years ago, as they seemed then so very extraordinary, and we are glad to see that they are confirmed by the results lately published by M. Blondel.[1] The exact cause of the uni-lateral nature of this arc is, as yet, obscure; but some further experiments made since writing our paper may help to elucidate it. When the metal-carbon arc is burning with the current

[1] *Comptes Rendus*, 1899, vol. cxxviii., p. 727.

in one direction only, as in our type 2 wave-form, it is silent; if, however, the current flows in both directions, types 1 and 3, then the arc generally hisses. The image of the arc in the two cases is also quite different; the vapour column is steady, and touches only a small point on the molten end of the metal electrode when the arc is uni-lateral; but when the current flows in both directions, the vapour column is in rapid motion and is surrounded by flames and fumes. We have attempted to force the current to flow in both directions through the steady arc by increasing the self-induction in series with the arc and the E.M.F. of the alternator until the instantaneous value of the P.D., tending to force the current from carbon to metal, has risen to between five and six hundred volts; but without success. Messrs. Duddell and Marchant.

We have not been able to destroy the uni-lateral nature of the metal-carbon arcs, either by heating the metal, as suggested by Prof. Fleming, or by burning the arc between a carbon and a bath of molten metal kept hot by means of an oxy-hydrogen flame. In both cases when the arc is silent the current only flows from the metal to the carbon, but the stability of the arc is greatly improved by the heating. In order further to test the effect of change of temperature of one of the electrodes, we tried an arc between a hollow carbon, as the top, and a solid carbon as the bottom, electrode (each 13 mm. diameter), the hole (6 mm. diameter) in the top carbon extending to within a few millimetres of the crater surface. In this hole was a small (3 mm.) copper tube, which nearly reached the bottom, and down which we could force a stream of water or liquid CO_2, and thus produce considerable cooling at a small distance from the crater, which distance gradually decreased as the carbon burnt away, until the thickness of carbon between the cooling liquid and the crater became so thin (about $\frac{1}{4}$ mm.) that the liquid came through into the arc and put it out. The following observations were made just before the first trace of the cooling liquid got through the carbon into the arc. The cooled carbon was only red-hot for a distance of about 3 mm. from its end, as against 15 mm. for the uncooled carbon; in spite of this the brilliancy of the crater on the cooled carbon did not appear less than that on the solid electrode; but the wasting away of the sides of the carbon was so reduced by the cooling that its end became almost flat. The cross section of the vapour column was reduced by the cooling, and the arc was very unsteady and inclined to hiss. The effect on the wave-forms was greatly to increase the front peak on the P.D. arc wave and to increase the fraction of the period during which the current remained small. These changes in the wave-forms are what we should expect from an increase of resistance of the vapour column. There was another effect that occurred when the arc was not hissing, which was that the maximum instantaneous value of the current was slightly *larger when it flowed from the solid carbon to the cooled carbon than when it flowed in the opposite direction*. This last result is opposed to the theory that cooling by conduction of the crater on the metal is the cause of the uni-lateral nature of the metal-carbon arcs; but it is to be remembered that neither in the experiment of heating the copper electrode, nor in that of cooling the carbon electrode, have we any evidence that we have

Messrs. Duddell and Marchant.

succeeded in altering the temperatures of the craters themselves; in fact the evidence is against our having done so. Thus the uni-lateral nature of the metal-carbon arc may still be due to the difference of temperature between the carbon and the metal craters. This difference of temperature between the craters necessitates a temperature gradient which may either be distributed over the whole arc, or concentrated at one or both of the crater surfaces; and this latter seems to us the more probable. In which case, a possible explanation of the uni-lateral nature of the metal-carbon arc may be that the current can more easily flow up than down a steep temperature gradient at the crater surfaces.

The uni-lateral behaviour does not seem to be in any way limited to the monatomic metals; in fact, up to the time of writing our paper, aluminium was the only metal with which we had not observed this effect. In this case we think our arc was not burning between carbon and *metallic* aluminium, but between carbon and an *oxide* of aluminium. The presence of a considerable skin of oxide was evident from the fact that it formed a bag on the end of the electrode about 2 c.m. long, which was full of molten metal while the arc was burning. Since our paper we have obtained a uni-lateral aluminium-carbon arc by cleaning the metal just before use, and uni-lateral wave-forms for this arc have been published by M. Blondel.[1]

In order to prove Mrs. Ayrton's deduction that, in the uni-lateral metal-carbon arc, the vapour column is entirely metallic we have examined its spectrum, but we find that besides the spectral lines due to the metal, the carbon lines are always present. We have also examined the spectrum of the vapour column of the direct current metal-carbon arc, the metal being positive, and we find that carbon lines are also present in this case. These tests seem to indicate that carbon vapour is present in the vapour column of the arc possibly produced by the volatilisation of the carbon which forms the negative crater. In view of the sensibility of spectroscopic test, we cannot decide how the carbon vapour gets into the arc until further experiments have been made.

A very pretty way of showing the lag of the luminous intensity behind the current flowing through the arc is the experiment, first proposed by M. Blondel, in which the arc under investigation is used as the source of light for the oscillograph, so that the brightness of each part of the wave-forms themselves is a rough measure of the light emitted by the arc at each part of the period.

In reply to Prof. Thompson's remarks on our first conclusion, we would point out that though this conclusion is not unexpected by those who have worked at the subject of the alternate current arc, it is not so well recognised as can be wished; and as it states the fundamental changes that we were investigating, it was necessary to give it.

We do not quite follow Prof. Thompson in his explanation, based on the variable resistance of the arc, of the fact that, when in a circuit we have a fixed impressed E.M.F. wave, and we substitute an arc for a *constant*

[1] *Comptes Rendus*, 1899, vol. cxxviii., p. 728.

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resistance, the root-mean-square current remaining the same, then the root-mean-square value of the P.D. between the terminals of the arc is *larger* than it was between the terminals of the constant resistance. For, if we understand his remarks correctly, his explanation leads him to an erroneous conclusion. He says: "If you could produce a case where the resistance was large when the current was large, and small when the current was small, then, I suppose, the converse ought to hold good;" which we take to mean that the root-mean-square of the P.D. value between the terminals of a resistance varying in this way would be *smaller* than it was between the terminals of the constant resistance for which it is substituted. We think Prof. Thompson will find that under the circuit conditions given above, the root-mean-square value of the P.D. between the terminals of the variable resistance is always *larger* than it was between the terminals of the *constant* resistance, independent of the law connecting the value of the variable resistance with the current.

We regret that we have not made it quite clear which are the dependent and independent variables in our experiments. The shape of the wave-form was in all cases a dependent variable, and no attempt was made to keep the shape constant, and the independent variable is that specified in the heading of each section, diagram or table. The confusion appears to arise from the fact that we have not only to change our independent variable from one experiment to the next, but also that during *each* experiment it is necessary to regulate the arc length to allow for the consumption of the carbons; and it was this latter regulation only that was done by means of the root-mean-square potential difference between the arc terminals, as we found (*see* p. 23) "as long as the conditions of the dynamo and circuit remained the same, that regulating the length of the arc so as to keep the P.D. between its terminals constant, maintained the arc length, the current and the power supplied to the arc constant." We may here mention that M. Blondel regulated by keeping the root-mean-square current constant.

We have tried plotting by the aid of Mrs. Ayrton's curves for the direct current arc, the P.D. wave-form derived from the observed current wave-form, and we have come to the same conclusions as Prof. Thompson, namely: that to a certain extent one wave-form can be obtained from the other. But we have not yet been able to predict by means of Mrs. Ayrton's curves both wave-forms from the known conditions of the circuit. The reasons why it is impossible accurately to predict one wave-form from the other has been made clear by Prof. Ayrton in his remarks. There is also another reason which tells against comparative results, which does not seem to be very generally known, viz., that two samples of ordinary carbons, nominally the same, do not necessarily give identical results. In fact, we were obliged to repeat a large number of our experiments through running short of a certain kind of carbon, as we found that, under the same conditions, the new stock burnt with 2 per cent. less P.D.

We have plotted a good many curves of the apparent resistance of the arc, taking as our co-ordinates both resistance and time as Mr.

Messrs. Duddell and Marchant.

succeeded in altering the temperatures of the craters themselves; in fact the evidence is against our having done so. Thus the uni-lateral nature of the metal-carbon arc may still be due to the difference of temperature between the carbon and the metal craters. This difference of temperature between the craters necessitates a temperature gradient which may either be distributed over the whole arc, or concentrated at one or both of the crater surfaces; and this latter seems to us the more probable. In which case, a possible explanation of the uni-lateral nature of the metal-carbon arc may be that the current can more easily flow up than down a steep temperature gradient at the crater surfaces.

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In order to prove Mrs. Ayrton's deduction that, in the uni-lateral metal-carbon arc, the vapour column is entirely metallic we have examined its spectrum, but we find that besides the spectral lines due to the metal, the carbon lines are always present. We have also examined the spectrum of the vapour column of the direct current metal-carbon arc, the metal being positive, and we find that carbon lines are also present in this case. These tests seem to indicate that carbon vapour is present in the vapour column of the arc possibly produced by the volatilisation of the carbon which forms the negative crater. In view of the sensibility of spectroscopic test, we cannot decide how the carbon vapour gets into the arc until further experiments have been made.

A very pretty way of showing the lag of the luminous intensity behind the current flowing through the arc is the experiment, first proposed by M. Blondel, in which the arc under investigation is used as the source of light for the oscillograph, so that the brightness of each part of the wave-forms themselves is a rough measure of the light emitted by the arc at each part of the period.

In reply to Prof. Thompson's remarks on our first conclusion, we would point out that though this conclusion is not unexpected by those who have worked at the subject of the alternate current arc, it is not so well recognised as can be wished; and as it states the fundamental changes that we were investigating, it was necessary to give it.

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[1] *Comptes Rendus*, 1899, vol. cxxviii., p. 728.

resistance, the root-mean-square current remaining the same, then the root-mean-square value of the P.D. between the terminals of the arc is *larger* than it was between the terminals of the constant resistance. For, if we understand his remarks correctly, his explanation leads him to an erroneous conclusion. He says: "If you could produce a case where the resistance was large when the current was large, and small when the current was small, then, I suppose, the converse ought to hold good;" which we take to mean that the root-mean-square of the P.D. value between the terminals of a resistance varying in this way would be *smaller* than it was between the terminals of the constant resistance for which it is substituted. We think Prof. Thompson will find that under the circuit conditions given above, the root-mean-square value of the P.D. between the terminals of the variable resistance is always *larger* than it was between the terminals of the *constant* resistance, independent of the law connecting the value of the variable resistance with the current. Messrs. Duddell and Marchant.

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4. Leakage error, which on long submarine cables is often large, and affects measurements more than any other cause.

With respect to definition of electrostatic capacity, two remarkable results of mathematical theory may be quoted from a paper by Dr. Hopkinson and Professor E. Wilson (*Phil. Trans.* vol. 189, pp. 109–136), though probably they are also to be found elsewhere. In this they demonstrate that, calling Y the *total* quantity of electricity coming out of a condenser which has been charged for time T to potential X, and writing ω for the symbol of variable time,

$$Y = X \int_0^T \psi(\omega)\, d\omega,$$

or the capacity is a function of the time of charge, increasing as the time increases. The other result is that the time-integral of current, or the electric displacement, through a condenser can be expressed in the form—

$$y_t = Kx_t + \int_0^\infty x_{t-\omega} \left\{ \psi(\omega) + \beta \right\} d\omega,$$

where the first term represents "instantaneous" capacity, the second residual charge or absorption, and the third conductivity of dielectric, separated for convenience but really all parts of a continuous magnitude.

What the latter formula indicates, with this addition that in a cable we have retardation of charge by conductor resistance, may be exhibited diagrammatically, I believe, as in Fig. 1, which illustrates what happens when a long cable, with its distant end insulated, is charged continuously. The horizontal shading shows constant leakage current to earth; the vertical shading, P Q R S, represents a portion of the charge that has entered the cable during a period of battery contact of 10 seconds, *p* P S *s* the quantity added to it after 15 seconds have elapsed; whilst after 30, 45 and 60 seconds respectively, small additional increments *p s s p*, &c., have flowed into the cable.

This takes place by virtue of the well-known phenomenon of absorption, or residual charge, which commences *at the same instant* as the true or surface charge (as it may be termed), and continues long after the latter has ceased, disappearing logarithmically with the prolongation of battery contact, and leaving the true leakage current O R on which it was superimposed.

Thus the surface charge of the conductor and the residual charge due to dielectric absorption form parts of

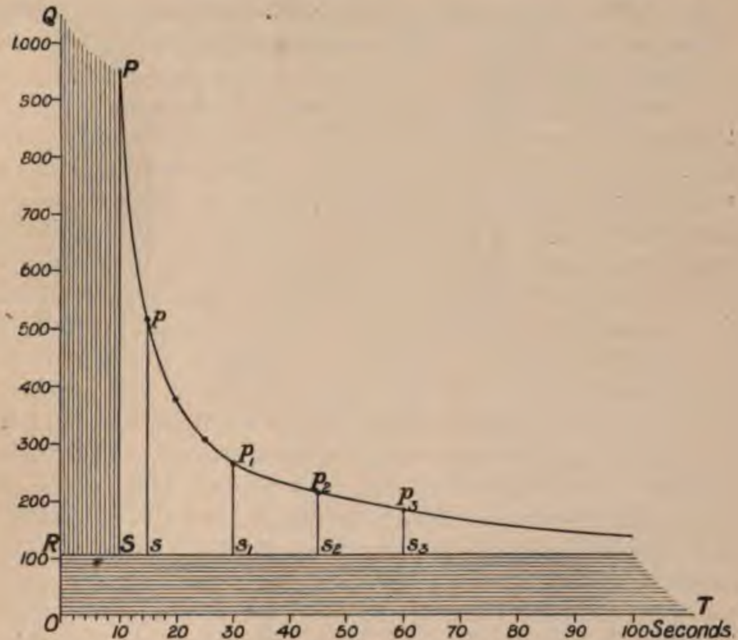


FIG. 1.

one continuous magnitude, varying with time, which must be regarded as the capacity of the cable, though theory divides the two. The same is true of any condenser except one having a dielectric of air, the absorption of which is negligible. If a paraffin-mica condenser be measured by an air standard its value may be raised some 5 per cent. by prolonging the time of charge.

Now in balancing a cable against a paraffin condenser, if they both have the same rate of absorption there will be

no apparent variation of results with the time of charging. This, however, is not the case in general, for not only do the specific absorptive capacities of paraffin and gutta-percha differ, but the conductor resistance of a long cable greatly modifies its rate of absorption, at all events near the beginning of charge or discharge. Thus there will be a divergence of results with different times of charge—quite independently of that due to true leakage effects. (I am assuming that comparisons are made by the usual "Thomson" or "Gott" methods.) This is sufficiently illustrated by the appended tests of two long ocean cables, where due correction was made for the leakage in the manner described further on. *See Tables (appended).*

Besides this effect of time, there is, moreover, one of temperature to be noticed. The absorptive capacity of both gutta-percha and paraffin is augmented by fall of temperature, thus causing an increase in their apparent capacities. This variation is of a smaller order of magnitude than the former, being in fact the increment of an increment. Thus in the case of paraffin the temperature effect is to increase the capacity by only 0·025 per cent. per fall of 1 degree Centigrade. The rate at which it affects that of gutta-percha, whilst in the same direction, is still smaller, and has eluded determination altogether hitherto. Still it cannot be regarded as negligible when we are seeking the maximum accuracy attainable, and dealing with cables of many hundreds of microfarads submerged at depths such that their temperature falls by some 20 degrees Centigrade below the standard 24° C. of the factories. In the case of ordinary paraffin condensers when employed for accurate measurement it is a factor requiring careful allowance to be made for it; though in the compensated standards constructed by Dr. Muirhead, partly of paraffin and partly of shellac, the opposite behaviour of the shellac neutralises their temperature variation.

There appears to be no *secular* change in the capacity of paraffin condensers when moisture and air have been expelled by heat during manufacture. An apparent secular change has, however, been encountered in the capacity of certain G.P. core, the origin of which deserves to be known amongst telegraph engineers, as it may occasionally explain otherwise anomalous measurements. This was traced to

the absorption of water by the G.P. after it had been rendered porous by alternations of wetness and dryness. Water has the highest specific I.C. known of any substance (I believe), and samples of G.P. containing low and high percentages of water have been found to show marked differences of inductive capacity, those with the higher percentage having the larger I.C.[1] This circumstance somewhat further obscures the search for a coefficient of temperature variation.

Leaving aside the subject of temperature corrections, which are to be made by means of proper coefficients, I pass on to the methods proposed for the elimination of the discrepancies and indefiniteness introduced into comparisons of long cables with condensers by reason of their difference of

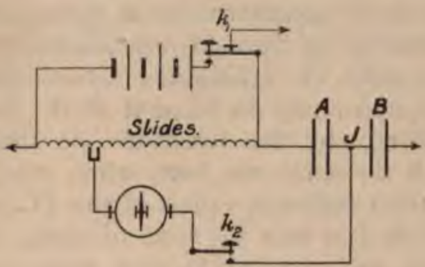


FIG. 2.

rates of absorption. For this purpose Dr. Muirhead has given us his correction of Gott's test, applicable perfectly to comparisons of condensers with one another or with short sections of cable, but of little or no use on very long ones, presumably on account of their C.R. retardation. This has been often described, and consists essentially in (1) raising the potential of the junction, J (Fig. 2), of the two condensers from zero to a value, v, by application of the charge "in cascade" (by Gott's connections), and then permitting absorption to proceed whilst balance is being found on the slides, which changes v to v_1 by the time balance has been arrived at; (2) lowering the potential of J, by earthing k_1, from v_1 to (v_1-v); (3) observing the amount (v_1-v) of this residual potential, and evaluating its effect on the slide reading.

[1] Dr. Obach, *Cantor Lectures on Gutta-percha*, p. 64.

By means of such a correction, if A (Fig. 2) be an air standard, and B a condenser of paraffin or other dielectric having absorption, whilst A has *none*, we can arrive at a value of B which is independent of the time of charging, and may be regarded as its constant capacity, eliminating a certain variable part due to absorption.

Now if B, substituted for A, be balanced against a naut of core C, substituted for B, and the same method of correction applied, we similarly get a value for C, in terms of the above constant value of B, practically independent of time of charges which may be taken as the constant capacity of C. So that for a laboratory or factory determination of the I.C. of a condenser or a cable core this plan ought to, and does in short, leave nothing to be desired for the elimination of difference of rates of absorption and the establishment of the constant value of the capacity.

When, however, we come to measurements of 1,000 or 2,000 nauts of cable, Dr. Muirhead's method ceases to yield correct results, apparently on account of the extra retardation of discharge by the resistance of the conductor. Abandoning it therefore, we have, when using either the Gott or Thomson balances, values of the I.C. of the cable varying more or less with the time of charge (apart from leakage effect), and are led to seek some other means of getting at our constant or *quasi*-constant capacity. Dr. Muirhead has suggested for consideration that we should take the *varying values of the condenser*, determined by reference to his air standard when absorption is *not* eliminated, and use these for the different times of charging employed on different cables. This would give us the total increment due to the absorption of the cable, in place of the difference of the cable and condenser increments, and thus cause the results of tests by the usual Gott or Thomson methods to rise with time more rapidly than they otherwise do, though more consistently.

A more satisfactory solution of the difficulty has been arrived at, which I will describe presently. Before doing so, however, it is necessary to deal with the important factor of leakage.

Referring to Fig. 1, we see that, as distinction has been made between the surface or "instantaneous," and the absorbed or residual charges, so the ultimate conductivity

of the dielectric (shown in the diagram by the horizontally shaded area) may be treated as a separate factor in the problem under consideration.

Without entering into a description of the three well-known methods of I.C. measurement applicable to long submarine cables, viz., the Siemens fall of charge, the Thomson parallel charge, and the Gott "cascade" charge, let us examine the effect, upon the capacities under comparison, of their respective insulation resistances in the two latter methods. In Siemens's there is, of course, no leakage error, because the D.R. enters into the formula, an advantage which would place this test above the others beyond dispute,

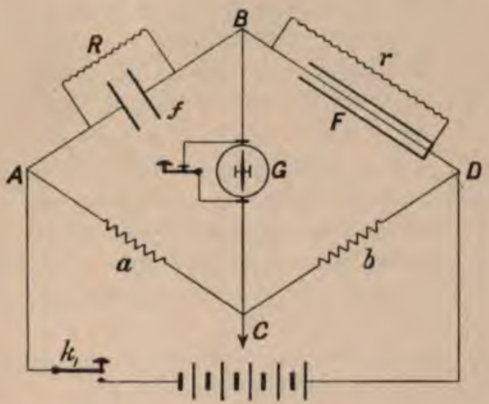


FIG. 3.

were it not that it is incorrect for other reasons. Leakage on the *battery*, a matter of vital importance in the Thomson method—though not so in Gott's—we may assume to be absent, but it must on no account be forgotten when joining up for the former test. Its vitiating influence on results has indeed been one of the chief reasons for preferring Gott's method.

Both the Thomson and Gott are null methods, or balances, differing essentially in the circumstance that in the former the two capacities are charged in parallel, and in the latter in series or "cascade." For the purpose of investigating the leakage error the diagrams of connections may be conveniently drawn as a bridge system. See Figs.

3 and 4, which represent the conditions during charging, f being the condenser capacity, F that of the cable, R and r their respective insulation resistances.

In Fig. 4 balance is observed by closing k_2 without discharging. In Fig. 3 it is observed after mixing the charges, by an arrangement of keys not shown; but it will be found that for the determination of the leakage error it is practically sufficient to examine the conditions during charging only.

In Fig. 3, then, the influence of the leakage during the period of charging is approximately proportional to the insulation resistances R, r, which alter the potentials of the

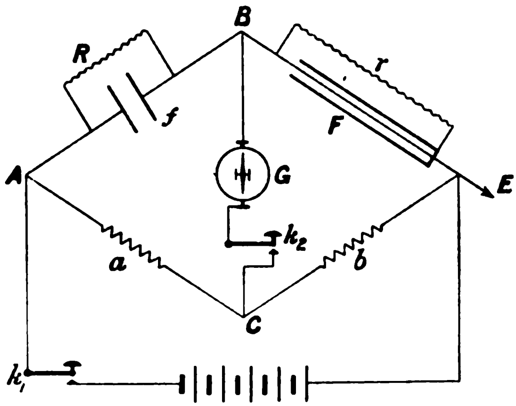


FIG. 4.

battery poles (A, D) by amounts dependent on the parallel resistances $\frac{a\,R}{a + R}$ and $\frac{b\,r}{b + r}$. This effect is independent of the time of charge since, by hypothesis, R and r stand for the true leakage resistances, and the apparent change of the D.R.'s of cable and condensers with time is treated as absorption. The potential of the junction (B) between the cable and condensers does not change, this point being connected to earth. The ratio of the capacities thus becomes

$$\frac{a\,R}{a + R} : \frac{b\,r}{b + r}$$

instead of $a : b$. This is approximate, because, on a long

cable, the distribution of the leakage along the line somewhat modifies the problem.[1]

Another point to be noticed here is that the slide resistances a, b must themselves be well insulated, since they commonly consist of 100,000$^\omega$, so that a leak of 10 megohms on the higher branch would mean, were the ratio of the arms, say, 4 : 1,

$$\frac{80000}{20000} : \frac{79365}{20000} : : 4 : 3{\cdot}9682,$$

nearly 1 per cent. error.

In Fig. 4 B is insulated during charging. Its potential therefore alters with time under the operation of leakage, producing a positive variation through R and a negative one through r; thus we have an error proportional to the time of charge, unless the leakages be balanced. Corrections of Gott's test for this error have been tried in the following ways :—

1. By Dr. Muirhead's method, already referred to, of earthing the condenser (f) immediately after balance, and then measuring the difference from zero of the potential at B. This, although correct under certain conditions, has proved quite unreliable on a very long cable of unknown I.C.

2. By compensating the cable leakage exactly by adjusting the insulation resistance of the condensers so that $f\,\mathrm{R} = \mathrm{F}\,r$, R and r having been previously measured.[2] This is a very satisfactory plan when capable of employment, either f or R being varied as most convenient.

[1] Taking this into consideration, Schwendler gives the following formula :—Multiply the apparent I.C. arrived at by the ratio $\frac{l}{\lambda}$, where l = length of line in miles,

$$\lambda = \frac{1 - \frac{1}{\mathrm{K}},}{m}$$

$$\log \mathrm{K} = m\,l \times 0{\cdot}4343,$$

and $$m = \sqrt{\frac{r}{i}},$$

r and i being the conduction and insulation per mile, both in ohms.

[2] See *The Electrician*, February 5th, 1892.

3. By calculation of the loss of charge during the interval between battery contact and balance. An analysis of the problem by Mr. E. C. Rimington was published in 1887,[1] but the formula arrived at does not appear to be what is wanted. A formula for the purpose was published by Mr. H. W. Ansell and myself in 1890,[2] taking into account leakage on the cable side only. Although it was evident that the error on the condenser side could be eliminated by a similar calculation so long as it acted alone, yet when both branches leaked simultaneously the problem did not appear to admit of a simple suggestion. Recently, however, Mr. Walter J. Murphy suggested to me taking the difference of the two errors, computed separately, which on trial I find yields practically exact results, at least in dealing with large capacities, such as those of long cables. The formula is then as follows :—

Let R, r be the *true* insulation resistances in megohms of the capacities f, F microfarads, and let t be the time of charge in seconds. Let

$$\log k = \frac{\cdot 4343 t}{f \mathrm{R}}, \text{ and } \log \mathrm{K} = \frac{\cdot 4343 t}{\mathrm{F} r}.$$

Then

$$\text{True I.C.} = \text{Apparent I.C.} \times \frac{k}{\mathrm{K}}.$$

This is the system of leakage correction applied to the results in Table I. Appendix. Experiments with shunted capacities of only 10 microfarads or less showed a slight departure from the rule.

Having indicated the *calculated* allowances for dielectric leakage to be made in the Thomson and Gott balances, I now come to what seems to be a more satisfactory mode of exact I.C. measurement than either of them, as regards correction not only for leakage but also for difference of rates of absorption. Being merely a modification of Lord Kelvin's original method, it has been termed the "modified Thomson" test. It appears to have occurred to more than one investigator, but to my friend Mr. W. J. Murphy belongs the credit of having proved its superiority as the most practical

[1] *Philosophical Magazine*, September, 1887.
[2] *The Electrician*, December 12th, 1890.

means of arriving at uniform results on long ocean cables, without the necessity for calculations; and my own experience has lately confirmed this conclusion.

In this method both leakages and absorptions are balanced *by observation and adjustment* during charging, absorption in *this* case being treated as apparent improvement of D.R. That is to say, the condensers are shunted by a resistance which is adjusted till it approximately balances the *apparent* D.R. of the cable at any time of charge. The connections for charging are those of Gott's method (Fig. 4), with this important difference, that the key k_2 is closed during the charge. The effect of this is to keep the potential of the point B constant, as in the Thomson balance, while at the same time the deflection on the galvanometer shows whether or not there is a balance of (apparent) insulation resistances in the bridge system. Since this balance is required to be such that $\frac{R}{r} = \frac{F}{f}$, it must be observed when $\frac{a}{b} = \frac{F}{f}$ nearly; that is to say, we must commence with an approximately correct position of the slider C. Then R, the leakage shunt across the condensers, is adjusted till G shows a resistance balance, as nearly as possible, at the end of any desired time of charging. For this purpose a set of plumbago resistances is useful, or it can be done roughly by the application of a pocket-handkerchief across the terminals of the condenser more or less closely as required; but it is preferable to estimate approximately the proper resistance and to arrange it beforehand.

Having thus charged, and balanced the apparent D.R.'s by observation, the test proceeds in the same way as the Thomson, viz., by short-circuiting and mixing the charges, and observing if any outstanding quantity remains on earthing the junction of the two capacities through the galvanometer. An additional advantage over the original Thomson balance is that there is no serious vitiation of results by battery leakage. Moreover, it admits of allowance being made for "cable current" by false zero observation, a most important point on long submarine lines. It should be remarked that in the modified, as in the original, method the *mixing* period must be sufficiently prolonged to discharge the cable, or variable results will ensue. On a section of 1850 nauts I have found 15 seconds' mixing suffi-

ciently long, within the limits of appreciation, even after a full minute's charging—*i.e.*, there was no difference between a 15 and a 25 seconds' mixing interval.

By means of a measurement thus carried out, it is possible to arrive at the same result for all times of charge, and any values of the dielectric resistance, and to dispense with all calculated allowances for leakage. Thus the desired uniformity of results is ensured, taking the constant value of the balancing condensers, determined by Dr. Muirhead's method, as a basis. With this value the latter should be marked, as well as with the temperature at which it was measured. Care must then be taken to correct the condensers for their temperature changes, by mean of the coefficients furnished by Dr. Muirhead. It only remains to determine the very small coefficient of temperature variation applicable to the specific I.C. of gutta-percha. I have attempted to deduce this from the series of careful tests of two long cables appended to this paper, and arrived at a coefficient closely corresponding to that for paraffin; but Dr. Muirhead thinks it must be smaller than that. The search for such a minute residual phenomenon, where so many sources of error exist, must necessarily be elusive, in common with determinations of residual quantities in other departments of exact measurement.

In conclusion, since it may be asked of what practical utility are these researches after the exact I.C. of long submarine cables, their possible importance in connection with repairing operations may be alluded to. It must be remembered that on ocean cables a microfarad means some 3 nautical miles; so that a mistake in measurement of 1 per cent. in the distance to a sealed break, on a section of, say, 700 microfarads—if for any reason localisation from the other end were impracticable, as sometimes happens—would amount to an error of 20 nauts, and would not satisfy the standard of modern practice.

TABLE I.

GOTT METHOD, CORRECTED FOR LEAKAGE BY CALCULATION.

Mean Temperature of Cables in every case, 5° Cent.

Condensers, 147·2ϕ. D.R., about 20Ω. Temp., 29·4° C.

CABLE A. I.C. by Factory data at 24° C. 596·5ϕ					CABLE B. I.C. by Factory data at 24° C. 724·8ϕ				
Time of Charge.	Appt. D.R.	Appt. I.C.	$\log\frac{k}{K}$	Corr. I.C.	Time of Charge.	Appt. D.R.	Appt. I.C.	$\log\frac{k}{K}$	Corrected I.C.
	Ω	ϕ		ϕ		Ω	ϕ		ϕ
5″	...	583·4	—·00193	580·8	5″	...	721·3	+·00014	721·55
10	...	600·5	—·00387	595·2	10	...	725·4	+·00029	725·9
15	0·5	606·2	—·00580	598·2	15	1·0	729·05	+·00043	729·8
30	0·98	618·0	—·01161	601·7	30	2·0	733·3	+·00087	734·8
45	1·10	631·4	—·01741	606·6	45	2·36	736·5	+·00130	738·7
60	1·20	638·0	—·02322	604·8	60	2·7	741·2*	+·00174	744·2*
300″	1·40 = value taken for corrections				300″	5·0 = value taken from corrections			

* Approximate.

TABLE II.

THOMSON METHOD, CORRECTED FOR LEAKAGE BY CALCULATION OR BALANCE.

Condensers, 146·3ϕ. D.R., about 20Ω. Temp., about 25° C.

CABLE A. I.C. by Factory data at 24° C. 596·5ϕ					CABLE B. I.C. by Factory data at 24° C. 724·8ϕ				
Time of Charge.	Time of Mixing.	True D.R.	Appt. I.C.	Corr. I.C.	Time of Charge.	Time of Mixing.	True D.R.	Appt. I.C.	Corected I.C.
		Ω	ϕ	ϕ			Ω		
30″	15″	1·0	577·8	589·6	30″	15″	20	727	No correction required, as leakages balanced (but not absorptions)
45	15	...	586·0	598	40	20	...	731	
60	15 & 25	...	594·6	604·1	60	20	...	735·2	
120	20	...	about same		180	30	...	742·8	
180″	30″	...	about same		300″	30″	...	745·9	

TABLE III.

MODIFIED THOMSON METHOD. ABSORPTIONS AND LEAKAGES BALANCED BY OBSERVATION, APPROXIMATELY.

Condensers, 119·25ϕ. D.R., 140Ω. Temp., 24·4° C.

CABLE A. I.C. from Factory data at 24° C. 597·5ϕ					CABLE B. I.C. from Factory data at 24° C. 724·8ϕ				
Time of Charge.	Time of Mixing	Appt. D.R. of Cable.	Leak on Condensers.	Constant I.C.	Time of Charge.	Time of Mixing.	Appt. D.R. of Cable.	Leak on Condensers.	Constant I.C.
		Ω	Ω	ϕ		Ω	Ω	Ω	ϕ
15″	10″	0·3	2·3	599·1	15″	10″	0·92	6	741·8
30	15	0·60	3·3	599·1	15″	10	0·92	12	too large
45	20	0·62	3·3	601·3	30″	15	1·7	12	741·8
60″	25″	0·63	3·3	601·3	60″	20″	2·5	51	about same

APPENDIX.

ON A STANDARD TIME OF CHARGE FOR ELECTRICAL MEASUREMENT OF LONG SUBMARINE CABLES.

To ensure the greatest possible accuracy and uniformity of measurement, it is desirable to fix some definite time of charge in observing the C.R. and the I.C. of a long cable, as well as for the D.R. test. In the last of the three measurements, 1, 2 or more minutes have been adopted according to requirements, but for the other two no such standard is generally recognised. It has been proposed (*vide* Darby and Fisher's "Submarine Cable Testing") to fix a period of $\frac{KR}{10^6}$ seconds, where K is the I.C. in microfarads, R the C.R. in ohms of the cable under test. This, however, is too short a charge; for, referring to Fig. 1, it is evident that the steady state is not sufficiently approached. The value $\frac{KR}{10^6}$ of the line from which this diagram was derived is 3·75 seconds. Its C.R. when observed at 30 seconds was 5,155·5 ohms, but when observed at 10 seconds was 5,152 ohms, and would have been considerably less at 3·75 seconds; whilst above 30 seconds no rise of apparent C.R. was appreciable. Since absorption becomes thus approximately negligible after an interval of 30 seconds, this would appear to be a suitable period of charge to adopt in C.R. measurements, affording as it does convenient time to adjust the bridge balance. If seeking the C.R. of an *insulated* loop of two very long cables, 60 seconds might be advisable; but this is much better done by earthing the junction of the lines, when the time of charge of the loop becomes sensibly the same as for a single cable.

Moreover, on a long cable the observed C.R. not only requires time to become steady, but also needs correction for leakage. This again involves the selection of a definite value of the D.R. for the purpose. Thus it will not do to correct the apparent C.R. of a very long cable observed at 10″ by its apparent D.R. at 10″ charge, since the usual formulæ for distributed leakage are not applicable after so *short* an interval. On the other hand the 5th minute D.R.

is somewhat too great, but its value at 30″ charge yields a closely correct result. Hence 30″ seems to be the proper standard time for D.R. observations taken for the purpose of correcting the apparent C.R.'s at 30″.

As regards I.C. measurements, although it has been shown in the foregoing paper that the "modified Thomson" method yields a constant I.C. value for all times of charge, yet a 30″ charge would be a very suitable one to fix for such observations, because the balancing condenser shunt can then be calculated for the standard 30″ D.R.; and its final adjustment can be conveniently made during this interval without undue hurry. If any other method of I.C. measurement be employed, a definite time of charging becomes necessary for purposes of uniformity.

The President. The PRESIDENT: As this paper has been circulated among the members, and as I find, on communication with Mr. Murphy, who represents Mr. Young, that he is willing that it should be taken as read, I propose, with a view to avoid unnecessary curtailment of the time available for discussion, that this paper be taken as read, and that we at once proceed to the discussion of the paper.

Mr. Murphy. Mr. WALTER J. MURPHY: Mr. Young's paper will, I hope, prove to be a milestone on the road of progress towards a satisfactory method of obtaining the capacity of long submarine cables. It is a problem which has, I think, bothered most of those who have had to tackle it. At the outset of my career it certainly produced a feeling of despair and a hopelessness of ever being able to qualify as an efficient electrician in that respect, because it seemed to me that I could obtain any value I chose for a cable, by slight variations in the manipulation of the test, and could find nothing to guide me as to which of the various results I obtained was the correct one.

Perhaps I had better point out, shortly, the difference between the usual Gott method of taking the test, and that which Mr. Young has finally found to be the best, and advocates in his paper. Gott's method took the place of the original Thomson method because, by placing the earth at the end of the slides instead of at the index, the effect of battery leakage was done away with, or at least greatly diminished. But though leading to an improvement in this respect, Mr. Gott's plan unfortunately introduced a cause of greater disturbance; because it is evident that, when the condenser and cable are joined in cascade, the cable only receives a charge from the battery during the instant the condenser is being charged (Fig. A).

Immediately afterwards the battery further charges up the condenser through the high resistance of the dielectric; and the cable is simultaneously gradually *discharged* through the same high resistance (represented diagramatically in the figure by r).

Mr. Murphy.

If sufficient time is given, the condenser will, in fact, take up practically the full potential of the battery; while, on the other hand, the cable will be reduced to the P.D. existing between the ends of *r*.

This is mainly the process which requires that the slider should be moved further and further towards the earthed end of the slides, as the time of so-called charge is prolonged.

It is about nine years now since I first realised that, if a connection is established between the index or slider and the point of junction of the condenser and cable (the index being placed somewhere near the position necessary to produce a balance), the action which I have just explained is prevented, and the condenser and cable have the potentials necessary to charge them with equal quantities impressed upon them. This idea gradually grew, and was eventually communicated to Mr. Young, who suggested that we should collaborate in writing this paper. But a difference of opinion upon an essential point, and the fact that we could not easily communicate with each other, eventually decided him to write the paper alone, and leave slight differences between us to be decided by the discussion, or by future experiment.

Mr. Young has since his return to India tried the method[1] I suggested to him, and finds it does away with the difficulty which arises from the

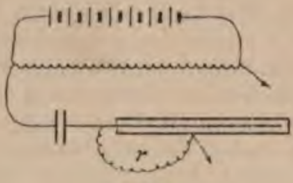


FIG. A.

action I have just described, and practically annuls the necessity of making any correction to the Gott test.

Another way of accomplishing the same end is to join a resistance across the condenser, which is inversely proportioned to the capacities of the condenser and cable, and to the dielectric resistance of the latter.

Thus if the cable has a capacity of 500ϕ and a dielectric resistance of 1Ω while the condenser is of 50ϕ, then a resistance of 10Ω would be placed across the latter.

The resistances R and *r* then assist to impress the proper potential difference upon their respective capacities.

The manipulation consists in watching, by means of the galvanometer *g*, placed in the index circuit, for the moment when, through the gradual rise of the dielectic resistance *r*, R and *r* are balanced (the index having previously been placed at a point which it is expected would give a capacity balance). When this occurs the charges in *c* and C are mixed by earthing K, and the accuracy of the adjustment of the index tested by ascertaining if they completely neutralise each other.

Another advantage which arises from placing the resistances across the condenser is that the effect of the earth or cable current is thereby

[1] First published in *The Electrician* of November 5, 1898.

compensated for; since it is evident from Fig. C that the E.C. will then be impressed upon each capacity in a proper proportion. Mr. Murphy.

Mr. Young refers to the effect of temperature upon the electrostatic capacity of cables. I took an opportunity, which offered a few months back, to ascertain if pressure or temperature really affected the true capacity of a cable. This occurred during the laying of 150 nauts of cable into a depth of some 1,000 to 1,100 fathoms of water, which meant changing the temperature of the cable from 83° F. to about 37° F. and increasing the pressure from one atmosphere to nearly twenty.

Careful tests were made for several days by the method advocated while the cable was in the ship's tanks, and at the higher temperature; and repeated when the cable was being laid out, and again after it had been down for some time, and had therefore settled down to the lower temperature, and greatly increased in dielectric resistance in consequence. No change in its true "free charge" capacity could be detected. I consequently conclude that if temperature affects the apparent capacity of a cable at all, it is due to the influence it has on

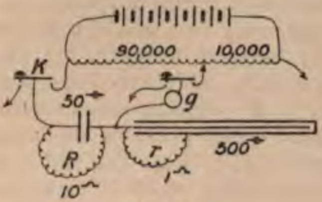


FIG. B.

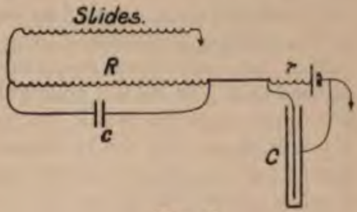


FIG. C.

the "absorption" and upon the dielectric resistance and not upon the true capacity of the cable.

Mr. SIEMENS: I should like to say a few words on this paper and the formula which is given therein. On the second page the author states, "The time-integral of current, or the electric displacement through the condenser, can be expressed in the form— Mr. Siemens.

$$y_t = Kx_t + \int_0^\infty x_{t-\omega} \left\{ \psi(\omega) + \beta \right\} d\omega.$$

This is the formula which was established by Dr. Hopkinson and Professor Wilson, where the first term represents "instantaneous" capacity, the second residual charge or absorption, and the third conductivity of dielectric, separated for convenience, but really all parts of a continuous magnitude. That appears to me a subject which it would be very well for young students to take in hand, to see how they can separate the three components of the capacity. You are all acquainted with the old experiment that if a Leyden jar is discharged you can get a very large spark at first, and then after a little time you can get some more sparks. A very similar thing takes place in signalling with submarine cables, which really consists in impulses being sent through, and which make it necessary that the cable should be charged and discharged at a rapid rate. The usual

Mr. Siemens. formula to express the function which governs the speed is K.R., where K. stands for capacity, and R. for copper resistance. But this is so far not quite correct, because that capacity ought to be split up into these components which Mr. Young gives. If you recollect, the usual illustration of capacity is that you look upon the insulated conductor as the hollow space in the middle, and electricity as if it were water which is to be run into the tube and run out again. Now, if there were only an instantaneous capacity, you can see that the plain tube would represent the insulator, and would therefore represent the phenomenon. But there is what Mr. Young calls residual charge or absorption, and that may be represented roughly in one's mind by a lining of the tube with some spongy material. You can easily see if the water is running in you have not only to fill the tube in order to get some water out of the other end, but you must also saturate the sponge, and if you want to get the water out before you introduce a second charge, you have to get the water out of the sponge, and hence the retardation. The great problem which cable-makers have before them is to reduce this spongy quality, if I may so call it, as much as possible, and that that is possible is clearly demonstrated by the table of cable speeds of the Atlantic cables, which was published some time ago by my firm, in which all the cables, not only those manufactured by Siemens Brothers, but all, showed that they did not follow the law of K.R. How very important it is to pay attention to this spongy factor of the capacity may be seen if you compare only the two latest Atlantic cables made in England, that is, the 1894 Anglo and the 1894 Commercial cables. You will find that the speed obtained on the Commercial cable, which is a copper conductor with 500 lbs. copper and 320 lbs. gutta-percha per nautical mile, and is 2,160 miles long, is somewhere between 36 and 40 words per minute. The speed on the Anglo-American cable, which is 1,850 miles long, has 500 lbs. of copper per nautical mile and 400 lbs. of gutta-percha, is 47 words per minute. Now, you are aware that the speed of speaking varies inversely as the square of the length of the cable, and, making all possible allowance, you will find that if the cable of the Commercial Companies were also 1,850 miles long it would speak at least as quickly as the Anglo-American cable. The direct proportion is that it would give 55 words per minute; but I give up those extra eight words because personal equations may come in, and I say it will only be equal. But you have the result that by attending to this second factor, namely, the spongy factor of the capacity, you can increase the speed through submarine cables to such an extent that a cable of 500 lbs copper and 300 lbs. gutta-percha per nautical mile can speak as quickly as a cable with 640 lbs. copper and 400 lbs. gutta-percha per nautical mile. I wish to take this opportunity of stating this publicly in the Institution, and to challenge the accuracy of these statements, because the publication of our table was met with some idea that there was a difference in the instruments, and other differences which accounted for those facts. I believe it is merely due to the fact that in the one case the spongy part of the capacity has been so greatly reduced.

Professor Ayrton. Professor AYRTON: I suppose it is of no use asking Mr. Siemens to add one word, and that is, the answer to the question, How?

The PRESIDENT: The discussion on Mr. Young's paper must now be adjourned to next week. I have to announce that the Scrutineers report the following candidates to have been duly elected. The President.

Members:

Captain John Nassau Chambers Kennedy, R.E. | Henry Wilson Young.

Associate Members:

Henry George Andrews. | Edmund Rowley Hill
William Johnston.

Foreign Member:

Einar Rasmüssen.

Associates:

Frederick Charles Dafforn. | Leon Harvey Lander.
Thomas Heffernan. | Robert Edwin Parker.
Henry Vincent James. | Thomas Plummer.
George Ernest Murray Stone.

Students:

William Banner. | John Thomas Irwin.
Allen Murray Coombes. | Karl Clifford Jacobsen.
Alfred Ambrose Harris. | Cecil W. Marshall.
Francis William Hewitt, B.Sc. | Edward James Morrah.
John Newton Archer Houblon. | John Guy Pointon.
Charles Hallewell Steele.

The Three Hundred and Thirty-Third Ordinary General Meeting of the Institution was held at the Society of Arts, John Street, Adelphi, on Thursday evening, May 4, 1899, Professor Silvanus P. Thomson, F.R.S., Vice-President, in the Chair.

The minutes of the Ordinary General Meeting held on April 27, 1899, were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council :—

From the class of Associates to that of Associate Members—

H. H. Berry.	T. W. Skinner.
C. P. C. Cummins.	H. G. Solomon.
H. H. Lewis.	A. K. Taylor.
David King Morris.	Duncan Watson.

From the class of Students to that of Associates—

William Collins.	Sidney John Roseblade.

Leonard G. Stanger-Leathes.

Mr. I. Braby and Mr. E. J. Brothers were appointed scrutineers of the ballot for new members.

Donations to the Library were announced as having been received since the last meeting from Herr Professor Arnold and Mr. Charles Bright, Member ; also a portrait of Mr. Edward Davy, by Mr. J. J. Fahie, Member, to all of whom the thanks of the meeting were duly accorded.

The CHAIRMAN : I have to announce that the Annual Conversazione of the Institution will be held on Thursday, June 15th, in the Hall of the Natural History Museum.

The next matter of business is the adjourned discussion on Mr. Young's paper.

Mr. MURPHY: As it did not seem clear to Mr. Siemens, and was probably not so to the rest of the members, that the test which is submitted to them actually *does* separate the absorption and leakage components of the total apparent capacity from the true or "free charge" capacity, I have asked to be allowed to add a few further remarks to point out that the real object of the discussion should be to decide whether the claim made for the method in this respect is a good one. Mr. Murphy.

It seems all the more desirable to make the true question in hand clear, because there was an evident danger of the discussion taking a course wholly irrelevant to the matter dealt with by Mr. Young's paper.

I must mention that Mr. Young and I are not quite in accord as to whether the absorption factor is completely eliminated from results obtained by my method of manipulating the test. My contention is this:—If a battery is applied to a cable or to a condenser which absorbs directly, and not "in cascade" as in Gott's method, the portion of the instantaneous charge which is absorbed by, or which leaks through the

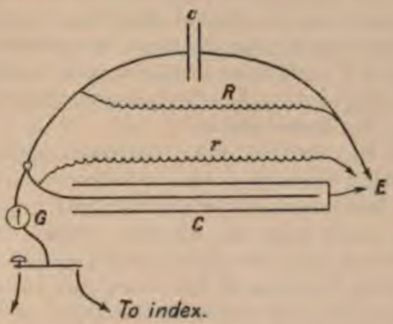


FIG. D.

dielectric, is continuously replaced; and the "free charge" is therefore kept constant.

This is true not only with regard to the cable but also with respect to the condenser (Fig. B.), so that if the index circuit is kept closed or if the resistances R and r are in the proper ratio during the time the battery is applied, the free charge of each condenser (including the cable under that term) is kept constant, and at its full value, until the moment of "mix."

It is only in the "mix" stage of the test that any doubt arises as to the action of the absorption upon the result. The condensers are then arranged as in Fig. D.

R and r now merely act as shunts. The question is—Does the absorbed charge leak out of the cable during the few seconds the mix period lasts, so fast as to add, to any appreciable extent, to the true free charge of the cable? In my opinion the discharge of the residual charge is so slow that the small amount which may be given out will not seriously interfere with the accuracy of the result, and is probably in any case proportional to the length of the core in circuit, and so will

Mr. Murphy.

not introduce errors in localisation. Even if the discharge of the residual charge is not delayed or damped back until nearly all the free charge is out, it is generally cancelled out of existence during the "mix" by a fairly equal amount of residual charge emaciating from the opposing condenser. The only precaution necessary is that the mixing period should not be prolonged greatly beyond the short time necessary to allow the true charges to mix, not only for the above reason, but because any difference between the "free" quantities, due to an error of index adjustment, may have time to escape *viâ* R and *r*. The proper time to allow for mixing can be calculated with the assistance of the K.R. law.

As I have already shown that the leakages on each side can be balanced up to the instant mixing is begun, and that their influence during that operation can also be discounted, I submit the problem Mr. Siemens has put to young students is, for practical purposes at least, solved. May I venture to say his appeal to young students is evidently justified by the fact that the old students, of whose school he is so bright a light, have not, in spite of their long innings, been able to devise a test which will separate the three components for us?

The Chairman.

The CHAIRMAN : I shall have to ask speakers kindly to observe the good rule of keeping to the subject in question, which is, the method of making this Capacity Measurement.

Mr. Bright.

Mr. CHARLES BRIGHT, F.R.S.E. : Telegraph papers are so rare in the present day, that when we have one I think it should be granted a full discussion. We have really two, if not three, papers for consideration here—quite apart from the electric locomotive that is coming along. First of all there is Mr. Elton Young's paper, followed by Mr. Murphy's double explanation of the same, and then Mr. Alexander Siemens gives us a little digression on the speed of Atlantic cables of various manufacture. I think I cannot do better first of all than follow the example of our worthy Past-President (Mr. Siemens). Mr. Siemens led off by calling the students' attention to the late Dr. Hopkinson's formula ; but the main point of his remarks seemed to refer to a table that was circulated by Siemens Brothers some months ago, of which no doubt many of us received a copy. I well remember that when I received one, it struck me that it was a great pity that these speed tests on various Atlantic cables had not been made by one and the same impartial individual and under precisely similar conditions commencing with the transmitting battery and ending with the receiving instrument, for purposes of satisfactory and true comparison. Of course the only comparison that we could reasonably make is that in regard to the two cables of the same year (1894), manufactured, as it happens, at different factories. With regard to one of these it may be mentioned that Mr. Arthur Dearlove—in the course of an article in *The Electrician* after the trials were made—gave us a *fac-simile* of the actual signals with which the trial speed was obtained on it, thereby giving an idea of their degree of definition. So far as I know, nothing of the sort has been published in connection with the trial speed set forth for the other cable referred to. If this were done, under the same conditions, we should then be able to compare the clearness of the signals as well

as their speed, and it is little use taking one feature into consideration without the other. The fact is if such comparisons of speed are to become the custom and of any value, we must have a standard type of signals to work to, with a standard set of instruments. Such a standardising system might lead to agreement amongst "practicians" as to the limits to which speed may be pushed consistent with definition and accuracy. It is already the custom with the larger cable-owning companies to stipulate for a certain degree of definition, bearing in mind that clerks vary in their efficiency; but other companies may obtain a higher speed, of course, with less definition. Mr. Bright.

Putting aside the personal element introduced—with which, of course, I have nothing to do—it would seem to me that one explanation of the increased speed on Atlantic cables of recent years as against those of formerly lies in the increased proportion of copper and the decreased proportion of gutta-percha, and here the factor of self-induction has no doubt been taken into account more seriously of late. On the other hand, Mr. Siemens specially calls our attention to the important part played by absorption in the problem of speed, and anything from Mr. Siemens naturally carries great weight. This is a subject I have always been greatly interested in from a research point of view. Presumably, a dielectric having a great power for absorption should be unfavourable to speed in so much as it tends to increase the effective inductive capacity of the cable and, therefore, the retardation. On the other hand I have heard the question argued the other way on different lines; and, in any case, it is, in my opinion, questionable whether absorption enters into the speed problem under ordinary conditions, though it is quite possible that it may do so when dealing with great retardation factors (as constituted by enormous lengths of cable), where the time period of contact for signalling is necessarily materially increased. The manufacturer so seldom comes out of his shell that it would be a great thing if we could induce Mr. Siemens to go a little further with his remarks. It is not improbable that he would be prepared to do so, for of late the principle of "give and take" has become largely recognised—as, for example, the late lamented Dr. Obach's recent Cantor Lectures on Gutta-Percha—and for this change of policy we have, I believe, largely to thank Mr. Siemens. In my opinion the table to which Mr. Siemens has drawn our attention does not give sufficient data to enable us to study the effect—if any—of absorption on speed, and if Mr. Siemens would enlarge on the subject I should be glad.

I quite agree with Mr. Siemens that the "K.R. law," as it now stands, cannot be relied upon strictly for great lengths; and we all know how substantially higher the speed on long cables is found to be when actually tried than when calculated.

The all-important question in regard to this matter is, in my opinion, What is the *effective* capacity of a cable from a signalling-speed point of view—in what proportion of the total electrostatic inductive capacity is it that enters into the "K.R. law," and how does this proportion vary with the length of the cable? May it not be suggested that the capacity which is entered into the K.R. formula for long cables should

Mr. Bright. really be only that capacity which would be indicated by the instantaneous charge test (applicable for lengths up to 50 nauts or so) taken by a contact of duration only equal to the duration adopted in signalling in the particular case in question?

I understand that Mr. Elton Young is at Aden; and this may, to some extent, account for his courage in presenting a paper on telegraphic matters to this Institution as at present constituted. Be this as it may, we have before us an extremely clear and concise treatment of the subject he has dealt with, such as is only in keeping with his recent literary contribution to *The Electrician* series on testing generally. I fancy that the value of really accurate capacity tests is not sufficiently appreciated. In my opinion it is a question whether paying out cable from a sealed end and testing in this way is not preferable to exchanging continuity signals with some one of doubtful technical reliability. But there are those who contend that, when picking up, you never get a seal with a broken end. This, however, is a mistake; for, as a matter of fact, when the apparently exposed copper end is seen coming up over the bows of the ship, there is, in reality, almost invariably a transparent film of gutta-percha over the end which, though it can only be detected by a magnifying glass, is quite sufficient to constitute a seal, and in the case of india-rubber—owing to its greater extensibility—there would never be any doubt about it.

Though the author of this paper commences by giving the Hopkinson-Wilson formula for definition of capacity increasing as the time increases, apparently he considers—rightly, I think—that the only practical course is to get at what is usually termed the surface or "instantaneous" capacity, obtained by eliminating the factor of time interval.

The author's paper mainly deals with variations or modifications of what has come to be commonly known as Gott's Capacity Test for long cables.

With regard to what Mr. Young says about Dr. Muirhead's correction for eliminating absorption, it would seem, for the moment, that by inserting a compensating resistance on the condenser side, Muirhead's correction might be made strictly applicable—*i.e.*, absolutely accurate even when testing cables. Of course, if we entirely neglect the absorption factor in a long cable we can obtain almost any result if a material time be given for charging—and it will vary with this period—as many of us have no doubt proved in practice.

So far as I can see from a short study of their method, Messrs. Murphy and Young, like Dr. Muirhead, profess to arrive at the instantaneous value. The main novelty in their procedure—for both seem to be about equally concerned in it—is apparently that of actually balancing the leakage and absorption (taken as one) of the cable, at the moment of obtaining balance for surface capacity. Bearing in mind the difference between theory and practice, I am inclined to reserve any comment on the method—in which Mr. H. W. Ansell seems also to have played a part—until I have had an opportunity of trying it, but the results certainly appear to be extremely satisfactory. It will be noticed, by the way, that all the tests on laid cables in the three tables give a higher

value than those at the factory, and this is, of course, always the case; Mr. Bright.
but I should be glad to know to what extent, if any, this is attributed to the effect of pressure. Possibly pressure comes in here more than has hitherto been supposed. Messrs. Siemens Brothers have, I believe, made a close study of the effect of pressure on a core, and their views on the subject would be of great value.

I observe that in Table III. the method proposed is referred to as a "Modified Thomson" test, though elsewhere in the text it is clearly shown to be a modified *Gott*. No doubt most of us know that Lord Kelvin (then Sir William Thomson) first described [1] both the original methods referred to, but the first method—independently suggested by Mr. J. Gott [2]—is always now known as the Gott method. And surely this discrepancy is liable to lead to confusion (especially as it happens that "mixing" is resorted to, but in an entirely different way to the second Thomson method proper). Why should not the authors attach their own name to the method described by Mr. Young?

Mr. Young has endeavoured to come at close quarters with the capacity variation of the gutta-percha by temperature, but I venture to think that here he has got on barren ground from a research point of view. The fact is, that if an exact result be obtained with one sample the co-efficient employed would probably not apply strictly to any other gum. When dealing, as here, in minute figures, the actual constitution and condition of the material has such an important bearing that the whole subject is, I fancy, scarcely worthy of the labour for practical purposes.

Mr. Young refers to the absorptive capacity of dielectrics being augmented by fall of temperature. This always strikes me as one of those curious facts connected with dielectrics generally which seems to call for further investigation, the reverse being what might reasonably be expected. It is, however, one which, in influencing the capacity, affects us more seriously in the matter of maintaining a balance in duplex telegraphy when using ordinary paraffin condensers in the tropics to balance a long length of cable at an average bottom temperature of, say, 40° F.

Perhaps the most important practical feature in connection with the paper is the Appendix; and I should like to take this opportunity of warmly supporting the author in his suggestion for fixing some outside definite and uniform time period for charge in connection with Conductor Resistance, Inductive Capacity, and Dielectric Resistance tests of long cables. Mr. Young has given the reasons for this very clearly, though I would venture to suggest that one sentence in the last paragraph is not as well expressed as it might be, and is, in fact, a little misleading. He speaks of "a constant I.C. value for all times of charge." Surely this should read "a constant result regardless of the time taken to secure a balance." In these days of Institution rules it would seem as though this suggestion of a uniform period for charging should almost become law.

With the fresh impetus likely to be now given to submarine

[1] See vol. i. p. 394, of the *Journal*.
[2] See vol. x. p. 278

Mr. Bright. telegraphy on an imperial basis—and with great lengths involved—any researches which tended to elucidate the problem of really accurate capacity testing and of an accurate modification—or rather extension—of the K.R. law should be of inestimable value; and it must be remembered that every extra word (or even letter) per minute got out of such cables will well repay researches of the latter description.

Mr. Taylor. Mr. F. A. TAYLOR: I should like to be permitted to add a few words to this interesting discussion from the point of view of one who has had considerable experience in the practical working speeds of submarine cables. It appears that Mr. Murphy has explained to us a method of taking the capacity of a cable, which, by eliminating the effects of absorption and leakage enables us to arrive at the instantaneous value of the capacity, quite irrespective of the time of charge—in other words his test eliminates the last two factors in the equation mentioned in Mr. Young's paper on page 476, and gives us a constant value which may well be taken as a basis for calculating future values for the localisation of insulated faults and disconnections, but which for signalling purposes is not the true capacity of the cable. In this latter connection it is impossible to disassociate the three factors which go to make up what I may call the working capacity, and it seems to me that a K.R. based upon an instantaneous value of K. would not be a true one, and would, if applied to the working speed of any cable, give a higher rate than could be obtained in actual practice. If, however, the value of K. be the true one, that is, a value arrived at by the inclusion of all three factors in the equation (and it should perhaps be borne in mind that even this may be only approximately true, as it seems to me that K. will vary with the speed) then if we could employ a receiving instrument having no mechanical or electro-magnetic inertia, I take it that we should obtain speeds which would be inversely proportional to the K.R. It has been the practice when calculating the speed for any given cable to take as a basis an empirical constant derived from the actual observed speed on some other cable of an entirely different K.R. value. Some experiments which I have recently had the pleasure of carrying out on behalf of the Eastern Telegraph Company convince me that this is an entirely erroneous method of procedure.

Having regard to the mechanical inertia of the signalling coil, to the elasticity of the fibre suspensions, and to the opposing E.M.F. set up in the coil by its motion through the magnetic field (which last effect is more marked as the speed is increased), I find that the present receiving instruments are unable to respond fully to the extremely rapid impulses which can be imparted to them through short cables, but are in a position fully to respond to the slower impulses of longer ones. The consequence is that if we take the fastest observed speeds with legible signals upon K.R.'s, say from one to eight millions, using in every case the same instrument, and adjusting it so as to give the highest possible speed in every case, and we then multiply these speeds expressed in words per minute by the K.R., through which they have been obtained, we get a sliding scale of constants differing widely in value and increasing with the K.R. From this I may say in conclusion it is evident that until we have a receiving instrument whose inertia is

negligible we cannot arrive at a constant which will be equally applicable to all K.R.'s, and therefore, at present, calculations based upon a common constant must necessarily be misleading. Mr. Taylor.

Mr. WILLOUGHBY S. SMITH: I wish to congratulate Mr. Elton Young and Mr. Murphy upon the useful method they have given us for determining the inductive capacity of submarine cables which, as is well known, is attended with difficulty. I must confess, however, that I really rose to break a lance with Mr. Siemens, and much regret that this is barred by the Chairman's ruling. Mr. Smith.

The CHAIRMAN: I hope you will send your remarks with the request that they be inserted in the Journal, for the Council will be willing to take the matter into consideration, but I do not think it would be right to lengthen out the debate here by one speaker in the debate answering another on a matter which is not relevant to the paper. We hope you will write your remarks and send them to the secretary for the Council. The Chairman.

Mr. WILLOUGHBY S. SMITH [*communicated*]: In reply to Mr. Alexander Siemens, I will briefly point out that the highest authorities, including Lord Kelvin, are agreed that the "K.R." of a cable is a correct indication of its theoretical speed. Surely then Mr. Alexander Siemens must recognise that such a well-established law can only be upset by strictly scientific evidence, but, unfortunately, instead of producing such evidence, he relies upon a tabulated statement, which is certainly misleading. Mr. Smith.

Mr. Siemens, when drawing up this statement, evidently overlooked the fact that, with the exception of the cable laid in 1894 by the Telegraph Construction and Maintenance Company, all the K.R. values are calculated from tests taken at 75° F., whereas the values given for the former cable are based upon tests taken after laying, and, therefore, at a very low temperature. I would also add that, since we have no standard of efficiency either as to definition or amplitude of signals, such comparisons as to speeds are most unreliable, especially when no data is furnished as to the electro-motive force employed.

Mr. A. DEARLOVE [*communicated*]: We are all indebted to Mr. Young for his paper, and the suggestions he makes with regard to the method he advises should be used when measuring the capacity of long cables, it represents undoubtedly some advance on the method of testing more usually employed, and I quite agree with Mr. Young that a recognised standard of "time of charge" with regard to this test should be adopted. Mr. Dearlove.

The writer first used the correction method and formula

$$C = K \frac{10000 - (R \mp P)}{R \mp P.}$$

in 1891 and pointed out in the *Electrician* of July 10, 1891, that "no correction for leakage is found when the insulation resistance per microfarad is the same in the Standard Condenser and the cable compared, but the correction becomes necessary when large differences of absorption exist between the two capacities under test."

This is very obvious, but the formula just given combines a correction for both absorption and insulation resistance differences in the

Mr. Dearlove.

capacities compared, and speaking from experience this correction is a sufficient one.

There is very little or no evidence that the capacity of gutta-percha cables alters either under the influence of temperature changes or pressure, nor can, so far as I know, much evidence be found in tests of laid cables.

In the last paragraph of the appendix to Mr. Young's paper it is stated that "If any other method of measurement be employed, a definite time of charging becomes necessary for purposes of uniformity."

The following remarks bear on this point. When testing a cable of 1850 nautical miles and using the Muirhead correction I obtained results which perhaps might have been still better had Mr. Young's "balancing" system been employed, but as they stand, they do not show any marked difference in the results obtained with the time of charge varying from 5 to 60 seconds. The particulars of these tests are :—

Standard Condensers 80 Microfarads ; Gott method with reversals and correction applied.

Number of observations.	Time of charge. Seconds.	Time of mixing. Seconds.	Corrected results in microfarads.
4	5	5	768·6
12	10	5	772·3
6	20	5	775·4
8	30	5	773·4
10	40	5	775·3
6	50	5	775·8
5	60	5	775·9

Another series taking on the same method using 241·2 microfarads as a standard gave :—

Number of observations.	Time of charge. Seconds.	Time of mixing. Seconds.	Corrected results in microfarads.
4	5	6	772·7
4	10	6	774·4
4	20	8	775·2
4	30	8	776·1
4	60	10	776·4

The aggregate capacity of the cable from factory tests of the core at 75° F. was 776·81 microfarads.

In the above table the time of mixing given was found to have no very sensible effect in the final value.

The maximum difference shown in the latter figures when 241

microfarads was used as a standard, if the value of the 5-second charge is neglected less than 0·3 per cent., a result which if not showing uniformity is very near to it, these tests are quoted as an instance of what may be done with the present known methods. Mr. Dearlove.

With regard to Mr. Young's remarks on the effect of temperature on the capacity of gutta-percha cores, my firm, Messrs. Clark, Forde, and Taylor, have made many experiments on this point, but the results arrived at are so discordant that, though I mention the figures obtained, I do not think it probable that any definite law for this variation can be stated.

In one very complete series of tests with one coil of core kept at a temperature of 75° F., and used as a standard of comparison, whilst another coil of similar manufacture and dimensions was tested and compared at the different temperatures of 75°, 60°, 50°, 42°, and 35° F., the results showed that the capacity of the coil *increased* as the temperature was *lowered* to the extent of 0·8 per cent., for a range of 40° F.

As a check upon the foregoing, a comparison of the capacity of each coil was made with a shellac condenser, the mean difference in capacity given by this method showed an increase of 0·94 per cent.

The average increase of capacity for this range stated, viz., 40°, would therefore be equivalent in the one case to ·02 per cent., and in the other ·023 per cent. per 1° F.

On the completion of the tests the second coil was raised again to a temperature of 75° F., and it was found that no sensible change of capacity had occurred by "ageing" effect.

When the two coils are at the same temperature, the insulation resistance and absorption of both being alike, the correction for all times of change was found to be nil.

On the other hand, using a variety of gutta different from the above, two similar coils were tested in the manner described, and with these, no change in their capacity occurred when their temperature was varied over a range of 40° F., whilst yet another pair of coils moved in the opposite direction, viz :—*increased* temperature, resulted in *increased* capacity.

The variation of the capacity of gutta-percha cores due to changes of temperature is undoubtedly a very small one, and unimportant. Moreover it seems more than probable from our experiments that such variation that does occur is dependent on the nature and quality of the gum under test.

The CHAIRMAN: Mr. Elton Young is unfortunately not in England, but if Mr. Murphy wishes in his name to make any reply to the criticisms that have been passed we shall be pleased to hear him. The Chairman.

Mr. MURPHY: Nothing has been said with regard to the test itself, so I suppose I must go on to the questions which have been put with regard to the branch subjects. With regard to Mr. Siemens' question, if I am correct in regarding the rate at which gutta-percha absorbs and gives out its residual discharge as a slow one, it is clear that any slight change Mr. Siemens' firm may have been able to make in the *spongy* quality of their cores will have little effect on the speed of their cables. I think you will be prepared to believe with me that the operation of Mr. Murphy.

Mr. Murphy. filling and emptying of the tube as a tube will be so overwhelming great a factor in determining the speed that the sponginess of the inner coating, to use Mr. Siemens' words, will, under the above circumstances, be negligible. I can, I think, explain how Mr. Siemens has obtained the astonishing figures he has ventured to quote to this institution. In the first place, it has been clearly proved by Mr. F. A. Taylor, than whom probably no one has had a greater experience in measuring the speed of cables, that the longer a cable or the higher its K.R. the better the speed constant will seem to be. This is in a great measure due to the fact that the slower working speed of long cables is less affected by the disabilities of the receiving instrument which handicap the shorter cables to a marked extent, even if measures be taken to modify the adjustment of the instrument, and otherwise assist it to give the best signals procurable under each condition. Mr. Siemens does not advance any evidence that the signals obtained on the Commercial cable were as good as those obtained on the Anglo.

There are at least two other points which Mr. Siemens has omitted to consider. First, the Commercial cable has a proportionally thicker dielectric, and therefore a lower capacity per knot, and cannot be rigidly compared by the K.R. law with the Anglo unless this is taken into account. It is wrong in any case to estimate the K.R. by multiplying the capacity and conductor resistance per knot by the lengths, because a greater proportion of a long cable will probably lie in deep water, and consequently have a lower K.R. than a shorter one. That affects the Commercial cable especially, because its conductor resistance per knot is higher, since the copper is lighter, and the temperature will have a greater effect on the total K.R. of the cable in consequence.

The actual observed K.R. only is worthy of acceptance for the purposes of calculation. But Mr. Siemens does not even seem to have paused to estimate the K.R. in trying to annihilate the poor Anglo cable. He prefers to reduce the speed of this cable to that of the shorter one by taking the inverse proportion of the square of the lengths. That is to say, he has, of the factors $(k \times l)$ $(r \times l)$, which go up to make K.R., rejected k and r because, I suppose, their retention did not suit his purpose. Until the speed of cables can be compared by some such instrument as the oscillograph (and I have urged its adoption for this purpose) or the polarised light recorder of Messrs. Squier and Crehore, we shall be unable to assert the K.R. law is false, although it probably is not absolutely accurate since it does not take the self-induction of the core into account. And that the self-induction of the core is an important factor may be judged from what follows.

It is now the custom introduced by Messrs. Siemens to build the conductor up of ten strands of copper round a thicker one instead of seven strands of equal diameter. The effect of this is threefold at least. In the first place, the average thickness of the gutta-percha is increased (weight for weight), and the capacity consequently decreased; in the second place, there is a slight decrease of C.R.; and in the third place, the self-induction is evidently increased. This latter tends perhaps slightly to heighten the apparent resistance of the conductor, but it also seems to decrease the effective capacity of the cable,

or at least a portion of it. That this is the case seems to be satisfactorily proved by the fact that in obtaining a duplex balance with the eleven-strand core the line resistance of the artificial line has to be increased, while the capacity has to be decreased to an appreciable extent to obtain a good balance of time ratio between the cable and the artificial line. It will be interesting to know if the "taped" conductor core recently introduced by the Telegraph Construction and Maintenance Company possesses these desirable properties to a still higher degree, as it is reasonable to expect it will. Mr. Murphy.

I would like, finally, to mention two interesting observations I have made when taking capacity tests by Siemens' fact of charge method.

The first is that the *slopping* about of the charge in the cable, which Mr. Oliver Heaviside foresaw should take place, is actually evident on the galvanometer even when the resistance of the path of discharge is in the order of one quarter to two megohms, the capacity being in that of five hundred microfarads.

The second is that the rate of discharge under the above conditions is much faster during the first third and much slower during the latter two-thirds of the discharge than, according to the Siemens formula, it should be. (See Young's "Electrical Testing for Telegraph Engineers," pages 86, 114.)

In the first case the waves, which seem to be analogous to those which would take place if a long narrow trough of water were discharged from one end, probably existed because at that slow rate the self-induction was too small to damp them down. In the second case self-induction may probably have assisted in quickening the first—more rapid—portion of the discharge, and the lagging of the latter part of the discharge have been due to the first indications of discharge of the absorbed or residual charge.

Whatever the true explanation of these phenomena, it is certain that the Siemens formula fails as decidedly to satisfy the truth, so far as the slow fall potential is concerned, as the K.R. does to indicate the rate at which alternations may be conveyed.

[*Communicated.*] Mr. Dearlove's communication affords very opportune illustrations of the defects which are characteristic of the Gott-Muirhead method at its best. From the tables he gives it will be gathered that he uses reversals, applies a correction, and is forced to make a number of observations for every test.

The reversals are necessary, because the E.C. is not otherwise neutralised.

The correction is a consequence of the facts (1) that reversals are used; (2) that the D.R.'s per micro. are not equalised; (3) that the "Muirhead method" is not a null one.

A number of observations are required, because the E.C. is variable, and the "discharge" and "reproduction" deflections are consequently also variable.

It is fairly obvious that in the cases he cites, the D.R.'s were nearly balanced, otherwise an alteration of the time of charge would have caused a greater divergence of his results. In any case his most constant and most accurate results were obtained with the longer

Mr. Murphy.

periods of charge, a fact which I beg will be borne in mind when what follows is considered.

It is absurd to say that the time given to "mix" is unimportant in Muirhead's method, because the correction depends entirely on the quantity remaining after mixing, and unless this is measured immediately it follows that some of that quantity will be lost by leakage, and the resultant correction will be too small.

That alteration of "mix period" did not, in his cases, affect the results to any important extent, is only another indication that the D.R.'s were nearly balanced, and that the correction was, therefore, small. But if with nearly a balance of D.R.'s he obtains such good results, why not come with me a step further, and, by balancing the D.R.'s exactly and prolonging the time of charge until the period of "unrest" has passed, do away with the correction entirely.

The existence of a correction destroys the nullity of the "slide method."

Reduced to its simplest, my argument is that the test should be taken when the curve representing electrification of the cable becomes sensibly horizontal.

This implies that I do not hold with the custom of discharging the

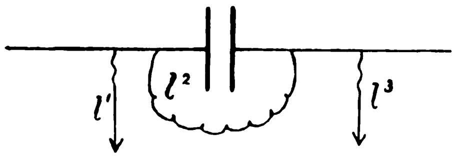


FIG. E.

condensers after every observation unless, of course, it is necessary to obtain a series of observations with the opposite sign current—a contingency which may occur in the case of a faulty cable—but prefer on the contrary to keep the dielectrics in as complete a state of polarisation or electrification as possible.

It has been the practice hitherto to attempt to test whilst the electrification was at its "perpendicular" period (see Fig. 1 of Mr. Young's paper). By allowing the charge to continue until the change rate becomes very slow, and arranging that any loss of free charge, due to absorption or leakage, shall be continuously replaced (by connecting the battery to each of the compared condensers), the test is made when the potentials throughout the system have assumed a practically steady value, and a calm, unhurried measurement may then be obtained. If Mr. Dearlove would give my plan a trial, I am sure he will ever after "use no other."

I am glad to find that Mr. Dearlove confirms my conclusion that the effect of temperature upon the true capacity is not appreciable, but I would suggest that it might be interesting to repeat his comparisons of gutta-percha core coils, taking into account any difference the variations of D.R. may make, either to the ratios of the D.R.'s *per se*

or to the resultant ratios of the slide arms. It is a question I mean to investigate if even laboratory tests are free from error in this respect. Mr. Murphy.

I find the slides themselves are apt to vary in their leakage, and in the resultant position of that leakage, sufficiently seriously to affect the values obtained.

Condensers (Fig. E.) also have three effective leakages thus :—

$$l^1, \quad l^2, \quad l^3,$$

a fact which points to the desirability of reverting to Lord Kelvin's original connections, because the leakages throughout the system would then be more probably symmetrically distributed. The three condenser leakages are thus reduced to one effective leak. Battery leakage (the *raison d'être* of the so-called Gott's form) can easily be provided against.

With regard to a "standard time," I have shown such a thing to be unnecessary as far as capacity testing is concerned. Mr. Young referred to it in connection with conductor resistance tests.

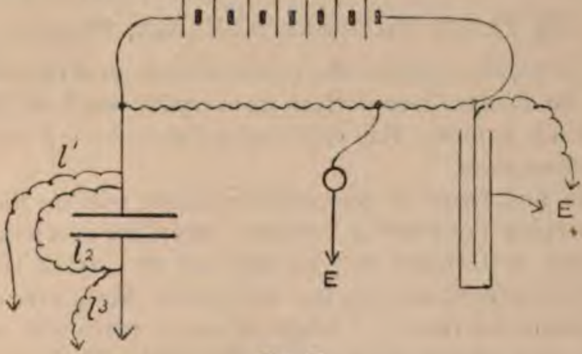


FIG. F.

My view is that in that case, also, the time of charge given should be sufficient to allow the electrification to settle down to its practically horizontal value. It is the only way in which the time factor can by effectually eliminated. The D.R. operating when such is the case, can be calculated from the observed C.R.'s by formulæ published by me in the *Electrician*, August 12, 1898.

The CHAIRMAN: Before I ask for a vote of thanks to Mr. Elton Young, I would like to add just one remark which was called out by the observation of Mr. Murphy, namely, that we must distinctly acknowledge that we owe to Mr. Oliver Heaviside the very important suggestion that in cables you cannot neglect the self-induction of the copper core. He himself suggested that the speed of signalling should be accelerated by adding to the self-induction of the core. This is a matter which is clearly of great significance, more especially when one takes into account the fact that modern methods of signalling are getting every year more and more away from the old idea that you simply start a current and then stop it again, with sudden and abrupt The Chairman.

The Chairman makes and breaks. Signalling is becoming more and more like an alternating current, a series of impulses which begin and increase and die away; and the nearer we get to signalling with alternating currents, the further do we get away from the strict and literal application of the K.R. law, and the more important does it become to take into account the effects not only of capacity and of absorption, but also of self-induction.

I take it that the Institution will give a cordial vote of thanks to Mr. Elton Young, and with that vote, which I now beg to propose, we must couple the name of Mr. Murphy, for taking charge of the paper, and for his contribution to the discussion.

Carried by acclamation.

The CHAIRMAN: I have now to call upon Mr. McMahon for his paper.

ELECTRIC LOCOMOTIVES IN PRACTICE, AND TRACTIVE RESISTANCE IN TUNNELS, WITH NOTES ON ELECTRIC LOCOMOTIVE DESIGN.

By PETER VALENTINE McMAHON, Member.

This paper contains the result of tests and observations made on electric locomotives in everyday work on the City and South London Railway, and extends over a period of about two years.

The first series of observations made was on the effect of "varying the starting current" upon the acceleration of the train, and shows that the amount of current taken by the locomotive at starting has not much effect, within fairly wide limits, on the time taken to run a particular section, or the total energy drawn from the line. This, of course, applies to a series motor, with plain series method of regulation. Experiments were then made to determine the nature and amount of the various losses in the locomotive, both at starting, and when the train had attained its full speed. To arrive at this some of the motors were tested under various conditions as stationary motors, and the results are compared with tests of the motors running on the rails under ordinary conditions of working.

Dynamometers with a diaphragm have not, so far as the author is aware, been used for this class of observation before, and they are therefore fully described. The "locomotive dynamometer" with a rubber diaphragm was used in tests for runs extending over 100 miles; it is still in good condition, and was calibrated before and after a test, time after time, and its readings agreed with wonderful

accuracy. From the results of locomotive tests with dynamometers on the rails, the "tractive resistance" of the trains running in the tunnels was obtained. Upon these experiments were based all the calculations and deductions arrived at in the notes on locomotive design.

The "tractive resistance" per ton of train as shown by

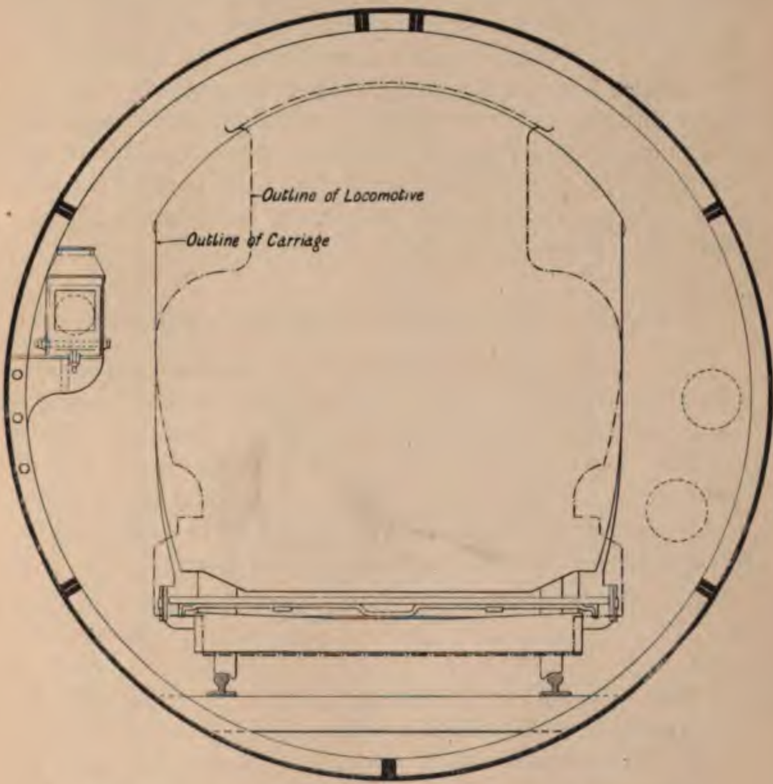


FIG. 1*a*.—Section of Tunnel, showing cross-sectional outlines of Locomotive and Carriage.

the tests may appear high as compared with the results obtained on ordinary steam main lines. This high resistance is no doubt due to the skin resistance of the train to the air in the close-fitting tunnel. Further it must be remembered that the train wheels are only two feet in diameter, which is about half that of a main line carriage

wheel. Fig. 1*a* shows the outline of locomotive and train in the tunnel.

Objections may be raised to the varying character of some of the figures in the tables. Experience of some thousands of observations on the locomotives shows that the values of current, speed, and draw-bar pull vary within certain limits over the same piece of track without any apparent reason. To insure accuracy the curves and figures given by way of illustration were chosen as representing a fair average of several trials, and the horse-power, efficiencies, &c., are the mean of these averages.

In dealing with the question of locomotive design, the results on a level piece of road only were considered, but obviously the conclusions arrived at also apply to a line with varying gradients, due allowance being made for the effect of gravity.

DESCRIPTION OF LOCOMOTIVES AND CARRIAGES.

Before describing the experiments or detailing the results, a short description is given of the locomotives and carriages employed. The three locomotives used, Nos. 12, 15 and 17, were built to the same general design and overall dimensions, the only outward difference being in the shape of the cab, which in the case of 15 and 17 is bulged at the sides, instead of straight as in the case of No. 12.

Leading Dimensions.

	Ft.	In.
Gauge	4	$8\frac{1}{2}$
Overall height above rail level	8	$5\frac{1}{2}$
Overall length, buffer to buffer	14	0
Extreme width	6	10
Wheel base	6	0
Diameter of wheels	2	3
Width of tyres	0	$4\frac{3}{4}$

Each locomotive has two motors, the armatures of which are fixed directly on the axles. The motors are arranged in series, and the regulating gear consists of a reversing switch, which changes the armature connections, a simple rheostat switch in series with the motors, and a main break switch.

The remaining details are given in the following table, which makes comparison easier.

	No. 12 Locomotive.	No. 15 Locomotive.	No. 17 Locomotive.
Type of Motor	2 pole Edison Hopkinson	2 pole Siemens	2 pole Edison Hopkinson
Section of Magnet	17" × 10"	24 × 10"	20" × 10'
Weight of Magnets, complete with bearings	40 cwt.	53 cwt.	44 cwt.
Type of Armature	Gramme Ring	Drum	Gramme Ring
Diameter of Core	17½"	14⅜"	17½"
Length of Core	17"	24"	20"
Number of turns	540	500	540
Cross-section of Conductor	No. 10 = ·01286 sq. in.	·400 × ·078 = ·0312 sq. in.	No. 10 = ·01286 sq. in.
Number of segments in commutator ...	54	125	54
Weight of Arm with wheels complete	19 cwt. 3 qts.	21 cwt.	22 cwt.
Armature resistance ...	·3 ohm.	·181 ohm.	·315 ohm.
Total resistance of Locomotive	·8 ohm.	·615 ohm.	·85 ohm.
Total weight of Locomotive	10 tons 7 cwt.	13 tons 10 cwt.	11 tons 12 cwt.

Each train consists of three coaches coupled together, with intermediate platforms. The following are the

Leading Dimensions.

	Ft.	In.
Gauge	4	8½
Overall height above rail level	8	4¼
Overall width of body	6	10
Length of body over end framing ...	26	0
Centres of bogies	16	9
Wheel-base of bogies	5	0
Overall length of train (3 coaches) ...	96	0
Diameter of wheels	2	0
Weight of empty train complete	21 tons 2 cwt.	
Seating capacity	96 passengers	

DYNAMOMETERS.

Some sort of dynamometer was found necessary in the earlier tests of the static pull exerted by the various locomotives, and that shown, Fig. 1, was designed and constructed.

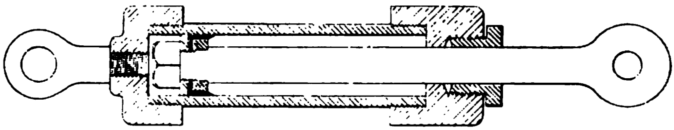


FIG. 1.—Hydraulic Piston Dynamometer.

It was made from a piece of steel tube 3 in. in diameter, with a wrought-iron cap and eyebolt screwed at one end, and a stuffing-box at the other, and was filled with water. On one end of the piston-rod an eye was forged, into which the draw-bar pin was inserted; the piston was fitted with the ordinary cup-leather. A pressure gauge was fitted close to the stuffing-box. This dynamometer was calibrated by hanging known weights on the draw-bar end, the other being slung up by a crane, the reading of the pressure gauge being recorded for each weight; in this way the calibration curve *a*, Fig. 2, was obtained. This dynamometer proved very useful, and could measure a pull of 6,000 lbs., but had the rather serious fault that small forces could not be accurately measured on account of the enormous friction of the stuffing-box and cup-leather. In addition, it was too long to be used between a locomotive and train in the ordinary running, for when the train ran up or gained on the locomotive,

the dynamometer was likely to touch the working conductor, causing a bad short circuit. It was therefore necessary to design a compact dynamometer in which the internal friction was a minimum. This was effected in the design shown, Fig. 3, which was inserted in the train draw-bar cradle in the place usually occupied by the spring. The available space in this cradle was limited to 6 in. across, and even if a larger cradle had been made, the clearance between the centre of the draw-bar and the conductor at the crossings

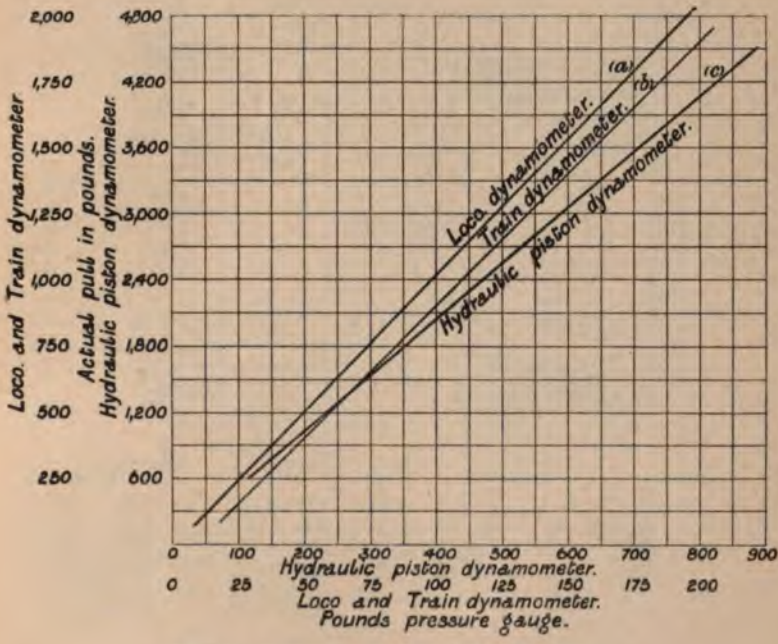


FIG. 2.—Calibration Curves of Dynamometers.

would have prevented anything greater than a 6 in. diameter being used. The difficulty was again increased by having to allow a $1\frac{3}{4}$ in. hole in the centre to take the shank of the draw-bar. The containing vessel or chamber was a cast-iron ring, $5\frac{7}{8}$ in. in diameter, with a $1\frac{15}{16}$ in. hole in the centre. A recess 1 in. wide and $\frac{1}{2}$ in. deep was turned in the face, and two steel rings were screwed on to make the joint between the face and the diaphragm. The circular ring and cap exerted pressure on this diaphragm. This was transmitted to the water, and its amount was recorded on the gauge.

This form, which we may call the train dynamometer, gave very satisfactory results, but it was found inconvenient, for reasons explained later, to have the dynamometer fixed permanently (during testing) in the train draw-bar. In order to get the dynamometer on the locomotive draw-bar, the design had to be completely altered, on account of the space available. The same idea was retained, but two diaphragms were employed with a passage between the chambers. The dimensioned sketch, Fig. 4, probably makes

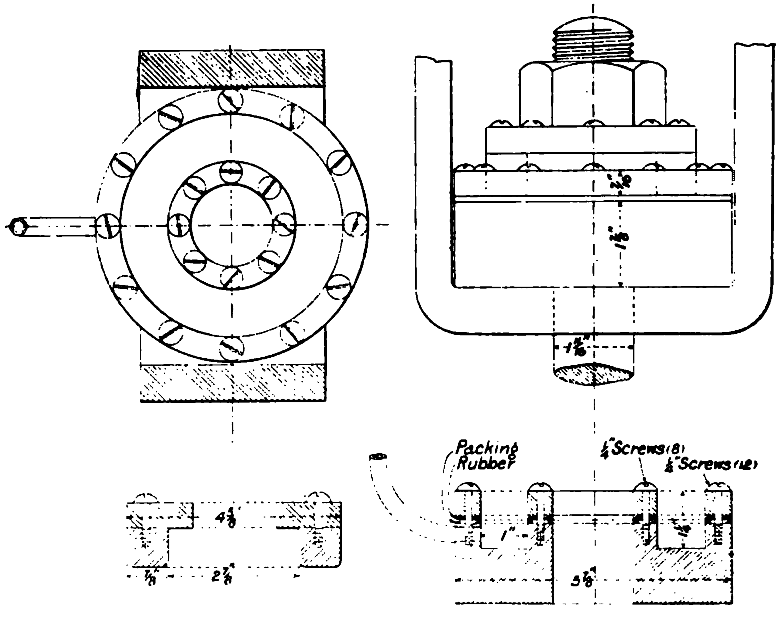


FIG. 3. Hydraulic Dynamometer for Carriage.

further description unnecessary. In actual working, this locomotive dynamometer gave even better results than the train dynamometer.

Considerable difficulty was experienced in finding a suitable material for the diaphragm. Very thin sheet-copper was first tried, but the sudden and uneven jerks tore the copper close to the joints. Plain indiarubber about $\frac{1}{16}$ in. thick was next used, but with this it was difficult to make a joint, as when the rings were screwed up sufficiently to make a joint to stand a pressure of about 200 lbs. per square inch,

the rubber expanded considerably and raised a bulge in the middle, which was found to affect the satisfactory working of the dynamometer. The ordinary rubber insertion, with wire gauze, was then tried, and gave very good results; but

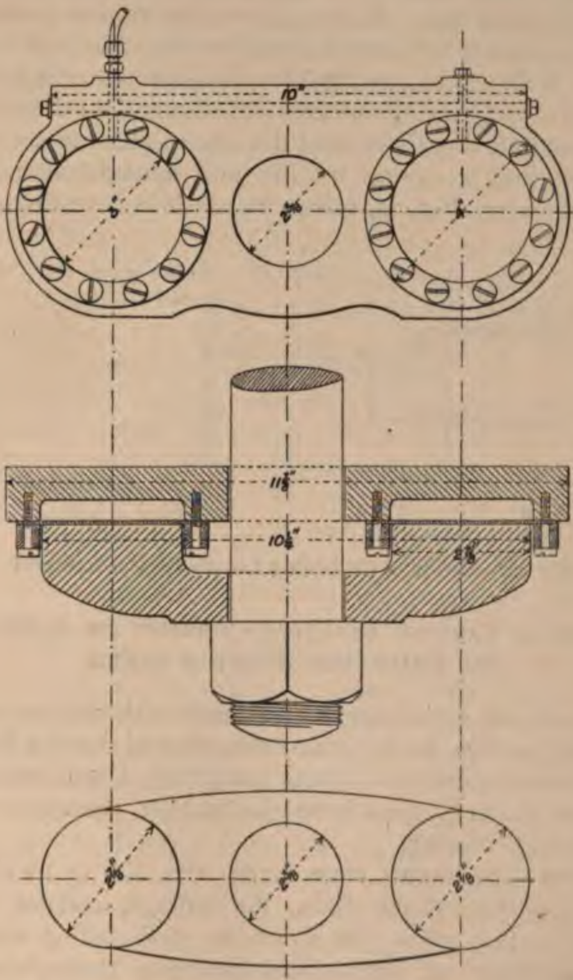


FIG. 4.—Locomotive Dynamometer.

the continual jerking caused the wire gauze to crack close to the joint, and then the pieces of wire soon came through the rubber. It may be said, however, that this last diaph ran for about 50 miles before giving out. A diaph

rubber and canvas insertion was next employed, and so far has proved satisfactory.

The calibration curves shown, Fig. 2, were obtained in each case by testing the dynamometer in position on the respective draw-bars with a spring balance and lever, with knife-edge bearings. In the case of the train the necessary pull was exerted by a screw, between the train and a fixed point. With the locomotive the draw-bar was attached to a fixed point and varying currents were used; then the locomotive static pull was checked at the same time, and compared with the pull as tested by the first dynamometer. The annexed sketch, Fig. 2*a*, makes the method of testing clear.

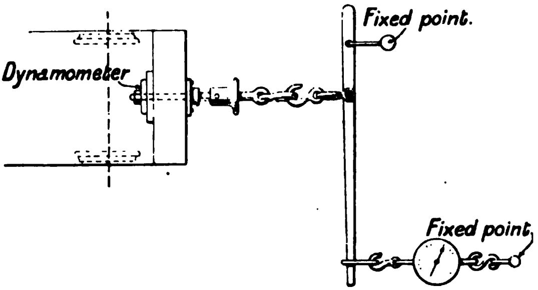


FIG. 2*a*. Method of Calibrating Locomotive Dynamometer.

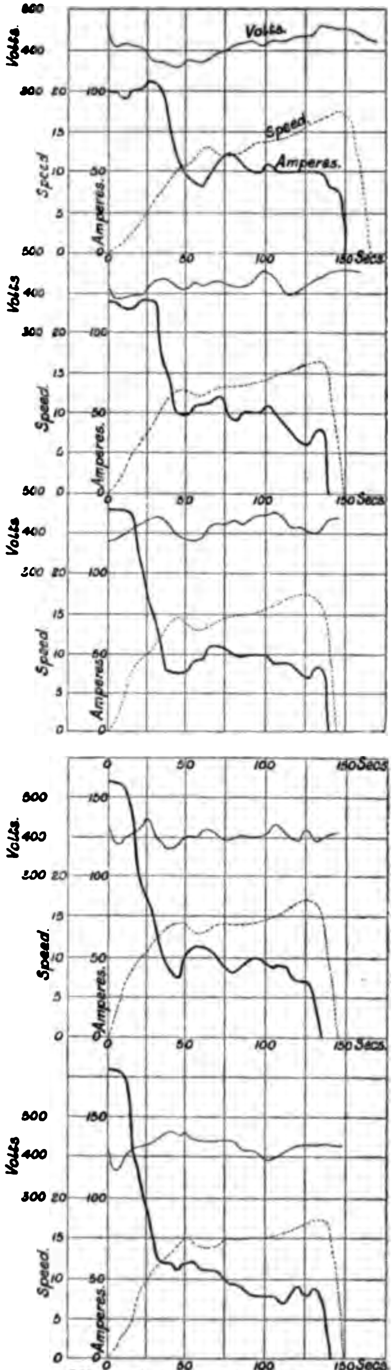
EFFECT OF VARYING STARTING CURRENT ON ACCELERATING TRAIN AND RUNNING SPEED.

A series of experiments was made with various starting currents, in order to determine the effect of using a large or small draw-bar pull at starting on giving a train its acceleration at starting, apart from acceleration, positive or negative, during running.

These experiments were made with No. 15 locomotive hauling a three-coach train, the voltage, current, speed, and in a few cases the draw-bar pull being observed simultaneously at five-second intervals throughout the run. Before carrying out these experiments, it was thought there existed a particular value of the tractive coefficient per ton (for any locomotive), both at starting and while running, which would give a minimum time between stations combined with a maximum efficiency for the energy expended. To arrive at a basis of com-

parison the total time between stations was divided into two periods, the starting and the running periods, the former being taken from the instant of switching on the current until the train received its acceleration, and the speed curve bent over and become parallel to the abscissa. This point was rather hard to determine, on account of the curve rising or falling almost immediately after the bend, but an inspection of the curves shows that their general shape is precisely the same for the same piece of road or section of the line, no matter what tractive force per ton was employed. The running period was taken from the last period described until the current was shut off and the brakes were applied. As the same man drove the locomotive on every occasion, and was accustomed to switching off within a certain distance of the position at which the train should stop, any little difference, due to shutting off before or after a certain point on the section was reached, can be neglected. Two complete round trips were made under ordinary running conditions, the only exception being that in the first trip the starting current was limited to 100 amperes and kept steady by the regulating switch until all the resistance was cut out, and the current commenced to drop on account of the back electro-motive force of the motors; the second trip was made with a starting current of 120 amperes; the third with one of 140 amperes; the fourth with 160 amperes; and the fifth with 180 amperes—starting from each station and maintaining these various currents steady, as in the first case, until the back electro-motive force brought the current down. As might be expected, this back electro-motive force destroyed, to a certain extent, the value of the experiments, but nevertheless they are, perhaps, worth recording. In leaving Stockwell on the up journey, and the City on the down journey, traffic regulations prevented the higher starting currents being kept steady, and therefore the observations obtained in the above-mentioned sections were not compared; neither are those obtained in any of the up-sections given, it being found that they are all much alike and that they point to the same conclusions. The trips between the Borough and Stockwell stations are therefore chosen for consideration. The preference is given to the latter on account of the trains carrying their maximum load on the down road, the

observations having been taken in the evening. Table I. gives the results fully worked out; the areas were in every case taken out with a planimeter, and from these the averages were obtained. It is hardly necessary to discuss in detail the results of each section, the variation in each being almost the same. The section between the Elephant and Castle and Kennington stations will be considered, but the remarks apply equally to the other sections given in Table I. For starting currents of 100, 120, 140, 160, and 180 amperes (see curves, Fig. 5); the velocities are 13, 14, 14·5, 14 and 14·75 miles per hour respectively; the difference in the speed attained is more marked at this station than the others, probably on account of the rising gradient on leaving the station. The mean speed is 14·05, but generally it seems to be independent of the value of the starting current. There is, however, a difference in the time taken to attain this velocity, the time decreasing as the starting current is increased, viz., 60, 50, 45, 40, and 45 seconds for current values of 100, 120, 140, 160, and 180 amperes respectively. It will be noticed that this relationship decreases as the higher values of current are reached; this is caused by the current falling off more quickly for higher values, the average current during the start being, therefore, much below its proper value. An inspection of the speed and current curves makes the reason clear; as the current or tractive force per ton is increased, the acceleration (in feet per second per second) becomes greater, and in consequence the back electromotive force of the motors prevents the initial value of current being maintained until the train has received its velocity. Had it been possible to increase the impressed E.M.F. or line pressure, the current could have been kept steady and more valuable results would have been obtained. The same effect might to a certain degree be brought about by weakening the field of the motors, but this would give less tractive force per ampere. It would, however, seem that the fields might be weakened with advantage at starting, or the motors and switching arrangements might be so designed that a larger draw-bar pull could be maintained until the train got up to speed; the advantage would obviously be a gain in time at starting. This question is, however, more fully considered later on. The time taken to com-



Effect of varying starting current.
Elephant Station to Kennington Station.

FIG. 5.

plete the running period of the trip, and of course the speed during this period, seems to be unaffected by the value of the starting current. The time taken between stations or the time between switching on and off current is not much affected by the starting current, except for low values. With stations about 860 yards apart, the same energy is required for starting and running periods, or the energy required to bring the train up to its proper speed is 50 per cent. of the total ; this again is affected by the gradients, but for a level road it seems to be fairly correct. Thus between the Borough and Elephant and Castle stations there is a downward gradient—here the number of kilowatt-hours at starting was ·339, the running kilowatt-hours being ·478 ; and between the Elephant and Castle and Kennington there is an upward gradient at starting, followed by a downward gradient—the number of kilowatt-hours to start the train was ·547, and ·474 completed the trip. These values are for a starting current of 140 amperes in each case. The total energy for a trip between stations seems to be independent of the starting current, except for values below 120 amperes, and here the time taken for a trip is longer.

An inspection of the last two lines of the table shows that up to a speed of 11 miles per hour the time taken to attain that speed decreases with the increased value of starting current. This is perhaps more closely shown in the speed of 9 miles per hour.

Motor and Locomotive Tests.

The efficiency of an electrical locomotive when hauling a load, is different from that of a motor tested on a foundation in the usual way. It is customary in giving the results of a test on a street-car motor to speak of the horizontal effort at the tread of the wheel of a certain diameter, and to call the result so obtained the draw-bar pull. Experience shows that the actual draw-bar pull at starting, as measured by a dynamometer, is only about 73 per cent. of the actual tractive effort at the tread of the wheel of the motor. This difference was shown forcibly in attempting to obtain the efficiency of No. 15 locomotive under actual running conditions. Assuming that the tractive effort at the tread of the wheel (as calculated from the tests of the motor

with a Prony brake) was the same as the draw-bar pull, the locomotive hauling a load came out with an efficiency of over 100 per cent. In order to make this clear, it may be well to state how the efficiency was worked out. The voltage current and speed readings were taken at five-second intervals. From the volts and amperes the electrical horse-power supplied to the locomotive was obtained; the mechanical horse-power given out at the draw-bar, *i.e.*, the work done in hauling a load, was arrived at by multiplying the tractive force at the tread of the wheel for each value of current by the space travelled in feet per second—this gives foot-pounds per second and consequently horse-power. In order to find where the error lay, several tests were made under various conditions. With the aid of a Prony brake the motor was first tested lying on its back, with the weight of the armature and wheels on the bearings as in the case of an ordinary stationary motor. Currents varying from 15 to 170 amperes were passed through the motor, and the voltage, speed, torque, and current were observed; from the torque the tractive force in pounds at the tread of the 27-in. diameter wheel was calculated; this tractive force and current being plotted gave a straight line (*A*, Fig. 6), 15 amperes being the amount taken to start and run the motor light. The speed and voltage need not be taken into consideration for our present purpose. The actual draw-bar pull of the locomotive on the level rails was next obtained by coupling, first a dynamometer, and afterwards a spring balance, between the draw-bar and a fixed point, this giving a condition of affairs similar to a train starting from dead rest. It may be mentioned, however, that the current necessary to just start the light locomotive was 50 amperes (the result of about 30 trials), the dynamometer being of course uncoupled for this test. The least current with which the locomotive would exert a pull that could be measured with tolerable accuracy was 60 amperes, the pull being 300 lbs. The current was then increased to 200 amperes by steps of 10, and the pull in pounds was noted. This static draw-bar pull was measured under several conditions, viz., backwards and forwards, on clean rails, rusty rails, and rails that were fairly worn, with two different hydraulic dynamometers and with a spring balance and lever, the results in every case agreeing within the errors of observation. The curve plotted (Fig. 6) is the result of

quite two hundred observations. It is worth recording, however, that better results were obtained with the spring balance for the lower readings on account of the friction between the piston-rod and gland of the dynamometer. Comparing the tractive force at the draw-bar (Fig. 6) with the tractive force at the tread of the wheel in the Prony brake test, we can see there is a difference between these two curves of 620 lbs. at 60 amperes and 800 lbs. at 150 amperes; taking the mean, there is thus a loss of over

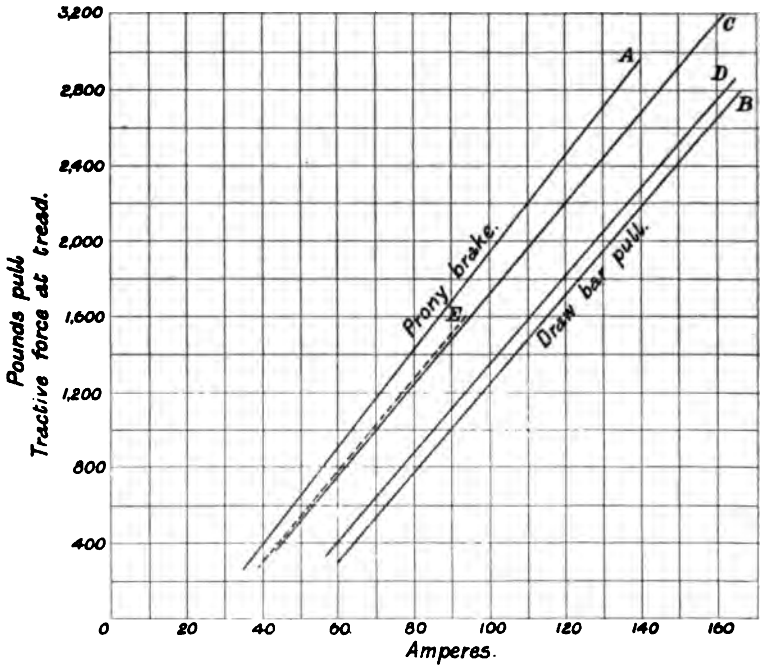


FIG. 6.—Locomotive No. 15.

700 lbs. to be accounted for. It is well known from steam railway working that the friction of bearings at starting is many times that obtained in running, due no doubt to the fact that the film of oil that exists between the surfaces of the axle and brass when the axle is at rest is squeezed out, and the surfaces are in metallic contact. With a view to determine whether this loss of 51 lbs. per ton was all due to the above cause, further experiments were made as follows: —*One* of the motors was tested lying on its back, as in the

Prony brake test, but the armature was only allowed to make a small fraction of a revolution—in fact it only turned an amount allowed by the elongation of the spring of the balance (Fig. 7); in this manner the static pull at the tread of the wheel was measured, with currents varying from 30 to 180 amperes, the current increasing by steps of 10. The result of several such trials, some beginning with high

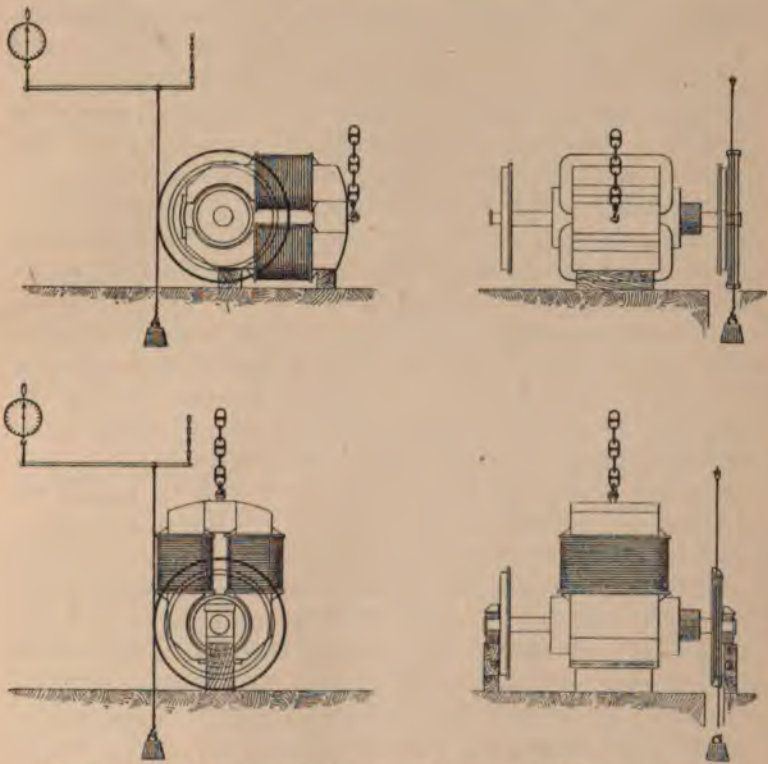


FIG. 7.—Tests of Motor of Locomotive No. 15, under different conditions.

currents and coming down, are given by curve *C* (Fig 6). There is a difference of 120 lbs. between curves *A* and *C* at 50 amperes, and of 280 lbs. at 150 amperes. It would seem as though the increased loss of 160 lbs. at 150 amperes was caused by the magnetic pull of the field magnets on the armature (due perhaps to the armature being slightly out of balance magnetically with respect to the field), tending to further squeeze out the oil for higher values of current, this

squeezing out of oil being opposed to a certain extent by the revolving armature in the Prony brake test keeping the bearing properly lubricated. In order to find the loss due to the weight of the magnets a test similar to the last was made, but in this case the motor was allowed to work on its outside bearings or axle-boxes, the motor being kept in a vertical position as shown by sketch (Fig. 7). In this manner the total weight of the armature and magnets was borne by the outside bearings, and the additional loss due to the weight of the magnets was obtained, the results plotted giving curve *D* (Fig. 6), which shows 360 lbs. less than *C* for all values of current, indicating that the loss due to the additional weight of the magnets is constant. If we now assume that the friction in the motor-bearing is proportional to load, the additional loss due to the weight of the locomotive cab switches, resistances, &c., is 350 lbs. The total loss then between the static tractive effort with the motor lying on its back and the draw-bar pull should be 710 lbs. This loss as actually measured and shown by the vertical distance between the curve *C*, and the draw-bar pull is about 700 lbs. Having to some extent accounted for the loss between the tractive force at the tread of the motor wheel and the draw-bar pull, the question then arose, What is the draw-bar pull when the locomotive is running on the rails ? Taking the value of the draw-bar pull from the curve *B* for 60 amperes, the average value of current during a run between stations showed that this pull, 300 lbs., was not sufficient to overcome gravity alone, not to speak of other resistances. Clearly then, the pull exerted in running must be considerably greater than that exerted at starting for the same value of current. An attempt was made to analyse this by further tests in the shops, but without success.

Tests were made in a similar way with No. 12 and No. 17 locomotives, and the results are plotted on Figs. 8 and 9.

The curve *A*, Fig. 8, is the Prony brake test of a motor from locomotive No. 12, *B* is the static draw-bar pull ; it will be noticed that the loss in this locomotive is not so great as that in the case of No. 15, the average loss being about 550 ; while in No. 17 the loss between the Prony brake test and the static draw-bar pull is 640 lbs. (see Fig. 6.) It seems at first sight that this loss should be proportional to the total weight of the locomotive, or to something representing

the locomotive weight into the total tractive force of the motors. It is, however, directly proportional to the total

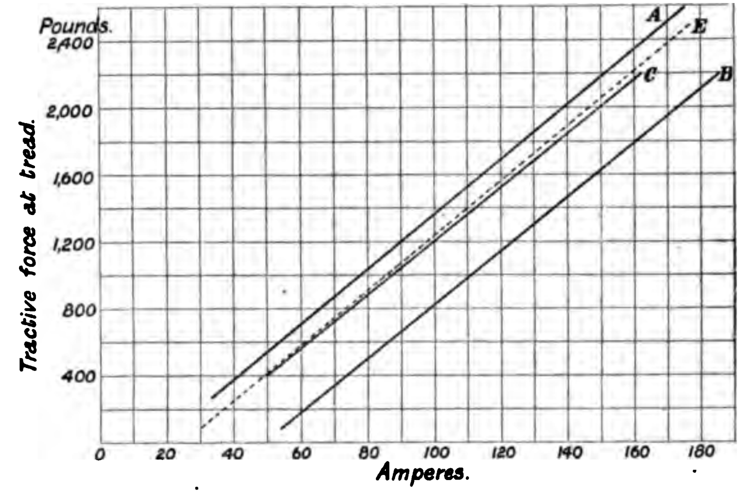


FIG. 8.—Locomotive No. 12.

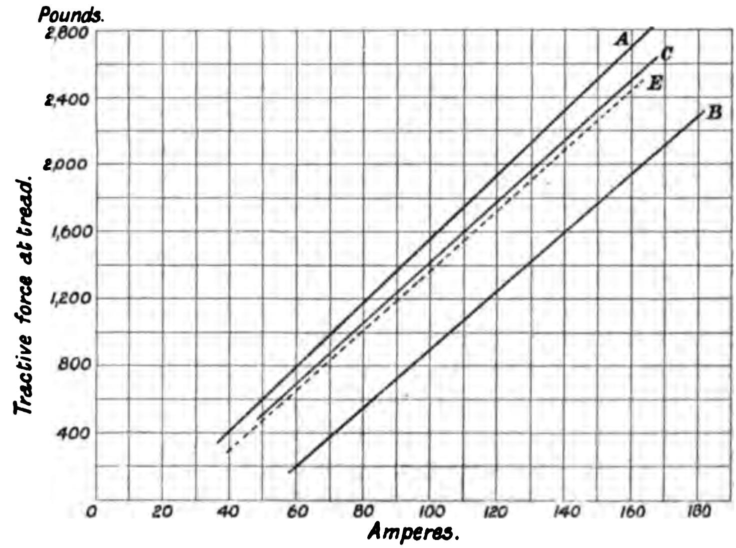
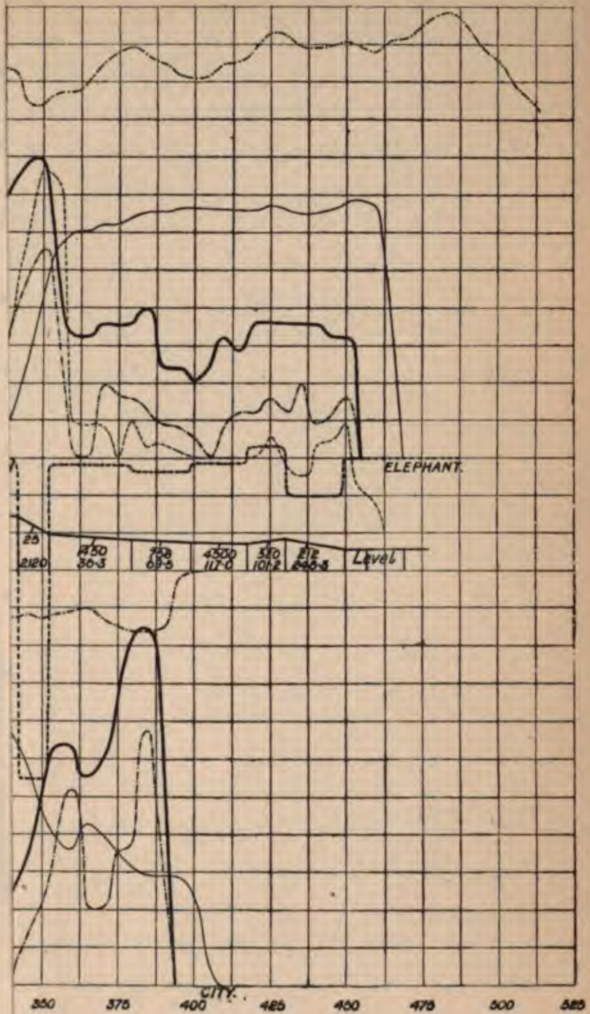


FIG. 9.—Locomotive No. 17

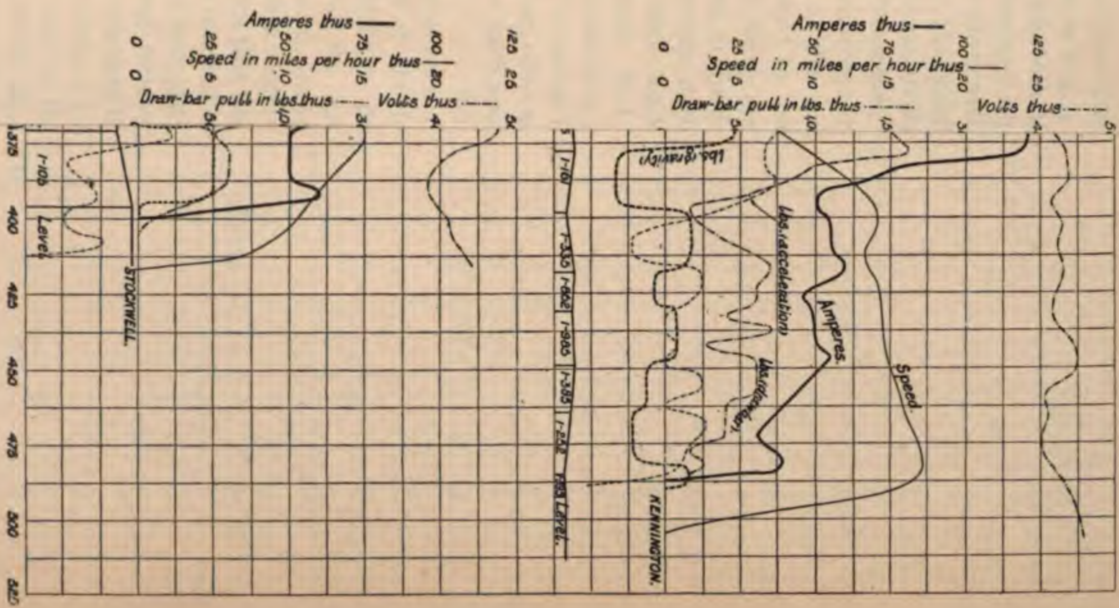
tractive effort alone. The result is easily expressed, by dividing the tractive force obtained, by the Prony brake test, by the loss at, say, 100 amperes, as follows :—

No. 12. $1360 \div 550 = 2·47$.
No. 17. $1540 \div 640 = 2·40$.
No. 15. $1920 \div 720 = 2·65$.

The motors from these two locomotives were not tested with the weight of the motor on the outside bearings as in the case of No. 15 locomotive. As from experiments made in the shop it was not found possible successfully to analyse the locomotive losses when running, the dynamometer, Fig. 3, was used to measure the pull exerted by the locomotive under actual working conditions; this dynamometer, as can be seen from the figure, was inserted in the train draw-bar in the position usually occupied by the first spring. A trip was then made between Stockwell and the City, readings of the current, voltage, speed, and draw-bar pull being taken at five-second intervals. Results of these observations are plotted in Fig. 10. It will be noticed that the current at starting in this case is not over 100 amperes, the current being kept low to avoid any jerk at starting, as it was not known at this stage what might happen to the diaphragm of the dynamometer. Although a value is given for the draw-bar pull at the first reading, the gauge on the dynamometer did not show that any pull was being exerted for about one to two seconds after the current was switched on, showing that the locomotive takes an appreciable time to move. However, when the gauge commenced to indicate, it gave varying values, the pointer sometimes going all over the scale with the current absolutely steady; by carefully watching the pointer during the interval it was possible to form a fairly accurate idea of the average pressure during each period. This jerking was most noticeable on downward gradients, as the pull of the locomotive seemed to be transmitted to the train by a series of jerks, the train and locomotive buffers alternately touching and separating. Upon an upward gradient the pull seemed to be more uniform, the buffers being nearly always apart. An inspection of Fig. 10 shows that the draw-bar pull curve follows the current pretty closely in shape, from which one might infer, that the dynamometer gave fairly accurate indications of what was going on between the locomotive and train. Let us leave for the moment the analysis of these curves. This dynamometer was placed in an



ELEPHANT.
25
2120
1450
30·3
4500
117·0
101·2
Level
CITY.
350
375
400
425
450
475
500
525


Amperes thus
Speed in miles per hour thus
Draw-bar pull in lbs. thus
Volts thus
Speed.
Amperes.
lbs. (acceleration)
lbs. (gravity)
lbs. (drawbar)
KENNINGTON.
1-161
1-335
1-662
1-985
1-383
1-252
Level.
STOCKWELL.
1-105
Level
375
400
425
450
475
500
525

[illegible]

awkward position for the observer; in addition, it had the disadvantage that the dynamometer was on the train and not on the locomotive. It was then decided to make a dynamometer for the draw-bar of the locomotive, and that shown and described in Fig. 4 was designed and constructed. The results obtained with this dynamometer agree very closely with those obtained with the "train" dynamometer. Two trips between Stockwell and the City were made with this dynamometer, the starting current being limited to 100 and 120 amperes respectively; then two trips were made between the City and Stockwell, and the current again was limited to 100 and 120 amperes as before. Readings taken at five-second intervals, as in the case of the first draw-bar pull test, are given plotted in curves, Figs. 11 and 12. At first sight it seems as if these curves gave the value of the amperes in pounds pull at the draw-bar, and were calibration curves of the locomotive itself; this would be quite true if the experiments were made on a perfectly level line, and at a constant speed. But as the gradients change very often between some stations, and the speed varied considerably, it was necessary to allow for the tractive force due to gravity and acceleration, otherwise the pounds pull per ampere would seem to vary at each point on the line. From the speed curve shown in ——— the acceleration in feet per second was calculated at each five-second interval; this, multiplied by the mass M of the locomotive, gave the force in pounds due to acceleration, positive or negative, shown by the .... Next the gradient was plotted to a time scale in the following manner: The average velocity during each five-second interval was obtained from the speed curve, and from this the space in feet travelled by the locomotive in the interval was calculated, the continuous sum of the space in feet gave the position of the locomotive at each five-second interval, and from the section of the line it was known whether the locomotive and train were on an upward or downward gradient, or on the level or on a curve. The ——— line at the bottom of each curve gives the gradient, and from this the tractive force in pounds was calculated and plotted. During the running period it was found unnecessary to make any allowance for the transition from one gradient to another, the speed being fairly high compared with the length of train—in fact in several cases it

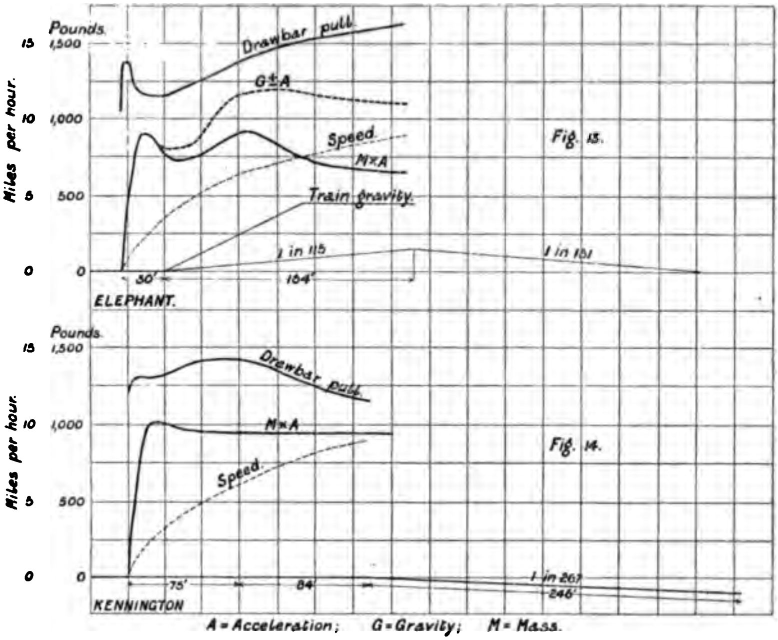
was worked out fully, but the difference was so small that it made no perceptible alteration in the area of the gravity curve. During the starting period, however, it was necessary to allow for, say, one carriage on the level with the locomotive and two carriages on a gradient.

From the analysis of these curves the losses in the locomotive, and its draw-bar pull at starting and running were obtained, as well as the tractive force per ton of train hauled on the level, including air resistance.

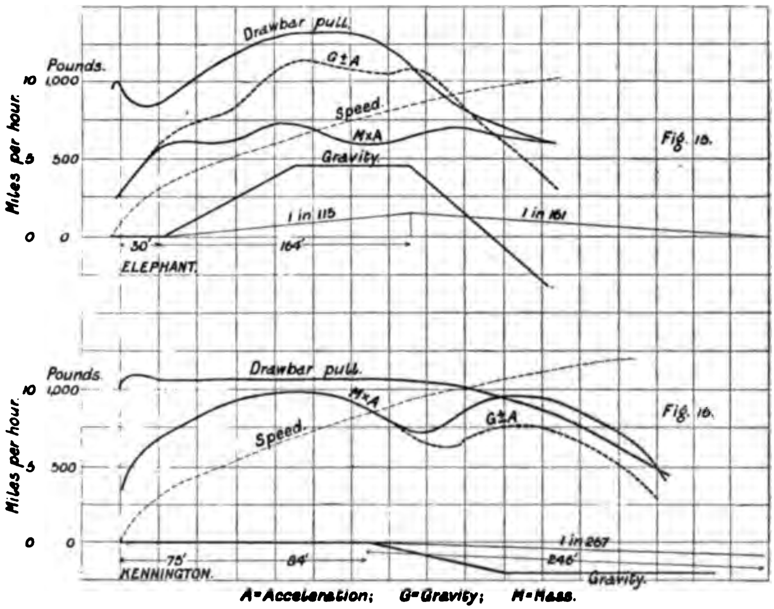
The losses in the locomotive and its draw-bar pull during the starting and running periods will first be considered.

Locomotive Losses and Draw-bar Pull at Starting.

In order to arrive at the value of the draw-bar pull exerted by the locomotive on the level at starting, the starting periods at Kennington with 100 and 120 amperes, and Elephant and Castle with the same currents, will be considered; these stations are chosen to avoid the errors (or uncertainties) introduced in the dynamometer readings by a quick downward gradient. In each of the four cases the values of the dynamometer reading, the tractive force due to gravity, and the tractive force due to acceleration were taken from the plotted curves, at five-second intervals, and reduced to the level at a constant speed. Thus at Kennington (curve Fig. 11) at five seconds after starting the draw-bar pull as shown by the dynamometer was 1,075 lbs, the tractive force due to acceleration plus or minus gravity was 201 lbs., and as this force did not appear at the draw-bar, but was given out by the locomotive at the tread of the wheel, it must be added to the draw-bar pull, consequently the total tractive effort which should appear at the draw-bar at a constant speed on the level would be 1,276 lbs. for a current of 105 amperes. In the same manner at the tenth second interval the draw-bar pull was 1,075 lbs., the tractive force due to acceleration plus or minus gravity was 345 lbs., giving 1,420 lbs. at the draw-bar at a constant speed on the level for a current of 100 amperes; in the same way the tractive efforts at the draw-bar at the fifteenth and twentieth second intervals were 1,467 lbs., and 1,596 lbs. for a current of 100 amperes. Table II. is obtained by treating the other cases in a similar manner and tabulating the results. To



FIGS. 13, 14.



FIGS. 15, 16.

facilitate the analyses of these curves during the starting period, both as regards locomotive losses and tractive effort per ton of train at starting, the curves, Figs. 11 and 12 (from the Kennington and from the Oval curves) are plotted to a distance scale in Figs. 13, 14, 15, and 16. The value of gravity for half a coach on the level and half on the gradient could then easily be allowed for. The curves are self-

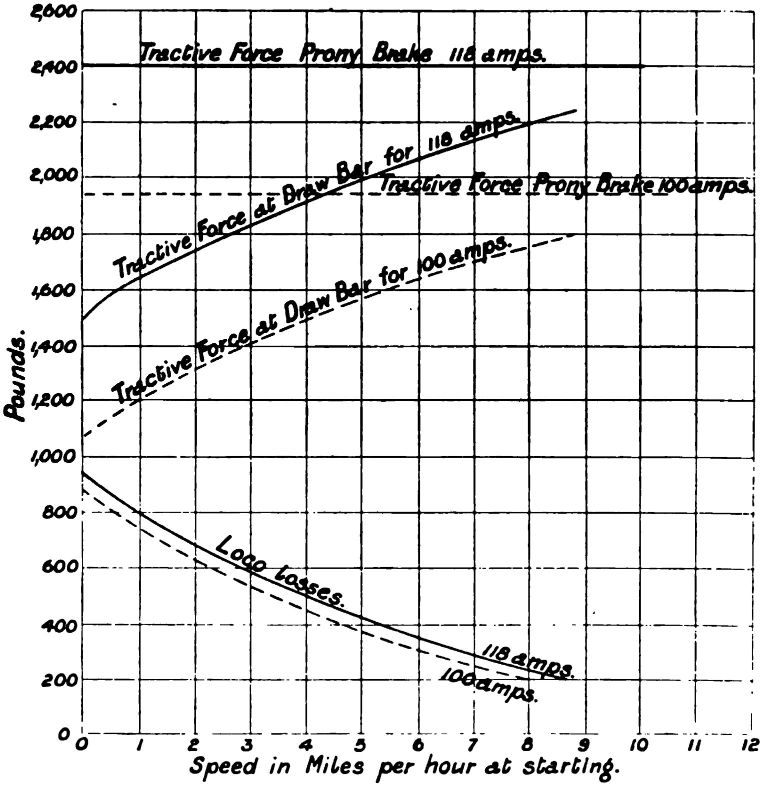
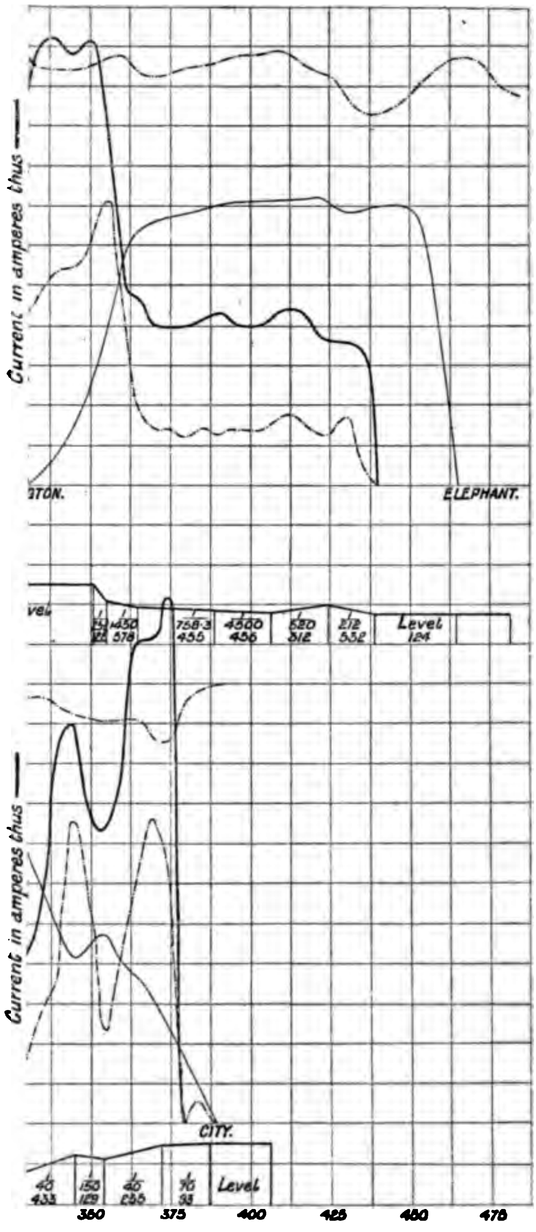
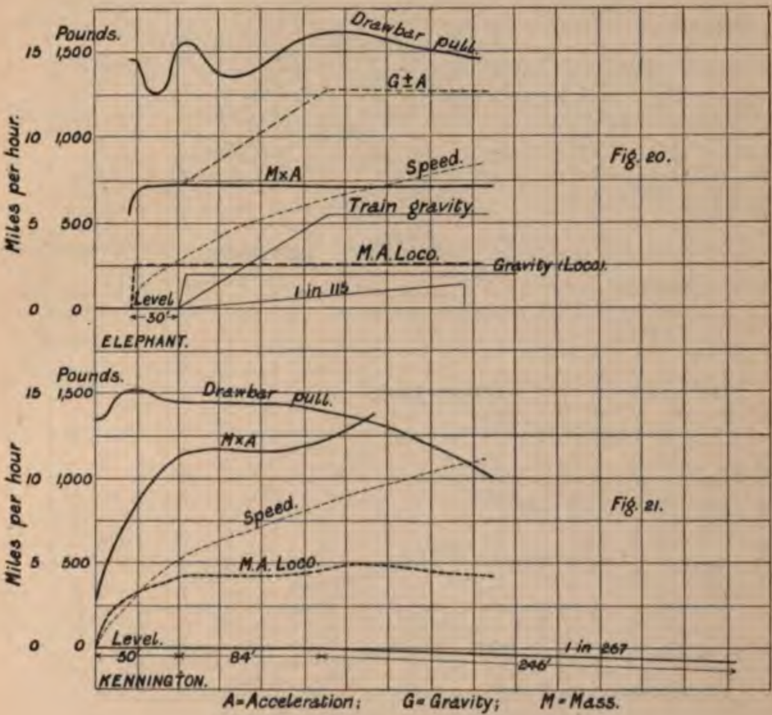


FIG. 17.—Locomotive No. 15, Tractive Force and Losses at Starting.

explanatory, and are again referred to in the section dealing with tractive resistance per ton of train. Plotting the values of speed and tractive force at the draw-bar at constant speeds on the level, we get two curves showing the tractive effort for 100 and 118 amperes at speeds of from 0 to 9 miles per hour. The value at 0 is taken from the static draw-bar pull. From these two curves it would seem as if


Current in amperes thus ——
TON.
ELEPHANT.
vel
1/1450
578
1/758·3
455
1/4500
456
1/520
512
1/212
532
Level
124
Current in amperes thus ——
CITY.
1/40
433
1/150
129
1/40
255
1/110
93
Level
350
375
400
425
450
475

the effective tractive force increased, or locomotive losses decreased, with the speed until the latter reached about 9 miles per hour. If two lines are now drawn representing the tractive force at the tread of the wheel, as found from the Prony brake experiment, for 118 and 100 amperes respectively, the vertical distances between the former and latter curves give the locomotive losses at the various speeds up to 9 miles per hour. Plotting these values we

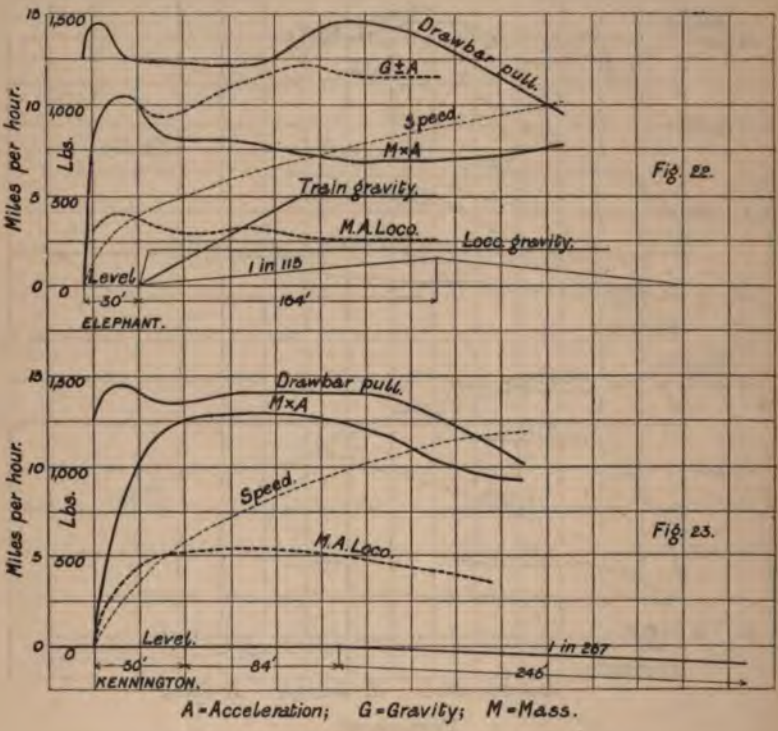


FIGS. 20, 21.

have two new curves giving these losses. These curves go to prove that the losses as shown by the difference between the Prony brake test and draw-bar pull at starting, already referred to, are proportional to the total tractive effort given out by the motor, and although the loss decreased with the increase of speed, still there is clearly a greater loss at 118 amperes than at 100 amperes.

Observations were next taken with No. 12 locomotive under ordinary everyday working conditions. The current

volts, speed and draw-bar pull were taken at five-second intervals as before. A complete trip from Stockwell to the City during heavy morning traffic is plotted in Fig. 18, and Fig. 19 gives the corresponding curves from the City to Stockwell for heavy evening traffic. In each case the number of passengers carried between stations was observed, and thus the actual weight of the train was found to within about 100 lbs. Three complete trips each



FIGS. 22, 23.

way were taken, and the results given in the tables are obtained from the six trips, although only two are quoted. The remaining four are simply a repetition of Figs. 18 and 19.

The locomotive losses and draw-bar pull at starting are treated in the same manner as No. 15 locomotive, Figs. 20, 21, 22, and 23 are the starting periods plotted to a distance basis for two trips from the Elephant and Kennington.

The values set down in Table III. are taken from these

four curves, and the curves, Fig. 24, give tractive force and losses at starting for No. 12 locomotive plotted from values given in Table III.

In this case the starting current in each case was kept as nearly as possible steady at 140 amperes. These curves are again used in obtaining the tractive resistance per ton of train at starting, as in the case of No. 15 locomotive.

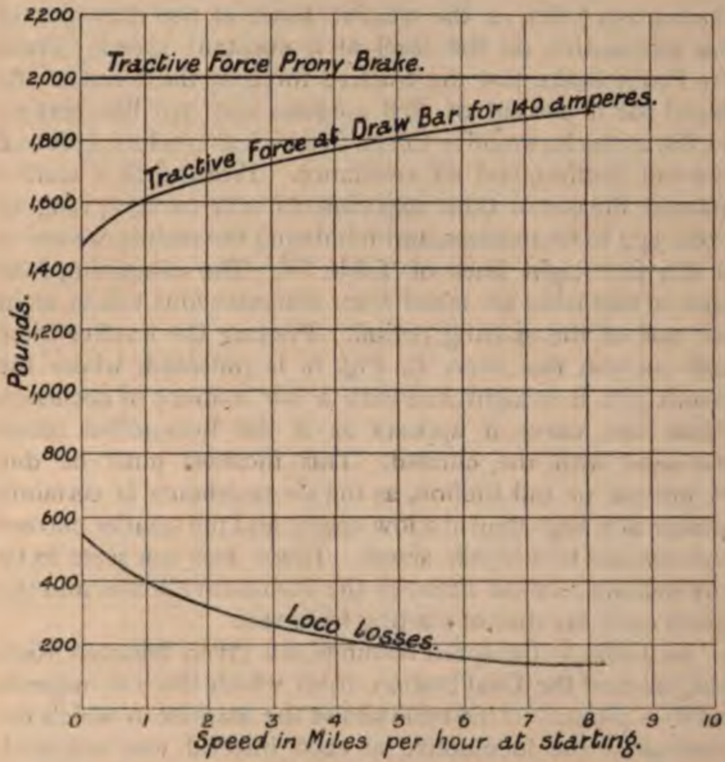


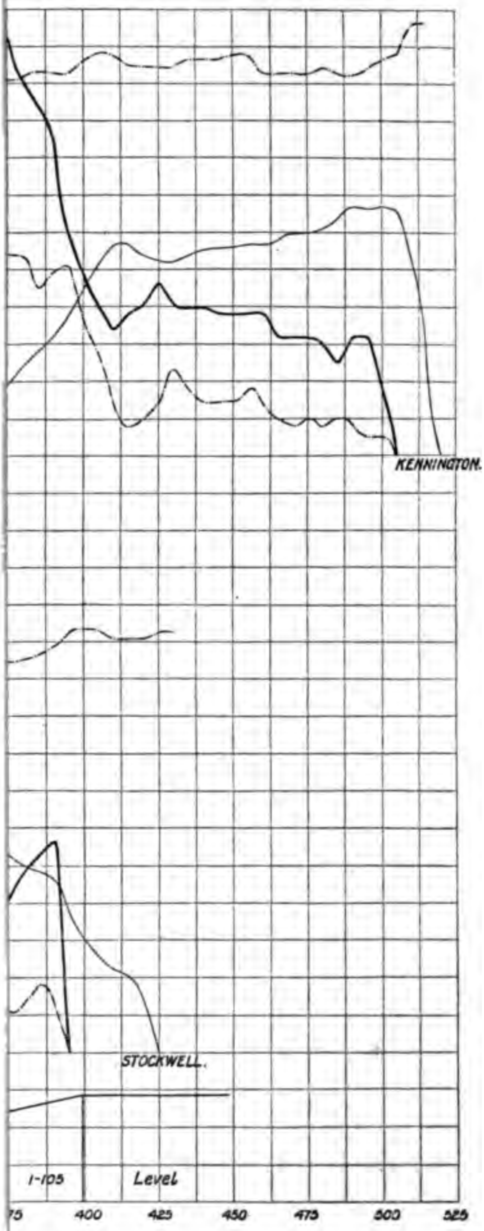
FIG. 24.—Locomotive No. 12, Tractive Force and Losses at Starting.

LOCOMOTIVE LOSSES AND DRAW-BAR PULL WHEN RUNNING.

To arrive at the draw-bar pull on the level exerted by the locomotive for speeds of 9 miles per hour and upwards, for various currents, the experimental runs between Stockwell and the Elephant, and the Elephant and Stockwell, were analysed as before explained, but in this

case the area of the speed, current, draw-bar pull, and the tractive force due to gravity and acceleration curves, Figs. 10, 11, and 12, were measured, and from these the average values were obtained as follows. Between Stockwell and the Oval the average draw-bar pull was 268 lbs. for an average current of 36·8 amperes at an average speed of 18·4 miles per hour (during the running period). The average tractive force due to acceleration, plus or minus gravity, was 8·36 lbs., leaving 259·7 lbs. as the tractive force at the draw-bar of the locomotive on the level at a constant speed. From the Prony brake test the tractive force at the tread of the wheel for a current of 36·8 amperes was 340 lbs., leaving 80 lbs. as the locomotive losses, in which are included journal and rail friction, and air resistance. Treating in a similar manner the seven other experiments with currents ranging from 34·2 to 60 amperes, and tabulating the results, we arrive at the first eight lines of Table IV. The remaining four lines of this table are taken from instantaneous values, as in the case of the starting period. Plotting the tractive force and current, the curve *E*, Fig. 6, is obtained, where the results give a straight line with a fair amount of accuracy. From this curve it appears as if the locomotive losses increased with the current. This increase must be due to journal or rail friction, as the air resistance is certainly greater at a high than at a low speed, and the smaller current corresponds to a higher speed. There does not seem to be any definite relation between the locomotive losses and the speed as in the case of starting from rest.

In Table V. the speed readings are given between Kennington and the Oval Station, from which the 120 amperes curve is plotted. This table shows the manner in which the position of the locomotive at each interval was obtained, and the tractive force due to acceleration. The column showing the total distance in feet gave an excellent check on the speed readings; the actual distance between the starting and stopping points being 2,824 feet, and the calculated distance travelled being 2,804 feet, there was an error of less than 1 per cent. In some earlier experiments a speed counter was attached to one of the locomotive wheels, giving the actual number of revolutions made; this counter was observed at each five-second interval, and from this the position of the locomotive in the section determined. It



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was, however, found that equally good results could be obtained, as shown in Table V., and this method was therefore discontinued.

The draw-bar pull at constant speed on the level for No. 12 locomotive was obtained from an analysis of the tests already explained between Stockwell and the City, and the City and Stockwell, as in the case of No. 15 locomotive. The curves of tractive effort due to gravity and acceleration are not plotted on Figs. 18 and 19, as in the case of No. 15, it being found that it was easier to calculate the values direct. A saving in time was therefore effected in working out the results, which will be appreciated by any one on whom falls the working out of a considerable number of observations. This draw-bar pull on the rails is shown in Fig. 8, curve *E*, and the losses in the locomotive can be found by scaling the vertical distance between this curve and the Prony brake test curve *A*. The results from which *E* is plotted are set down in Table VI.

The draw-bar pull for No. 17 locomotive is worked out exactly as for No. 12, from the trips between the City to Stockwell, one of which is given plotted, Fig. 25. The nett results worked out are given in Table VII., from which the curve *E*, Fig. 9, is plotted. The locomotive losses on the rails are represented by the vertical distance scaled between curve *A* and curve *E* as before.

Some objection might be raised to the short duration of some of these tests unless the column giving the time is a little more fully explained. The test proper occupied the run from station to station, but as the current varied considerably, it was necessary to pick out pieces of the current curve that gave the average value of current required; the number of seconds in the column refers to length of the curve on a time basis.

TRACTIVE RESISTANCE PER TON OF TRAIN.

Treating in a very similar manner the curves Figs. 10, 11, and 12, the tractive resistance per ton of train, including air resistance, was arrived at. In arriving at the tractive force per ton at starting, it was necessary to consider the length of the train in passing from one gradient to another, and this was allowed for in the following manner: The

speed, draw-bar pull and tractive force due to acceleration were plotted to a distance basis from the calculated distance travelled, as in Table V.; the gradient was then plotted and the position of each coach on the gradient (up or down) determined at each interval; the tractive force due to gravity was then obtained and plotted, and so on until a speed of about 10 miles an hour was reached. This is seen more easily by reference to Figs. 13, 14, 15, and 16. Here it may not be out of place to consider in detail what tàkes place in a starting period, say from Elephant and Castle, Fig. 13. At the moment of starting the front end of the train was on the level, and 30 feet from an up gradient of 1 in 115. After travelling about 60 feet, one coach was completely on the incline; in this position the effect of gravity was 152 lbs.; when another 30 feet had been passed over, two coaches were on the incline, and the value of gravity was 304 lbs. When the whole train was on the incline the gravity curve ceased to rise and ran parallel to the base until the downward gradient of 1 in 161 was reached by the first coach. The down gradient was analysed in a similar manner, and from the result the corresponding part of the gravity curve was plotted. From the curves, Figs. 13, 14, 15, and 16, the Table VIII. was compiled, giving the total tractive force on the level for the train, at speeds varying from 0 to 10 miles per hour. This table was obtained by scaling the vertical distance between the observed dynamometer draw-bar pull and the curve giving the tractive force due to acceleration, plus or minus gravity. These values are given in columns 2, 3, 4, and 5, and represent the total pull in pounds on the draw-bar of the train reduced to the level at a constant speed; column 7 gives the tractive force in pounds per ton on the level. The figures in column 7 are plotted on Fig. 29, and give the first part of the curve shown there. The points of this curve above 9 miles per hour are the average values obtained from the running periods between Stockwell and the Elephant and Castle up and down roads, and are arrived at from the dynamometer observation curves, Figs. 10, 11, and 12, in a manner similar to that explained above. It may perhaps be well to explain this a little more in detail. Between Kennington and the Oval stations the average draw-bar pull during the running period was 500 lbs., the tractive force due to acceleration, plus or minus gravity, was 213·5 lbs.,

leaving 287 lbs., or 11·75 lbs. per ton, as the amount necessary to overcome tractive and air resistance, for an average speed of 13·35 miles per hour. The duration of this test was 105 seconds. In a similar manner the other values set down in Table IX. were obtained.

In order to check in some manner the accuracy of these figures, three more experiments were made between Stockwell and the Oval. Here, shortly after leaving Stockwell,

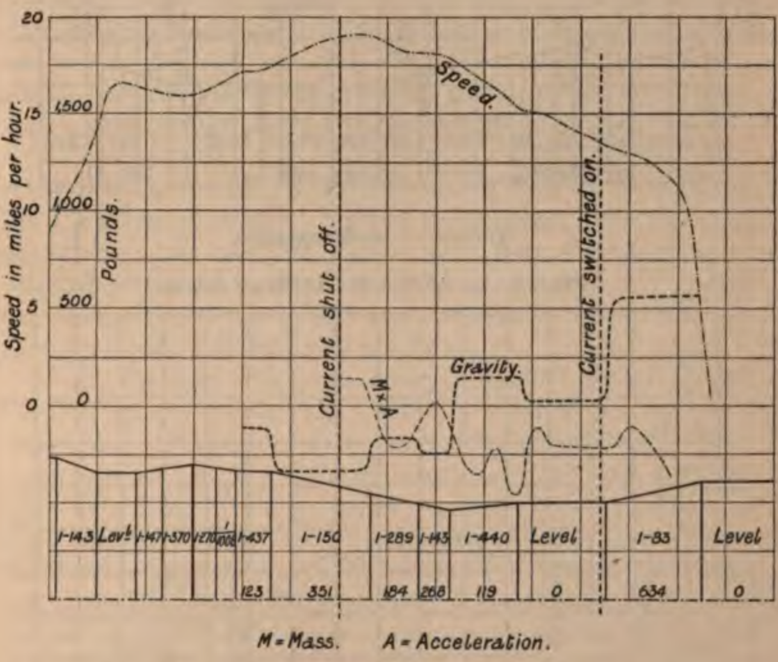
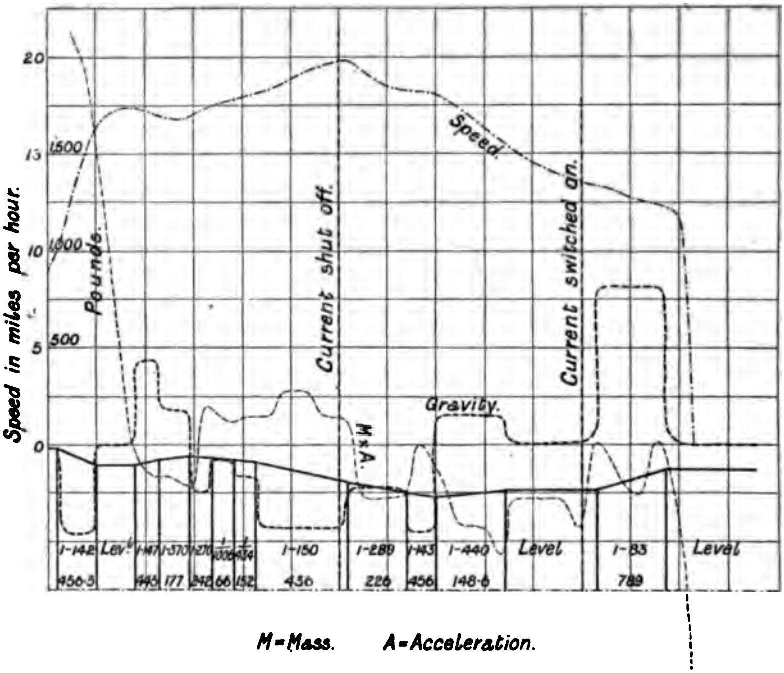


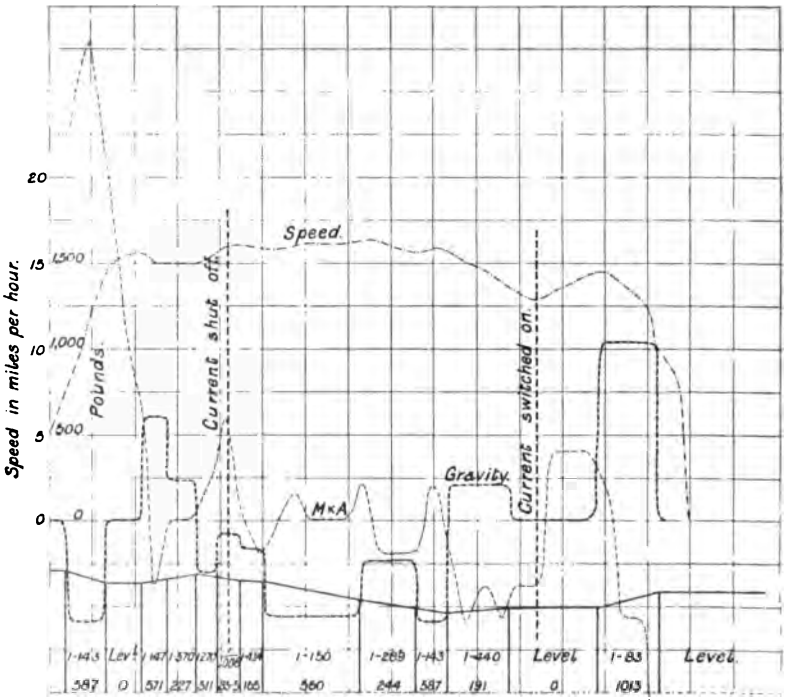
FIG. 26.—Locomotive No. 15, Train No. 7.

there is a downward gradient, which continues almost until the Oval is reached. The speed was recorded at five-second intervals as before, and when the locomotive reached a predetermined point the current was shut off, and the locomotive and train were allowed to run by gravity and their own kinetic energy until the speed fell to about 13 miles per hour; at this point the train was about to ascend a slight up gradient, and the current was switched on again. These tests are plotted, Figs. 26, 27, and 28, the first with a three-coach train and the



M=Mass. A=Acceleration.

FIG. 27.—Locomotive No. 15. Train No. 14.



M=Mass. A= Acceleration.

FIG. 28.—Locomotive No. 15, Train No. 1

second with a two-coach train; the speed at shutting off the current in the former case was 18·5 miles an hour, and it dropped to 13·5 before the current was switched on again. This speed curve was analysed as before, and the tractive force due to acceleration and gravity was found to be 406·3 lbs.; this divided by the weight of the train and locomotive, which was 37 tons, gave a tractive force of 10·95 lbs. per ton on the level at an average speed of 15·86 miles per hour.

The tractive force per ton in the case of the two-coach train, Fig. 27, came out rather higher, being 12·1 lbs. per ton for an average speed of 16·5 miles per hour. This slight increase may be due to both trains having the same area of cross-section exposed to the air, while the total weight in the second case is less than in the first. Again, the skin resistance should be less in the second case on account of the train being shorter. In the third experiment, Fig. 28, the current was shut off earlier, and thus the speed was maintained almost constant until it was switched on again; the resistance per ton in this case came out at 11 lbs. for an average speed of 15·86 miles per hour.

From the tests of No. 12 locomotive further points on the curve (Fig. 29) connecting tractive resistance per ton and speed were obtained, in a manner already explained. The results are set down in Tables X. and XI.; the former table gives the values up to eight miles per hour, these being taken from the curves (Figs. 20, 21, 22 and 23) plotted to a distance basis as in the case of No. 15 locomotive. Table XI. gives the values up to 19·5 miles per hour; in this table the actual weight of the train, which varies from 24·37 tons to 28·9 tons, is given in each case.

Effect of Curves.

To arrive at the additional resistance offered by the curves on the line, the observations from which the tractive force per ton was obtained were further analysed in the following manner. The position of the curve in the section being known, the exact time when the train was passing over the curve was found as already explained; for the gradient, the draw-bar pull, tractive effort due to gravity and acceleration were taken from the respective plottings for the

particular section, and in this manner the total tractive effort per ton on the level at a constant speed was obtained; deducting from this the tractive resistance per ton already found at that particular speed, the result gave the additional resistance due to the curve. Although there are several sharp curves their length is small, and it was difficult to get accurate results, as in some cases the length of curve was not greater than that of the locomotive and train. The most consistent results were obtained on the curves between the Borough and Elephant stations, and between the Elephant and Kennington stations. The first curve has a radius of

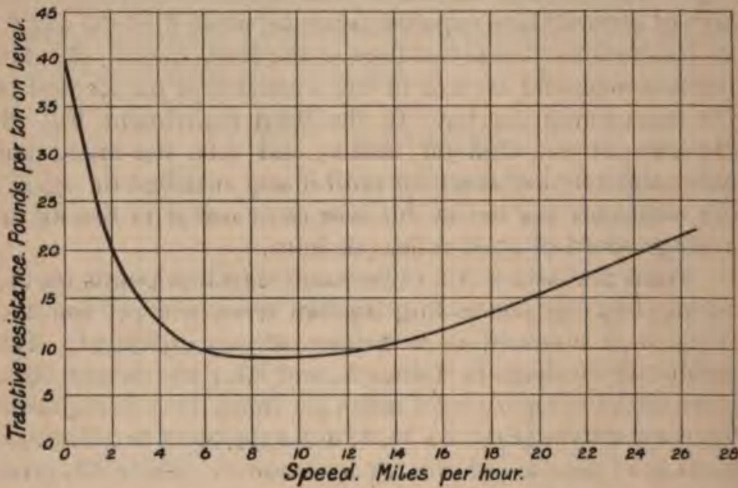


FIG. 29.—Curve showing Tractive Resistance Per Ton at Various Speeds.

390 feet, and the average of three sets of observations gave 27·9 lbs. per ton at 16·5 miles per hour, the tractive force per ton on the straight and level road being 12·75, leaving 15·15 lbs. per ton due to the curve alone. The next curve has 540 feet radius, and the average of six sets of observations gave 22·6 lbs. per ton, at 13·5 miles per hour, that on the level straight road being 11·3 lbs. leaving 11·3 lbs. due to the curve.

It is perhaps worth mentioning that there is a guard-rail on each of these curves, which, although in good condition and very smooth, no doubt adds to the resistance.

COMPARATIVE RESULTS OF DIFFERENT LOCOMOTIVES.

In order to arrive at a comparison between the performance of the three locomotives under actual working conditions, the results of two trips for each locomotive between Stockwell and Borough stations, up and down, are given in Table XII. The various runs are divided into starting and running periods as before; the average current and time taken to get up speed are given, as also the current and time for running, and the average current taken for the section, averaged from the switching on until the current is shut off. It will be seen from this table that, for each locomotive, there is a difference between the time taken from starting until the locomotive comes to rest in the next station and the actual time the locomotive is taking current from the line. In every case, No. 12 locomotive takes current for the shortest period, although the actual time for running the section differs only slightly from the time taken by No. 15 and No. 17 locomotives. This is due to No. 12 running at a higher average speed than the other two locomotives. A further inspection of the table shows that No. 12 takes a greater average starting current and requires a longer time to get up to full speed than do Nos. 15 and 17. No. 15 locomotive takes the lowest average starting current, but requires a slightly longer time to attain full speed than does No. 17; the latter, however, draws a larger average starting current from the line. These few facts seem to indicate that the tractive effort per ampere at starting is too low in No. 12 and too high in No. 15, No. 17 locomotive striking the happy mean.

The high tractive effort per ampere is undoubtedly the right thing, but as the locomotive gains speed the current falls off very quickly, until the tractive effort at the tread of the wheels is less than that given by Nos. 12 and 17. This can to a certain extent be overcome by shunting the field when a speed of, say, 10 miles per hour is attained; but the result is not economical, as for the higher current then flowing the tractive effort per ampere is less, on account of the weaker field. A few experiments with this particular locomotive prove this to be so, the energy for a section being increased between 50 and 75 per cent.

Further reference to the table shows that although No.

12 locomotive takes a larger current from the line during the running period, it takes this current for a shorter interval of time. There is not much difference between Nos. 15 and 17 either in current or time for the running period, the balance, if any, being in favour of No. 17.

As to the energy consumed, taking No. 15 as a standard, No. 17 locomotive shows a decrease in six of the eight sections, varying from 1·5 to 9 per cent. Turning to No. 12, it shows an increase in every section varying from 6 to 26 per cent. This is a very large increase, and points to the locomotive not being (as the motors are now wound) so well adapted to its work. This large increase in current consumption does not indicate inefficient motors, as is shown by Table XIII. Subsequent alterations in the motor winding and switch gear, followed by experiments, have shown that it is merely a question of arranging the right number of pounds tractive effort at the tread of the wheel per ampere.

It is worth notice, before leaving this table, that No. 12 locomotive is more economical on the long than on short sections; and on the Oval-Stockwell section, where there is an up gradient the whole way, it is running under its most economical conditions. This almost leads one to predict, that were the sections longer, or, in other words, if the starting period bore a smaller relation to the running period with regard to time, No. 12 locomotive would be the most economical of the three. The reason is not hard to find, as this locomotive allows a higher current to flow at the higher speed than does No. 15 or No. 17, giving a tractive effort at the tread to suit such speed.

Efficiency of Locomotives on Line.

The preceding table does not show that No. 12 locomotive has a low commercial efficiency; that this is not so can be seen from Table XIII. This table gives the efficiencies of the three locomotives for the starting and running periods, both at the draw-bar and tread of the wheel. The horse-power at the draw-bar is obtained from the average pull, as shown by the dynamometer and the average speed during the starting and running periods. To get the horse-power at the tread of the wheel, the locomotive is, as to air resistance, journal friction, &c., treated as if it were a carriage,

the tractive resistance per ton being taken from the curve (Fig. 29) for the different speeds; and this amount, added to the draw-bar pull, gives the tractive effort at the tread of the wheel.

Turning to No. 12 locomotive, the average efficiency for four sections at the draw-bar during the starting period is 26·0 per cent., and the efficiency at the tread of the wheel is 52·35 per cent.; the efficiency of No. 15 is 28·4 and 53·2 per cent. under similar conditions, while No. 17 has an efficiency of 30·5 and 57·7 per cent. at the draw-bar and tread of wheel respectively.

During the running period, No. 12 locomotive gives the highest efficiency at the draw-bar, viz., 73·4 per cent. This is partly due to this locomotive being so much lighter than the other two that it naturally requires less power than the others to move itself along. Its efficiency at the tread of the wheel is 86·4 per cent. No. 15 locomotive has an efficiency of 71·6 per cent. at draw-bar and 88·15 per cent. at the tread of the wheel. The respective efficiencies for No. 17 are 69·75 per cent. and 86·25 per cent.

The C^2R losses are greatest in No. 12 locomotive, and amount to 23·5 per cent. at starting and 12·5 per cent. during the running period, although its resistance is less than that of No. 17, where the internal losses are 20·0 and 10·2 per cent. No. 15 has the smallest C^2R losses, being 12·8 per cent. at starting and 8·12 per cent. during the running period. A high C^2R loss at starting is, however, no real disadvantage if it can only be got rid of after the external resistance in the regulating switch is all cut out.

Taking the sum of the percentage of useful work appearing at the tread of the wheel, and the C^2R losses for the three locomotives, we are able to account for 98·9 per cent. of the energy put into No. 12, 96·27 per cent. for No. 15, and 96·5 per cent. for No. 17. There is thus an average of 3 per cent. unaccounted for. This 3 per cent. is probably accounted for by treating the locomotive as a carriage when taking the tractive resistance per ton, and is partly due to the larger effective area of the locomotive front setting the column of air ahead of it in motion, and to increased journal friction in the locomotive as compared with the ordinary train journal friction.

Turning back for a moment to the starting peri·

No. 12 locomotive 75·8 per cent. of the total energy can be accounted for, and it can be safely assumed 3 per cent. must be added as already explained. This leaves 21·2 per cent. of the energy supplied to the locomotive wasted in regulating resistance, until the train is under way. In No. 15, 31 per cent. is lost in external resistance, and in No. 17 the loss in external resistance is 19·3 per cent.

Electric Locomotive Design.

From the experience gained by the foregoing locomotive tests, and knowing exactly the tractive force required

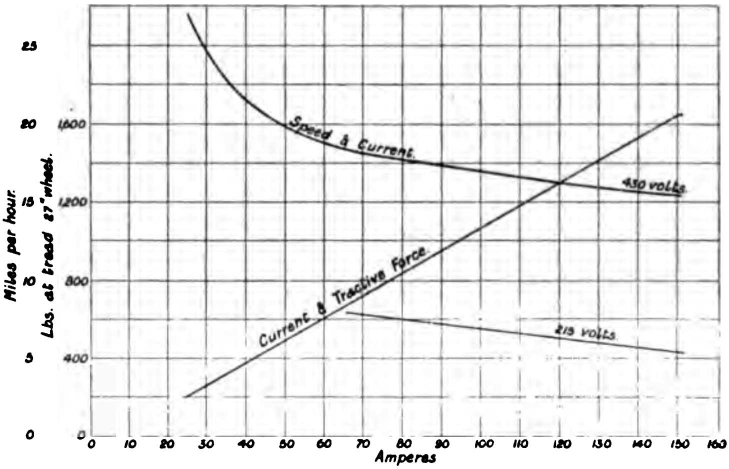


Fig. 30.—Characteristic Curve of Motor No. 1.

at the draw-bar of the train from the curve (Fig. 29), a further set of investigations was gone into. It was thought that an improvement might be made in No. 12 locomotive by winding the motors to give a higher tractive effort per ampere. This would solve the "starting period" part of the problem, but at the expense of the "running period," if the existing switching arrangements were employed. The switching arrangement gave very good results in the past, and the cost of repairs was exceedingly small, so that it was with a certain amount of reluctance that it was decided to try the series-parallel method.

From the Prony brake tests of these motors, the magnetisation curve and other data required for the following

calculations were obtained. The necessary calculations were made for altering the armature and field magnet winding, utilising the same carcase. The original number of conductors on the armature was 540. It was decided to increase this by 50 per cent., or to re-wind with 810 conductors. The characterstic curves of the re-wound motors were then calculated and plotted (Fig. 30). It will be seen that the tractive force at the tread of the wheel (27 in. diam.) was 1,595 lbs. for 150 amperes, falling to 415 at 50 amperes. The speed in miles per hour at average voltage (430 volts) was 15·5 with 150 amperes, and 22·75 miles per hour with 50 amperes; with a pressure of 215 volts (corresponding to the series position) the speeds were 5·5 miles per hour at 150 amperes and 9·3 at 70 amperes.

With the above curves it is possible to show the performance of this motor in actual work on the rails. The length between the starting and stopping point is taken as 2,700 feet. This is about the average length between stations or short sections on the City and South London Railway, and in some of the proposed new schemes. To simplify calculations, the section is assumed level and free from curves. This assumption will not of course affect the general result, as the same section is considered in each case.

The weight of the locomotive and four-coach train now under consideration is 49 tons. From the curve, Fig. 29, giving the tractive resistance per ton at various speeds a table was compiled, from which could be found the total tractive force necessary to overcome the resistance due to the locomotive and train, at all speeds within the limits of this investigation. Points were taken every two miles: thus between 0 and 2 miles per hour the average tractive resistance was 1,640 lbs.; between 2 and 4 miles per hour it was 906 lbs.; in the next interval, that is between 4 and 6 miles per hour, it fell to 707 lbs., and so on.

Turning now to the characteristic curves of the locomotive (Fig. 30), and limiting the starting current to 150 amperes, the tractive force at the tread of the wheel is 3,280 lbs. for two motors, and this can be maintained until a speed of 5·5 miles per hour has been reached by the locomotive. In the interval between 0 and 2 miles per hour we have a tractive force of 3,280 lbs., of which 1,640 lbs. is absorbed

by train resistance, &c., leaving a force of 1,640 lbs. available for accelerating the train and locomotive. The kinetic energy stored up at 2 miles per hour is 14,500 ft. lbs., and as the average force acting in the interval was 1,640 lbs. the space passed through must be 8·87 ft., and the time taken 6·06 secs. In the interval between 2 and 4 miles per hour the tractive resistance is 906 lbs., leaving 2,374 lbs. for accelerating the train and locomotive; the kinetic energy stored up between 2 and 4 miles per hour is 43,700 ft. lbs.; and dividing this by the average force of 2,374 lbs. gives 18·4 feet as the space traversed in the interval, the time occupied being 4·2 secs. In the same manner, between 4 and 6 miles per hour the resistance is 707 lbs., the tractive force begins to fall at 5·5 miles on account of the current falling, so that the average tractive force in the interval is 3,060 lbs. for a current of 140 amperes, the space travelled is 32 feet, and the time is 4·39 secs. In the next interval the current has an average value of 100 amperes, giving a tractive force of 2,100 lbs., the resistance is 660 lbs., and the kinetic energy is 101,500 ft. lbs.; working out as before, we find that a space of 70·5 feet is travelled through in 6·91 seconds.

It will be seen that the train and locomotive have now attained a speed of 8 miles per hour, and have passed through a space of $8{\cdot}87 + 18{\cdot}4 + 32 + 70{\cdot}5 = 129{\cdot}7$ feet in $6{\cdot}0 + 4{\cdot}2 + 4{\cdot}39 + 6{\cdot}91 = 21{\cdot}5$ seconds. At this point it is convenient to put the motors into parallel. They can of course remain in series until 10 or 12 miles per hour has been attained; a saving is thereby effected in current consumption, but at the expense of tractive force, and as will appear later, at considerable sacrifice in the total efficiency of the locomotive.

When the motors are placed in parallel the current is limited to 125 amperes per motor or 250 amperes for the locomotive. The proper limit of current when the motors are placed in parallel is fully considered later on. This current of 250 amperes will begin to fall at 17·0 miles per hour, and is prevented from exceeding that amount by external resistance, when the motors are placed in parallel. The tractive resistance in this interval (8 to 10 miles per hour) is 660 lbs., and the tractive force is 2,580 lbs., leaving 1,920 lbs. for acceleration; the kinetic energy is 131,000 ft. lbs., with the result that 68·2 feet is passed through in 5·21 seconds.

Proceeding in a similar manner at 2-mile intervals until a speed of 20 miles per hour is reached, we find that in the interval between 18 and 20 miles per hour the tractive force is 1,140 lbs. for an average current of 116 amperes, and the resistance is 857 lbs., leaving 283 lbs. for acceleration, whilst the kinetic energy is 276,000 ft. lbs.; treating as before, the space travelled is 572 feet, and the time occupied is 20·6 seconds.

After this point is reached the locomotive is unable to give a surplus over the tractive force necessary to overcome the tractive resistance at 20 miles per hour, and so the speed curve turns over and becomes flat. Tabulating the values obtained, the form below offers a ready means of detecting errors, and the speed and current curves can readily be plotted from the table.

Miles per hour.	Time in seconds.	Total time, seconds.	Distance in feet.	Total distance, feet.	Amperes in interval	Tractive force, lbs.
0	...	...	...	...	150	3280
2	6·06	6·06	8·8	8·8	150	3280
4	4·20	10·20	18·4	27·2	140	3060
6	4·39	14·59	32·0	59·2	100	2100
8	6·91	21·50	70·5	129·7	250	2580
10	5·21	26·70	68·2	197·9	250	2580
12	5·22	31·90	84·0	281·9	250	2580
14	5·40	37·30	102·0	384·0	250	2580
16	5·43	42·70	118·7	502·0	200	2150
18	7·57	50·30	184·0	686·0	116	1140
20	20·60	70·90	572·0	1258·0		

When a speed of 20 miles per hour is attained, the locomotive and train have passed through a distance of

1,258 ft., and as the total length of the section is 2,700 ft., there remains a space of 1,442 feet to travel.

From experience we know that a train can be brought to rest from 20 miles per hour in 20 seconds without inconvenience to the passengers. Adopting this as a standard negative acceleration, the time taken to bring the train to rest from 20 miles is 20 seconds, and a space of 292 ft. is passed through during that time; this leaves 1,150 ft. to travel at 20 miles per hour, occupying 39·4 seconds. The total time taken from switching on the current and until the train is brought to rest in the next station is 130·3 seconds.

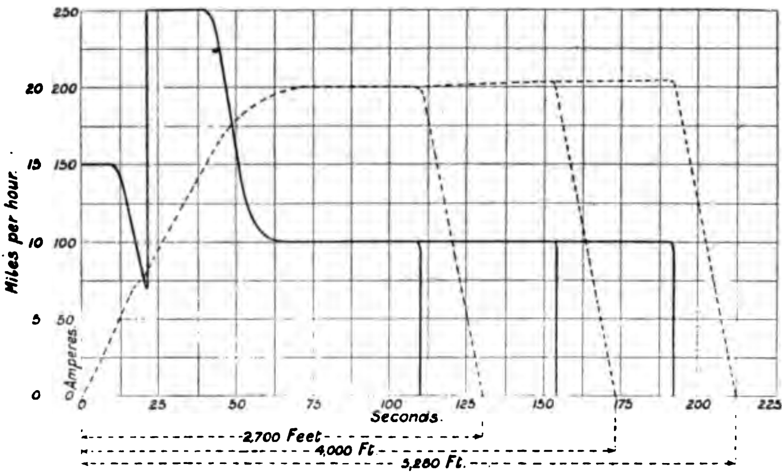


FIG. 31.—Motor No. 1; Performance Diagram.

Plotting these results, we obtain the two curves shown on Fig. 31. The solid line represents the amperes at 430 volts, and the dotted curve the speed in miles per hour.

Having described the manner in which the two curves showing the performance of the locomotive on the rails are obtained, the effect of limiting the current when the motors are placed in parallel may be considered.

Another set of curves was worked out for this same locomotive under exactly similar conditions to the last, as to length of section, weight of train, and starting current limit, the last-named being 150 amperes. The current when the motors were put into parallel was, however,

limited to 140 amperes, or 70 amperes per motor. These curves are given in Fig. 32, and show very clearly the performance of the locomotive in actual work, and also the difference arising from a change in current supply.

As the remaining part of this paper deals chiefly with curves obtained in a similar way under varying conditions as to winding and as to supply of current to the locomotive, they may perhaps be called Electric Locomotive "Performance Diagrams."

Referring to Fig. 32, the energy supplied is plotted in kilowatts instead of in amperes (at constant voltage) as before, in order to show more clearly the loss in external

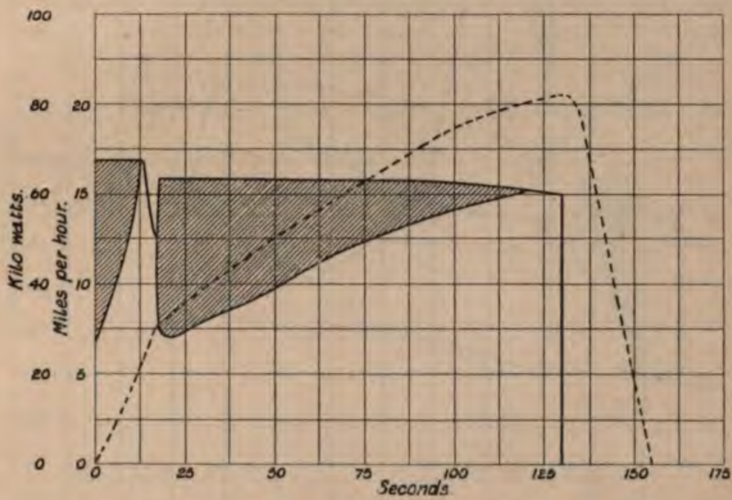


FIG. 32.—Diagram showing effect of Low Starting Current.

resistance by keeping the starting current down. It will be noticed that the time required to run the 2,700 feet is 155 seconds. The electric energy taken from the line was 2,200 watt-hours, and of this amount only 1,679 watt-hours were usefully employed. The shaded portion of the energy curve shows the watt-hours wasted in external resistance to keep the current within the specified limits. If the motors when in series will stand a current of 150 amperes there is no reason, so far as they are concerned, why 150 amperes per motor should not be allowed when they are in parallel. The chief objection to this manner of working is the additional strain thrown on the generating machinery;

but when a sufficient number of locomotives is running, this jump from about 80 to 300 amperes should not be of much consequence, and if it proves very objectionable the corners can be rounded off by making the first jump 200 amperes and almost instantly raising the current to 300 amperes.

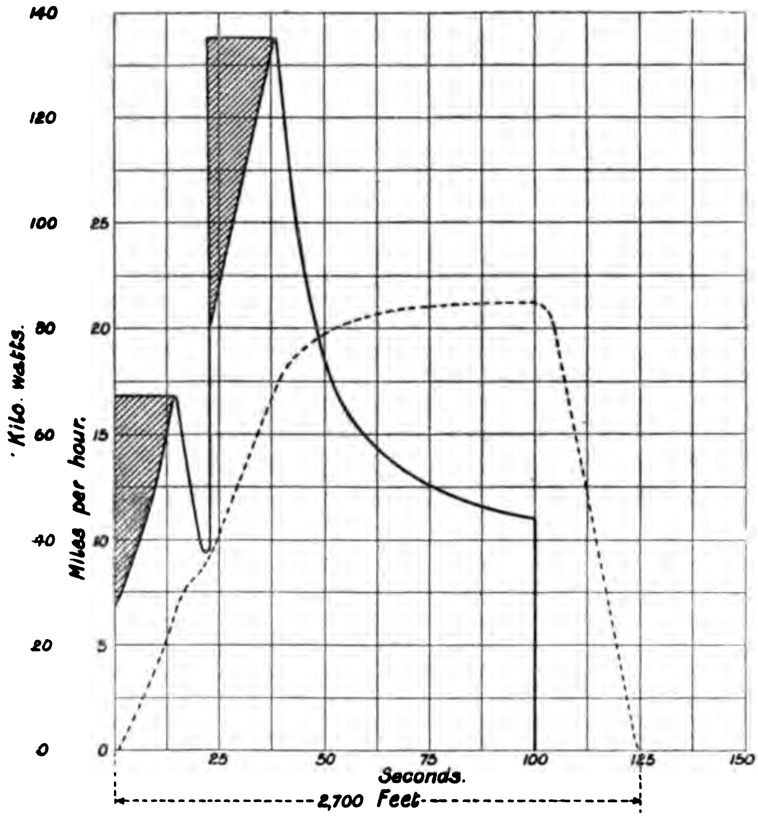


FIG. 33. Diagram showing effect of High Starting Current.

Neglecting the generators for the present, Fig. 33 shows the locomotive performance when 300 amperes are allowed at the moment when the motors are placed in parallel. The run occupies 125 seconds, and 1,945 watt-hours are drawn from the line, 1,736 of these being usefully employed. The shaded portion of the curve shows the watt-hours wasted in external resistance. A further study of these two figures shows that the time taken is 24 per cent. longer in the first

case than in the second, and also that the watt-hours are higher by 11·5 per cent. This enormous gain should more than compensate for the extra strains, and shows how different methods of driving may affect the economy.

From the above, it appears that a high starting current, and consequently a high tractive force, kept up as long as possible at starting has a decided advantage. With this idea in view it seemed that a still further increase in the number of conductors on the armature might be an advantage. A fresh winding was worked out for the same carcase, the number of conductors being increased to 972.

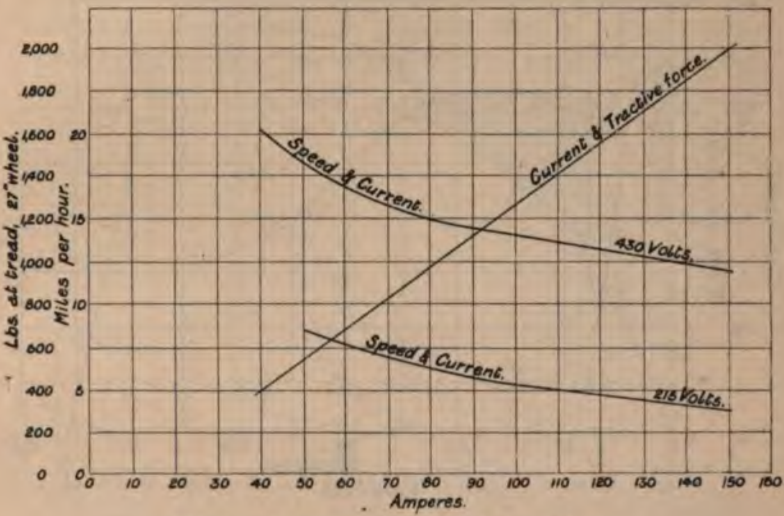


FIG. 34.—Characteristic Curve of Motor No. 2.

The characteristic curves of this motor are shown in Fig. 34. It will be noticed that for 150 amperes the tractive force at the tread of the wheel is 2,000 lbs., and this can be maintained up to a speed of 4 miles per hour at 215 volts, and up to 12·3 miles per hour at 430 volts; while at 50 amperes the tractive force is 540 lbs., and can be maintained up to 8·4 miles per hour at 215 volts, and up to 18·25 miles per hour at 430 volts. Working out a "performance diagram" for this locomotive as already explained in the case of the motor with 810 conductors, and limiting the current to 140 amperes when the motors are put into parallel, we arrive at the curves plotted in Fig. 35. The electric energy is

here plotted in kilowatts, and the loss in external regulating resistance is shaded as before. The time taken to run the 2,700 ft. is here 140 seconds, the total watt-hours are 1,584, and the watt-hours usefully employed are 1,420. Comparing these results with those already obtained with the first motor under the same conditions of starting and parallel current limits, the time taken shows a saving of 10·7 per cent., and the watt-hours a saving of 38·9 per cent. If the current were allowed to reach 300 amperes when the motors were placed in parallel, still greater economy would result.

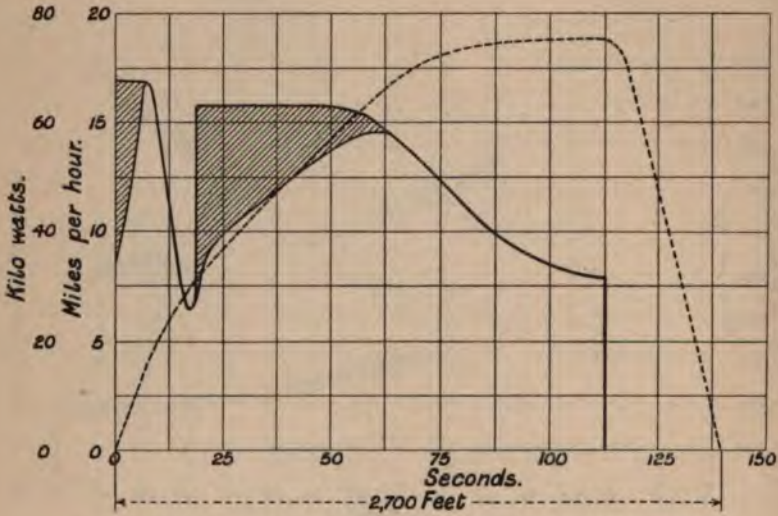


FIG. 35.—Showing Effect of Low Starting Current with No. 2 Motor.

It seems from the last "performance curve" as if the number of armature conductors could be still further increased, but a limit must be reached on account of the extra armature resistance. To test the accuracy of this supposition another set of characteristic curves was calculated for the same carcase, with the number of armature conductors increased to 1,134. This reduced the cross-section of the wire to a point below which it did not seem desirable to go. In order to avoid confusion, it may be well at this point to give in a tabulated form some details of the winding of these motors.

Distinguishing number.	Number of conductors.	Armature resistance.	Field resistance.	Total resistance of Motor.	
No. 1	810	·45 ohm	·21 ohm	·66 ohm	
" 2	972	·618 "	·135 "	·75 "	
" 3	1134	·856 "	·218 "	1·064 "	
" 4	810	·673 "	·123 "	·796 "	Motor armature 50 per cent. longer.

The characteristic curves of this No. 3 motor wound with 1,134 conductors are given in Fig. 36. The tractive force at the tread of the wheel is here 2,300 lbs. per motor for 150 amperes, falling to 670 lbs. at 50 amperes; the former can be maintained until a speed of 1·75 miles per hour is

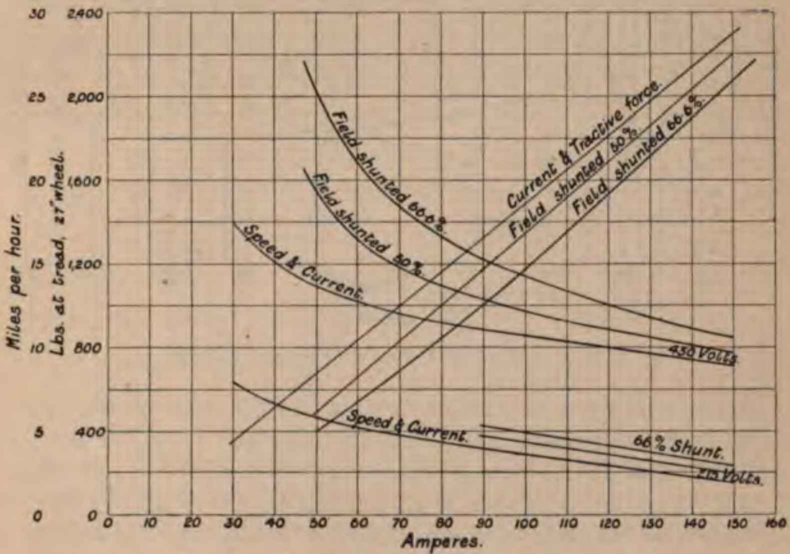


FIG. 36.—Characteristic Curve of Motor No. 3.

reached, and the latter up to 6 miles per hour at 215 volts, while at 430 volts the respective speeds are 9 and 14 miles per hour. In order to compare the results that would be likely to be obtained in practice from these three motors wound as described, their "performance curves" were worked out for each motor under as nearly as possible the same

conditions. The current at starting with the motors in series was 150 amperes; they were put in parallel at what appeared to be the most economical speed, and the current was limited to 250 amperes, or 125 amperes per motor. By the term "most economical" speed is meant a point in the speed curve with the motors in series before it commences to become flat, and when with the motors placed in parallel there would not be too great a rush of current. This gives a fairly uniform acceleration, as can be seen from the shape of the curve.

Fig. 31 gives the "performance curve" of No. 1 motor. The time required to run the 2,700 ft. is 130 seconds, the

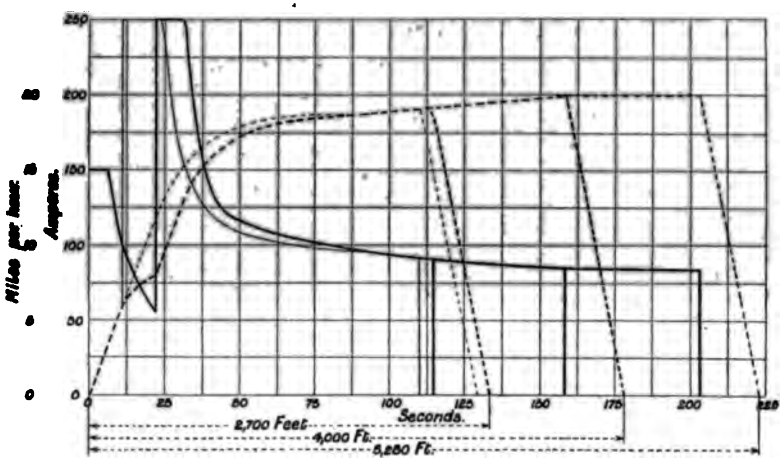


FIG. 37.—Motor No. 2; Performance Diagram.

electric energy drawn from the line is 1,905 watt-hours, and the maximum speed attained is 20 miles per hour. Fig. 37 shows that No. 2 motor takes 133 seconds to travel the same distance, and draws 1,700 watt-hours from the line, while the maximum speed reached before the current is shut off is 19 miles per hour.

The "performance diagram" of No. 3 motor is plotted in Fig. 38, from which we can see that it requires 145 seconds to run the section, the watt-hours are 1,410 and the maximum speed 16·5 miles per hour.

If the section is to be travelled in, say, 130 seconds, this motor clearly will not fulfil the required conditions, and *recourse* must be had to weakening the fields in order to get

a higher speed. By shunting the motor fields first with a 50 per cent. shunt, or allowing only half the armature current in the field, and then reducing the strength of the field still further as the speed increases, it is possible to make these motors do the work in the required time. In Fig. 36 are shown the characteristic curves of this motor with the field shunted 50 and 66·6 per cent. From an inspection of the "performance diagram," it will be seen that when a speed of 12 miles per hour is reached the 50 per cent. shunt is used; by this arrangement the current rises from 145 to 200 amperes with an increase in tractive effort which can be

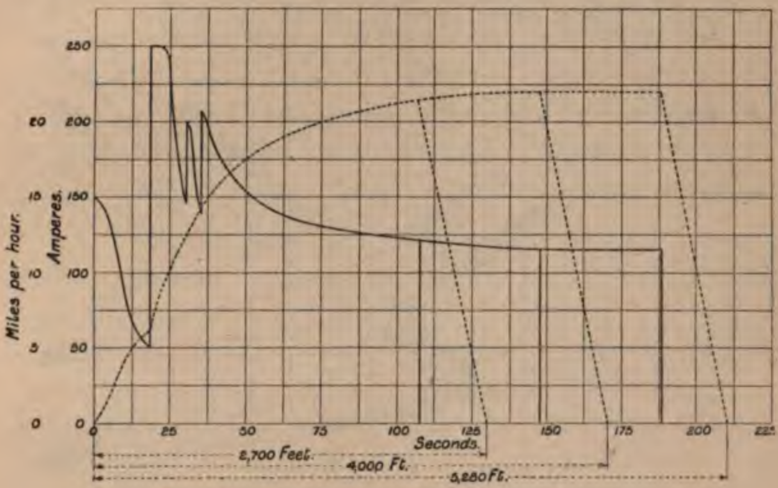


FIG. 38.—Motor No. 3; Performance Diagram.

maintained at a higher speed, but of course at the expense of current. When 14 miles per hour is attained the second shunt is used, with the result that the locomotive attains a maximum speed of 21·5 miles per hour before the current is shut off and the brakes are applied. In this manner the section is run in 129·1 seconds, with a consumption of 1,780 watt-hours.

No. 3 is clearly the most economical motor under the above conditions; but by placing No. 2 in parallel earlier the watt-hour consumption is 1,770 and the time 129 seconds.

The effect of altering the dimensions of the carcase was next tried, keeping to the same type of motor. It was im-

possible to increase the armature diameter, so the only course left open was to increase its length, retaining the same diameter. The armature and magnet cores were increased in length by 50 per cent., and the same number of conductors was used as in the case of No. 1 motor, viz. 810.

Fig. 39 gives the characteristic curves of this motor with 50 and 66 per cent. shunts, and further description seems unnecessary. The "performance diagram" Fig. 40 shows that the starting current was 150 amperes, and that the current when the motors were placed in parallel was limited

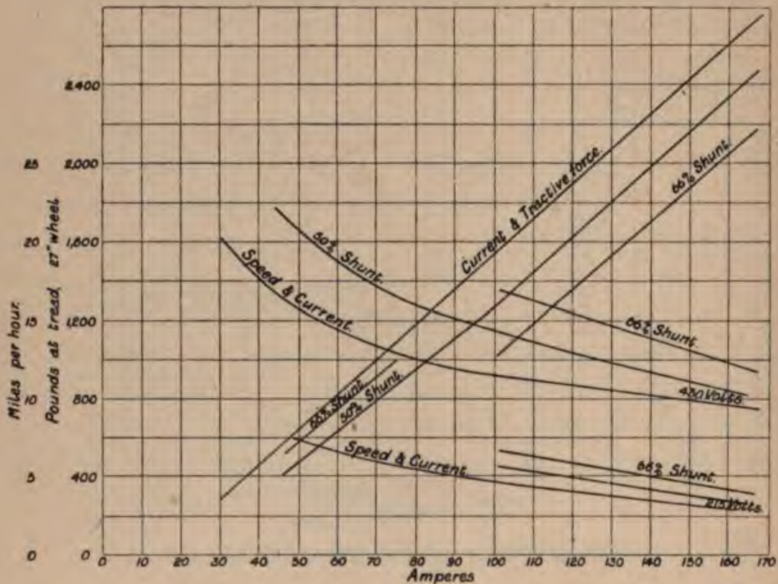


FIG. 39.—Characteristic Curve of Motor No. 4.

to 250 amperes as before. As the distance of 2,700 ft. could not be covered in the time (130 seconds), it was necessary to shunt the field. It was found by trial, that by using the 50 per cent. shunt at 16 miles per hour, the locomotive would perform its work in 126·6 seconds for a consumption of 1,552 watt-hours.

It will be seen that these motors give the best results yet obtained; but of course they are not strictly comparable with the first three, on account of the increased dimensions.

At this stage it was thought that the length of section

might have an important effect upon the performances of the above four motors.

The "performance diagrams" were then worked out in each case for lengths of 4,000 and 5,286 feet respectively. The curves, figures 31, 37, 38, and 40, clearly show the alteration in the "performance diagrams" in each case, and the following table shows the time taken for running the sections, and the watt-hours consumption.

Motor No.	2,700 feet section.		4,000 feet section.		5,280 feet section.	
	Time in seconds to run.	Watt-hour consumption.	Time in seconds to run.	Watt-hour consumption.	Time in secs. to run.	Watt-hour consumption.
1	130	1,905	173	2,372	211	2,760
2	133	1,700	179	2,160	221	2,660
3	130	1,780	170·3	2,360	210	2,900
4	126·6	1,625	170	2,150	208	2,660

Calling No. 4 motor the standard and expressing the differences in percentages, we have the following table :—

Motor No.	2700 feet section.		4,000 feet section.		5,280 feet section.	
	Percentage increase.		Percentage increase.		Percentage increase.	
	Time.	Watt-hours	Time.	Watt-hours	Time.	Watt-hours
4	Taken	as stan	dard of	compari	son.	
1	2·7 %	17·3%	1·76%	5·67%	1·44%	2·6%
2	5·58%	4·6%	5·29%	·46%	6·2 %	0
3	2·7 %	8·3%	·04%	9·77%	·09%	9·1%

We will consider the 2,700 ft. section, as it is no doubt the most important, at least in connection with the present system of underground railways. No. 1 motor is the worst

when both time and consumption of energy are considered, for here we have an increase of 2·7 per cent. in time and 17·3 per cent. in watt-hours. With No. 2 the increase in time is 5·08 per cent., while the watt-hours are only increased 4·6 per cent. In the case of No. 3 the increase in time is 2·7 per cent. and that in the watt-hours 8·3 per cent. For a short section clearly No. 3 comes next in economy to No. 4; but as the latter is slightly heavier, No. 3 must naturally be classed as the most efficient motor for the same weight.

This seems to show the necessity for winding the arma-

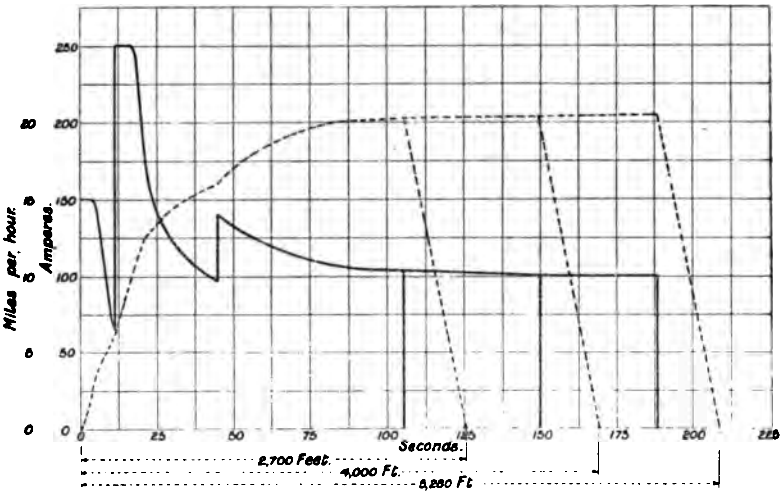


FIG. 40.—Motor No. 4; Performance Diagram.

ture with as high a number of conductors as can be got on with safety. A striking feature, however, comes to light when this (No. 3) motor is running in the longer section; there is almost no difference in the time taken as compared with No. 4, but the watt-hour consumption increases by 9·8 per cent. for the 4,000 feet section and by 9·1 per cent. for the 5,280 feet section. From this proportionate increase it is fair to assume that for longer sections still this motor would prove even less economical. On the other hand, No. 1 motor shows greater economy as the length of section is increased, and No. 2 comes close to No. 4.

The above deductions, while very interesting and im*portant,* lead one from the immediate part of the subject

under consideration, *i.e.*, the most economical way to wind a given motor, or to build a motor to put in a given space, and when the motor is built to get the most out of it, with the least expenditure of energy, for a section 2,700 ft. long. The subject was then attacked from another point of view as follows. From practice it appears that to run a level section 2,700 ft. long, 130 seconds is a convenient time to meet ordinary traffic requirements. Given this basis, the lowest speed at which it is possible to run the section is 14·25 miles per hour, assuming infinitely quick positive and negative acceleration, or in other words, entering the section at full speed and running through without stopping. This is of course an impossible condition, but the nearest approach to it is to get up to full speed in the shortest possible time, and to stop in the same manner. As explained earlier, a negative acceleration of 1·46 feet per second per second, corresponding to stopping in 20 seconds from a speed of 20 miles per hour, does not cause discomfort to the passengers and is not too hard on the brakes and wheels. In practice we sometimes get an acceleration of 2 feet per second per second or more, but 1·46 feet per second will be taken as the maximum in this investigation.

If for practical reasons it is not desirable to adopt a higher negative acceleration than 1·46 feet per second per second in bringing the train to rest, there is clearly no need to try and get a higher positive acceleration at starting. Adopting this acceleration for starting and stopping, we find that the speed at which the locomotive and train must run, after the acceleration is attained is 16·25 miles per hour. This is made clear by Fig. 41. Here the ordinates represent the speed in miles per hour, and the abcissæ the time in seconds. A series of speed curves can be obtained which will enclose the same area, and consequently any curve of this series will enable the run to be accomplished in the specified time. Stopping from 20 miles per hour in 20 seconds was adopted as the negative acceleration in all cases, and positive accelerations of 1·46, ·974, ·73, ·584, ·487, and ·417 feet per second per second were also adopted, corresponding to starting from rest and attaining speeds of 20 miles per hour in 20, 30, 40, 50, 60, and 70 seconds respectively. A line was drawn parallel to the abscissæ from each of the above acceleration lines (if they may be so called), enclosing the same area in each case ;

this gave the speed at which the train must run, after attaining its acceleration, until the brakes are applied.

With a uniform acceleration of 1·46 feet per second per second during the starting period, we see from Fig. 41 that the speed during the running was 16·25 miles per hour until the brakes are applied. The next speed curve, with its

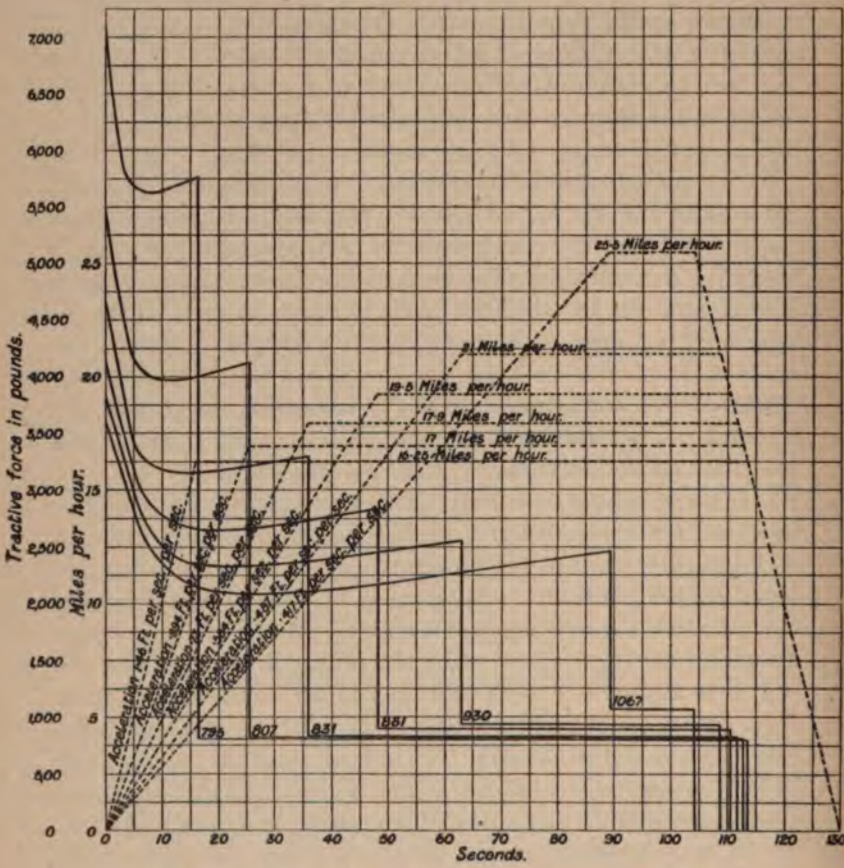


FIG. 41.—Diagram of Accelerations and Tractive Forces.

acceleration of ·974 feet per second per second during the starting period, requires a speed of 17·0 miles per hour during the running period. For accelerations of ·73, ·594, ·487, and ·471 feet per second per second, during the starting periods, the respective speeds during the running periods, and until the brakes are applied, are 17·9, 19·5,

21·0, and 25·5 miles per hour. It is evident from this that the more quickly the train attains full speed, the slower the running speed will be. And as will be seen from a study of the tractive force curves, corresponding to the various speed curves, the slower the running speed, the lower the kinetic energy stored up at starting, and taken out again by the brakes in stopping.

Turning now to the tractive force required to obtain the various conditions laid down above, we see (Fig. 41), that to give the train and locomotive an acceleration of 1·46 feet per second per second a force of 4,915 lbs. is required, and this must be kept up for 16·3 seconds until a speed of 16·25 miles per hour is attained. To this must be added the tractive resistance taken from Fig. 29 at every interval, as already explained. The total tractive force, then, exerted by the motors at the moment of starting is 7,125 lbs., dropping to 6,250 lbs. at 2 miles per hour, 5,750 lbs. at 4, 5,650 lbs. at 6, and 5,625 lbs. at 8 miles per hour; it now commences to rise again, and is 5,650 lbs. at 10 miles per hour, 5,700 lbs. at 12, 5,725 lbs. at 14, and 5,750 lbs. at 16 miles per hour; when 16·25 miles per hour is reached, it drops to 795 lbs., this being the tractive force necessary for running at that speed.

The remaining five tractive force curves were arrived at in a similar manner, and need no further explanation. Returning for a moment to the tractive force curve for an acceleration of 1·46 feet per second per second, we see that 4,975 lbs., acting for 16·3 seconds, that is 81,100 pound-seconds, were required for acceleration, and if this number is multiplied by the feet passed through in the specified time the result is foot-pound-seconds. It is, however, more convenient for present purposes to speak of pound-seconds or the area enclosed by the tractive force curve. When the tractive resistance is added to the above the pound-seconds for the starting period are 95,220, and the running period requires 77,400 pound-seconds, making a total of 172,620 pound-seconds for the run.

The following table gives the results of the six sets and is self-explanatory :—

Second to attain 20 miles per hour.	Acceleration in feet per sec. per sec.	Lbs.-secs. for acceleration only.	Lbs.-secs. for acceleration and tractive resistance.	Difference due to tractive resistance.	Lbs.-secs. for running period.	Total lbs.-secs. for 2,700 ft. run.	Percentage increase due to acceleration.	Percentage increase in total lbs.-secs.
20	1·46	81,100	95,220	14,120	77,400	172,620	Standard	
30	·974	84,600	106,350	21,750	70,600	176,950	4·32%	2·51%
40	·730	89,400	119,540	30,140	63,150	182,690	10·24%	5·84%
50	·584	95,600	136,140	49,540	55,200	191,340	17·87%	10·86%
60	·487	104,600	158,870	54,210	42,650	201,460	29·0 %	16·7 %
70	·417	126,800	204,300	77,500	16,070	220,370	56·4 %	27·65%

This table shows the enormous advantage to be gained by adopting the highest acceleration compatible with the practical details of motor design. It fortunately happens, however, that we have a fairly large choice without increasing by more than 6 per cent. the total energy drawn from the source of supply. In designing a locomotive, it is not of very great importance, or at least it will not affect the final result to any great extent, if the sharp peak of the tractive force curve at the moment of starting is averaged over the whole period of starting. This will reduce the size of the motors and the tractive force that they must give out during the first few seconds, and will prevent the speed curve from rising in a perfectly straight line as shown. Thus in the case of tractive force curve for an acceleration of 1·46 feet per second per second, the tractive force instead of being 7,125 lbs. at the moment of starting, may be reduced to an average of 5,839 lbs. over the starting period.

Fig. 41 and the table of results show the way in which to apply the tractive force to obtain economical working. It now remains to find how far these suggestions can be carried out in practice, remembering that the locomotive has only two motors and that the space available is limited.

Turning to the table on page 50, we see that No. 4 motor gave the best results. The maximum speed attained was 20 miles per hour (Fig. 40), and this corresponds to a mean or average acceleration during the starting period of between ·584 and ·487 feet per second per second (Fig. 41). The

table on page 55, shows that this performance is somewhere about 12 per cent. more extravagant than the ideal motor (the motor efficiency, &c., is of course here neglected). The speed curve during the starting period follows a fairly straight line until a speed of 12 miles per hour is reached, and then bends over slowly. If this line could be maintained straight during the whole starting period better results would follow; but this means that the tractive force of the motors must be kept constant, or fairly so, during the period, and the back-electromotive force of the motors prevents this desirable state of affairs from being realised. If the ideal be unattainable in these matters one must be satisfied with the nearest possible approach to it, and this is fairly arrived at by keeping the current up to its starting value by shunting the field after the current commences to drop. The tractive force per ampere is reduced, but the "performance diagrams" that follow show that it is far more economical to successively shunt the field to a certain point when the motors are in series, before going into parallel, and when in parallel and the current again drops to commence shunting the field until, say, the sparking limit of the motor is reached, or the tractive force per ampere has fallen so low that acceleration can no longer be economically obtained. If at this point the field be unshunted, the current and tractive force will drop and the locomotive will run at the required speed (if the motor is so designed).

Following out the lines laid down above in calculating a new "performance diagram" for No. 4 motor. The starting current is limited to 150 amperes as before, and when the current has fallen to 114 amperes at 14 miles per hour, the fields are shunted 50 per cent.; the current now rises to 142 amperes, falling again to 114 at 5 miles per hour; here the field is again shunted 66 per cent, and the current rises to 138 amperes. In the ordinary way at this point the current would have been 55 amperes, with a correspondingly reduced tractive force. The motors are now (at 6 miles per hour) put into parallel, and the total amperes limited to 300; when a speed of 11 miles per hour is attained the current has fallen to 228 amperes; here the 50 per cent. shunt is used, allowing the amperes to reach 300, but these fall again to 236 at 13 miles per hour. At this point the 66 per cent. shunt is used, and the current

rises to 202 amperes, falling to 223 when a speed of 16 miles per hour is attained. The shunts are now disused, the current drops to 97 amperes, and gradually falls to 76 amperes at 18 miles per hour. The run is here completed in 128·5 seconds for an expenditure of 1,477 watt-hours. It is interesting to compare this diagram, Fig. 42, with the former "performance diagram," Fig. 40, for the same motor. In the former the shunts were used during the running period, and in the latter during the starting

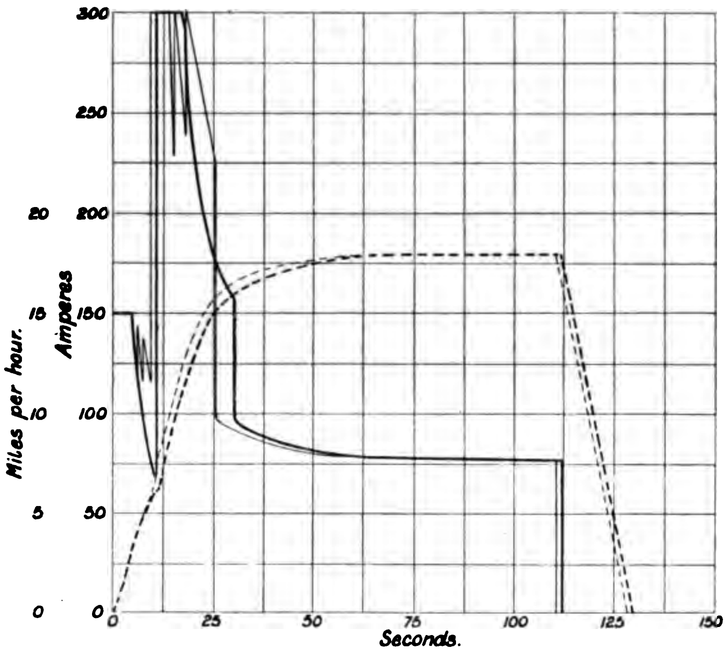


FIG. 42.—Motor No. 4; Performance Diagram.

period. The average current during the running period with a shunt was about 115 amperes for 60 seconds, while without a shunt the current for the same time was only 77 amperes.

For the same motor used in a different manner the watt-hour consumption is reduced from 1,624 to 1,477. The time is, however, 1·9 seconds longer, which is equivalent to an increase of 1·5 per cent., while there is a gain of 8·86 per cent. in the watt-hours.

The second "performance diagram," Fig. 42 (thick line),

shows the effect of not using a shunt in series, and only the 50 per cent. in parallel; the watt-hour consumption is the same as before, but there is an increase of 1·17 per cent. in the time.

Instead of using only two shunts as explained, a series of six or more could be used in practice without unduly complicating the controller, beginning with a 10 per cent. shunt and continuing until 60 or 70 per cent. is reached.

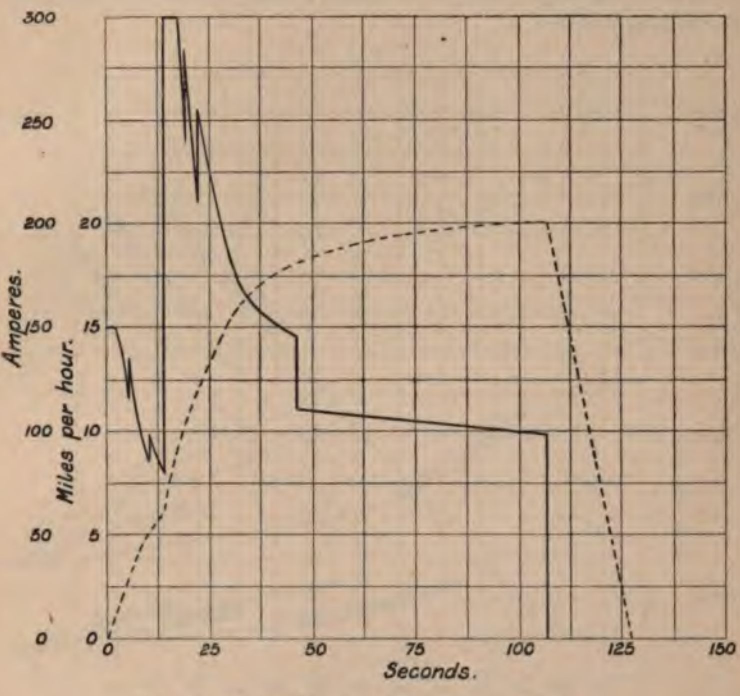


FIG. 43.—Motor No. 3*a*; Performance Diagram.

In this way the top of the current curve could be kept straight instead of jerky, as plotted. It need hardly be added that the calculations were made easier by only considering two instead of six or seven shunts.

Having obtained considerable economy by shunting the field at starting in the case of No. 4 motor, the same arrangement was tried with No. 3, that is the motor with 1,134 conductors and short armature. The "performance diagram," Fig. 43, was worked out, and the field was shunted

both in series and in parallel. The jump in the ampere curve shows the points at which the field was shunted. The watt-hours required for the section were 1,730, and the time was 127·5 seconds. There is a saving of 1·96 per cent. in time, and 2·89 per cent. in watt-hour consumption for this method of using the same motors as compared with shunting the field during the running period. The saving is small as compared with the case of No. 4 motor, and points to having a longer armature (the diameter being, of course, constant).

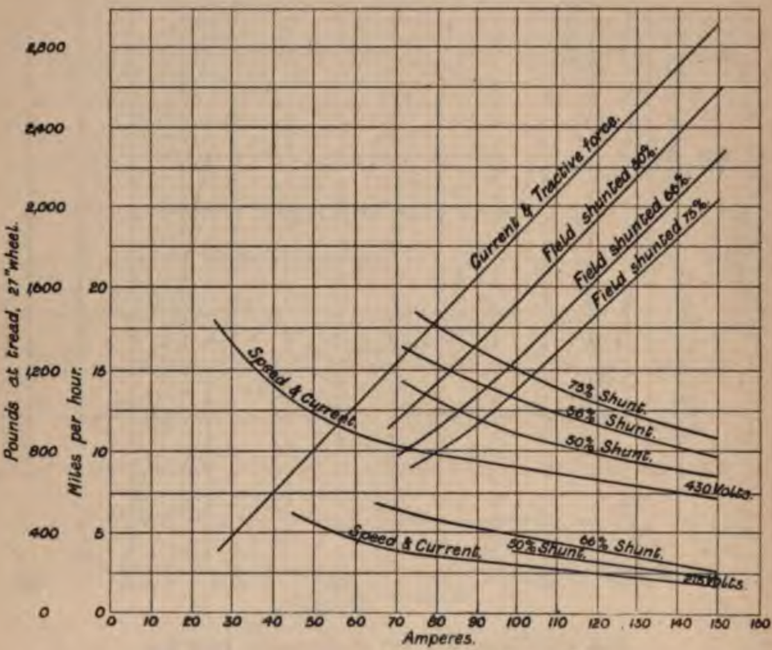


FIG. 44.—Characteristic Curve of Motor No. 5.

The motor with 972 conductors (No. 2) was then taken, and the armature length increased by 50 per cent., the same number of conductors being retained. The characteristic curves of this motor, which we shall call No. 5, are given on Fig. 44, and a comparison with Fig. 39 shows that the tractive force per ampere is greater than with No. 4 motor, but at a slower speed for the same tractive effort. The "performance diagram," Fig. 45, for No. 5 motor shows that it was necessary to use a third shunt when the motors

were in parallel, and even then it took 132·5 seconds to complete the run. The watt-hours were 1,489. Comparing this with the "performance diagram" Fig. 42, which is the most economical yet obtained, the time shows an increase of 2·87 per cent., and there is also an increase of 1·29 per cent. in watt-hour consumption. This seems to indicate that the most economical motor had already been found in No. 4, and that Nos. 3 and 5 show a falling off in economy.

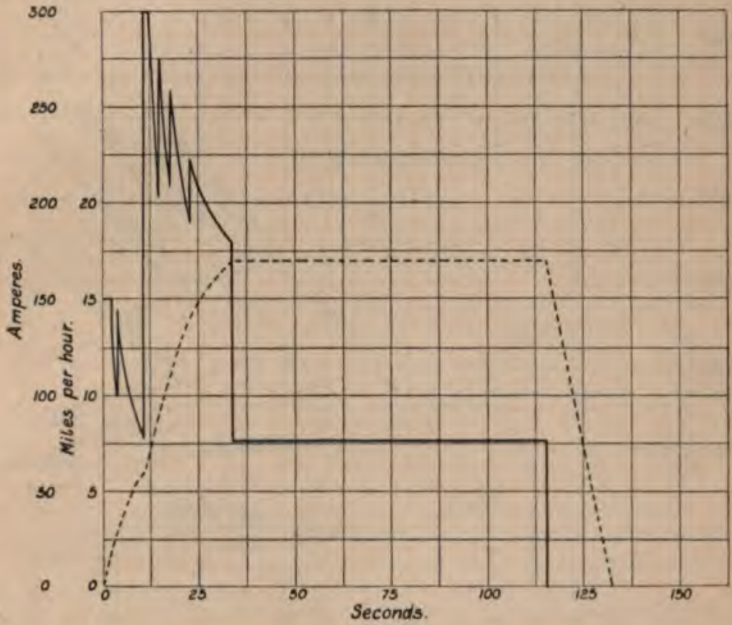


FIG. 45.—Motor No. 5; Performance Diagram.

From the diagram, Fig. 41, it appears that the most economical method of running the section is to use a very high tractive force at starting; and as tractive force is the product of useful magnetic field and number of conductors for a given current, it would seem that No. 5 motor should have given better results. There is evidently some relation between the number of conductors and the strength of field for the same diameter of armature that will give maximum results. It is of course assumed that one is not tied down very closely to the length of armature, but is

allowed to utilise all available space between the locomotive wheels, due allowance being made for commutator, bearings, &c.

With these premises, and still keeping in view the necessity of a high tractive effort per ampere, notwithstanding the unsatisfactory results from No. 5 motor, another motor (No. 6) was designed to give the same tractive force per ampere as in No. 5, namely, 2,900 lbs. at tread of the wheel for 150 amperes, and 761 lbs. at 50 amperes. The length of

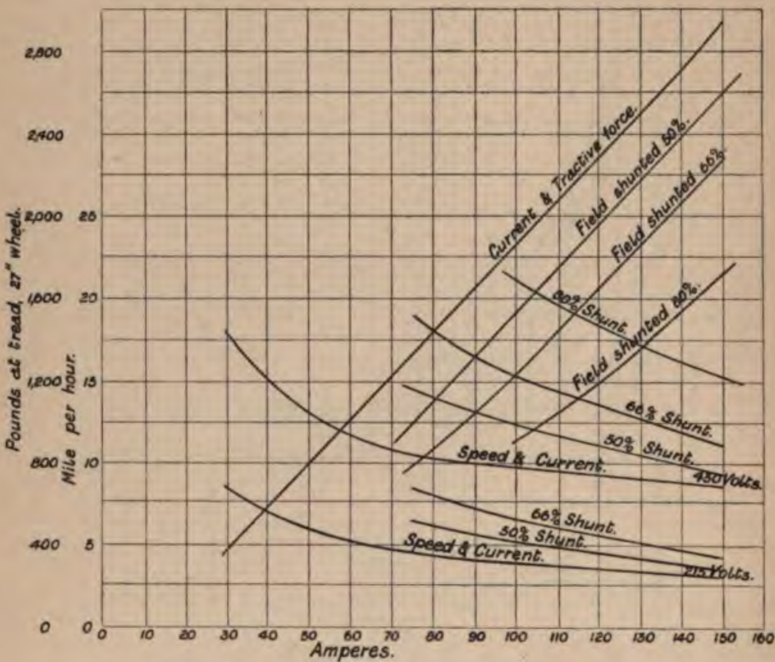


FIG. 46.—Characteristic Curve of Motor No. 6.

the armature was made twice that of No. 1 motor, or with a 100 per cent. increase on the original carcase, the same diameter being maintained. The number of conductors required to meet the above condition was 729; the characteristic curves of this motor are plotted in Fig. 46. It will be noticed that with 150 amperes at 430 volts a speed of 8·6 miles per hour can be maintained, the tractive force being 2,900 lbs. per motor. When 10 miles per hour is reached the tractive force is 1,650 lbs. for 90 amperes. In

the case of No. 5 motor, Fig. 44, the speed is 7·2 miles per hour for 150 amperes, and at 10 miles per hour the tractive force is 1,320 lbs. for 75 amperes. Thus, at a given speed, No. 6 motor consumes more current but exerts a higher tractive force than No. 5. The result of this is best seen by referring to the "performance diagram" of No. 6 motor, Fig. 47. The time taken for the run is 128·5 seconds, and the energy consumed is brought down to 1,330 watt-hours.

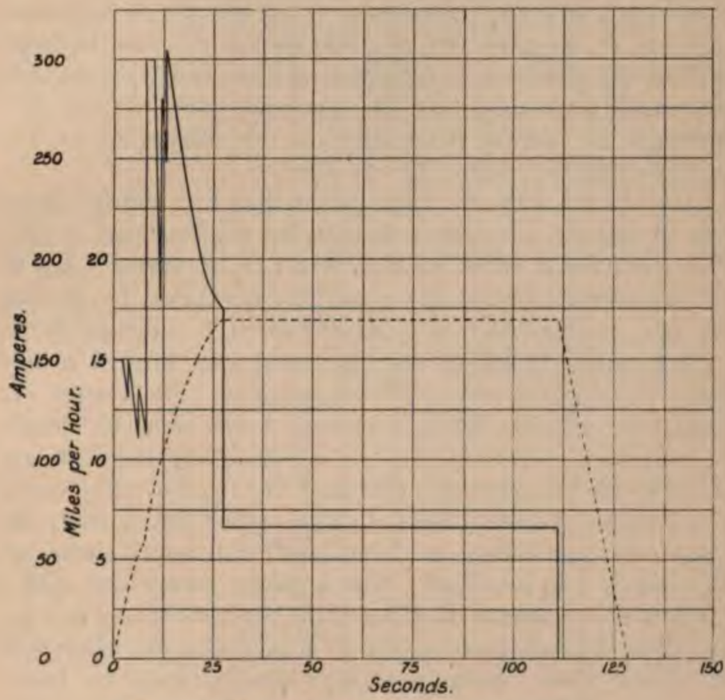


FIG. 47.—Motor No. 6; Performance Diagram.

It will be remembered that we started with a motor, No. 1, requiring 130 seconds to run the section and 1,905 watt-hours, and have now arrived at a motor, No. 6, doing the run in 128·5 seconds for a consumption of 1,330 watt-hours. The commercial efficiency of motor No. 1 is equal to that of No. 6 when both are tested as motors. The only reason that can be assigned for their different performances when placed in an electric locomotive is—that one motor is

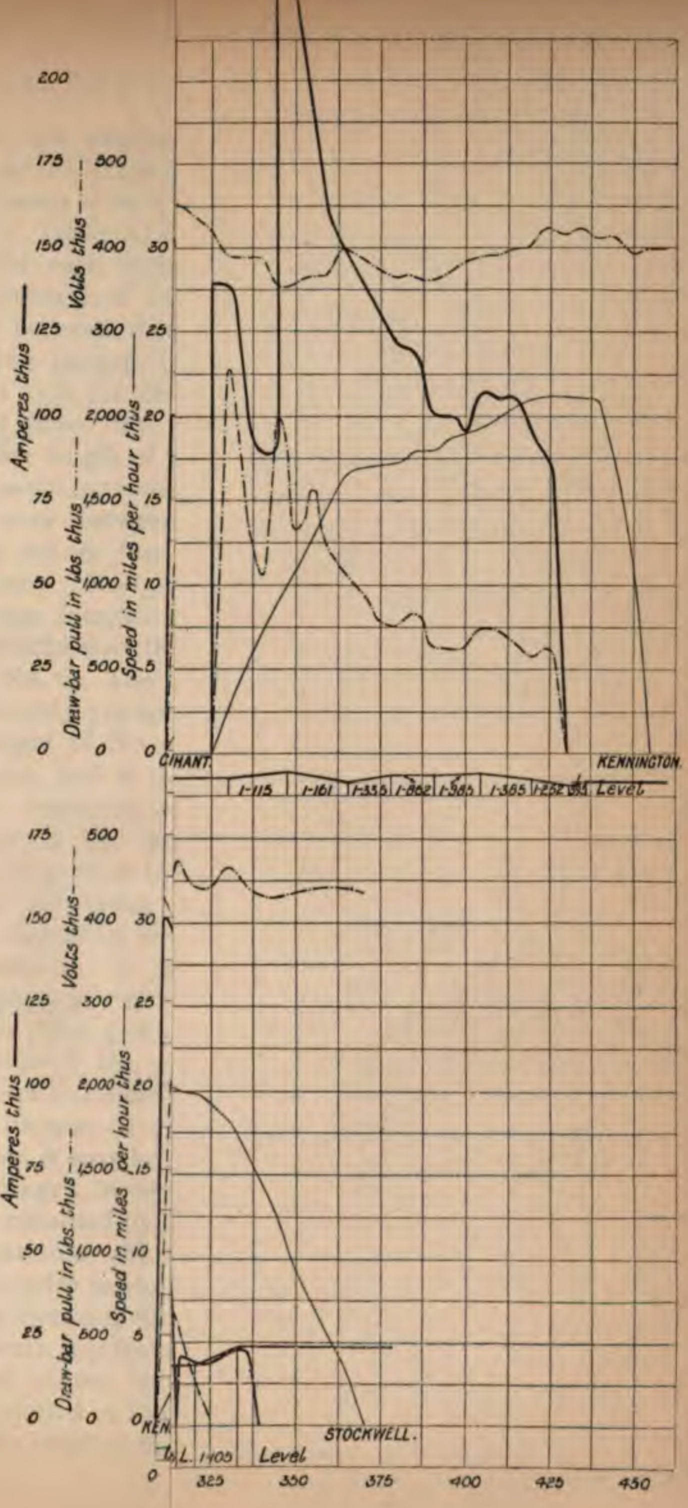
better adapted for the special conditions laid down than the other. Whether the watt-hour-consumption could be still further reduced is a matter that can be settled by trying a few more different designs; but as regards size we have reached our limits, and any further improvement must be in the arrangement of winding.

Before leaving this subject it is perhaps well to institute a more detailed comparison of the six different motors. This is rendered a little more difficult, as the time in every case is not the same. We have thus two variables, time and watt-hours, and the relationship is not strictly proportional. If, however, we pick out any one motor as a standard and express the increase or decrease of time and watt-hours in percentages, we may take the algebraic sum of these percentages as a total percentage or an indication of the suitability of the motor for its work.

We started with the assumption that the motor carcase was to remain constant and that the winding was to vary. This was carried out in motors Nos. 1, 2, 2*a*, and 3. No. 2*a* is introduced because the time was decreased by putting the motors in parallel at 6 instead of at 8 miles per hour. In these four examples the amperes were limited to 250 when the motors were put into parallel. The results are tabulated in Table XIV., and only a few remarks should be necessary. Taking No. 2*a* as standard, there is not much to choose between that and No. 2, the latter being 0·85 per cent. better. Next in order comes No. 3, where the combined percentage of time and watt-hours shows an increase of 1·34 per cent. No. 1 motor comes out with a combined increase of 8·37 per cent. It thus seems that for the same current limit, so far as it is under the control of the driver, that a motor with 972 conductors is far better than one with 810 conductors, and slightly better than one with 1,134 conductors. If, however, the parallel current is limited to 300 amperes and this is kept as nearly as possible constant by means of shunting the field, as already explained, the 1,134-conductor motor shows an improvement of 3·4 per cent. on the 975-conductor motor.

In the remaining five examples the parallel current was limited to 300 amperes, and shunting the field during the starting point was resorted to both for the series and parallel positions, except in the case of No. 4 motor, where

Level
1/25
1/1430
1-258
1-4
1-320
1-212
Level.
KENNINGTON.
ELEPHANT.
0
325
350
375
400
425



Amperes thus
Volts thus
Draw-bar pull in lbs. thus
Speed in miles per hour thus
CIHANT.
KENNINGTON.
1-115
1-161
1-335
1-862
1-385
1-385
1-252
Level
KEN.
STOCKWELL.
L. 1403
Level
0
325
350
375
400
425
450

the shunt was employed during the running period, the parallel current limited to 250 amperes.

No. 6 motor is here taken as the standard; the eleventh column of Table XIV. shows the combined percentage increase for each motor. Next to No. 6 comes No. 4*a*, with an increase of 11·05 per cent., and the remarkable difference between shunting the field during the starting and running periods is here shown by No. 4 having an increase of 19·8 per cent., caused by shunting the field at the wrong time. No. 5 shows a 14·9 per cent. increase, although the length of armature is the same as in No. 4*a*; it has, however, 972 conductors as against 810 on No. 4*a*. The difference between Nos. 4*a* and 5 is 3·9 per cent., and shows that for this length of armature 972 conductors are too many. No. 3*a*, with its 1,134 conductors, shows the very large increase of 29·7 per cent. over No. 6, but it must be remembered that its armature is only half as long as that of No. 6. The latter motor gives the best result of all, and has only 729 conductors.

The weight of No. 6 motor is greater than that of any of the others, and if this is taken into consideration the percentage combined saving would be slightly, but not much, lower, as the increased weight would only bear a very small proportion to the weight of locomotive and train.

Having laid down certain conditions, arrived at by theoretical reasoning, which govern the design of an economical locomotive, it may be well, before bringing this rather lengthy paper to a close, to describe what success has followed the putting of some of these ideas into practice.

The motors of No. 3 locomotive, which is of the same type as No. 12 locomotive, were re-wound with 810 conductors, or an increase of 50 per cent. on the old winding (the old armature, it may be remembered, had 540 conductors) and was arranged to work with a series parallel controller. The results obtained by this change are perhaps best shown by an inspection of the actual "performance diagrams" in ordinary everyday working conditions. Fig. 48 is obtained from a trip between Stockwell and the City, and Fig. 49 gives the return trip from the City to Stockwell. The speed and current curves taken on the Elephant-Kennington section are plotted in Fig. 50 for Nos. 3 and 12 locomotives, and show the effect of the alteration in the locomotive.

The time taken by No. 12 locomotive for a complete round trip, from Stockwell to the City and back, was 28 minutes 5 seconds (exclusive of the time required at the stations); while No. 3 locomotive performed the same journey in 24 minutes 10 seconds, or 15·8 per cent. less time than No. 12. The energy consumption for No. 3 was 13,150 watt-hours, and that for No. 12, 12,100 watt-hours; the latter thus took 9·85 per cent. less energy from the line than the former. At first sight it appears that there is not much gained by the change in locomotive. But to decrease the

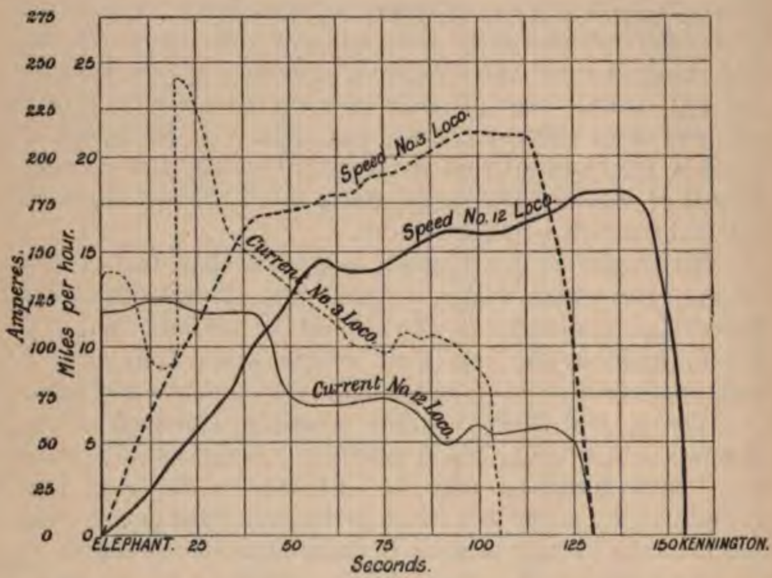


FIG. 50.—Diagram showing difference between Series and Series-Parallel control.

time by 15·8 per cent. means a very much greater increase in the work done, as will appear from the following. The average maximum speed attained during the trip in the case of No. 3 locomotive was 21·3 miles per hour, whilst in the case of No. 12 it was 18·5 miles per hour. The weight of the locomotive and train in each case was about 37·35 tons. The energy required to give the train and locomotive a velocity of 21·3 miles per hour was 129,600 foot-pounds, while that required to give the same train and locomotive a velocity of 18·05 miles per hour was 93,100 foot-pounds, a difference of 36,500 foot-pounds or a 39 per cent. increase

for acceleration alone; in addition to this there is a slight increase in the case of No. 3 locomotive due to a higher tractive resistance at the higher speed. With regard to the efficiency of No. 3 as compared with No. 12 locomotive, an inspection of Table XIII. shows that the efficiency on the line of No. 3 at starting is much greater than that of No. 12. Here the efficiency at the draw-bar is 46·1 per cent. for No. 3, while No. 12 has an efficiency of only 29 per cent.; the efficiencies at the tread of the wheel are 65·5 per cent. and 52·35 per cent. respectively. The C^2R losses for No. 3 are 27·6 per cent., and for No. 12, 23·5 per cent.; but the loss in external regulating resistance is only about 4 per cent. in No. 3, while in No. 12 it is about 21 per cent. The difference is, of course, usefully employed. During the running period, No. 12 locomotive is, however, about 3 per cent. more efficient than No. 3. The nett result is a decided gain in favour of the alteration, and may, perhaps, be looked upon as some proof of the soundness of the theories advanced on the design of an electric locomotive.

ADDENDUM.

Portions of this paper were written over two and a half years ago, and the complete paper was ready for publication in the early part of January, 1898. Circumstances over which the author had no control prevented its appearance until the present time. In the interval, three new locomotives have been added to the rolling stock of the City and South London Railway, and twelve more are now in course of construction. These locomotives differ in several points from their predecessors, and a short account of them, together with some tests as to their performance, are given in this addendum.

Two of the twelve new locomotives are being built in the Railway Company's own shops, and the first of these, No. 21, is described in detail; the remaining ten are being built to the same specification as No. 21.

When the Clapham and Moorgate Street extensions of the Railway are opened it is proposed to add another coach to each train, increasing the seating accommodation by 33 per cent., and also to run a two-, instead of the existing three and a quarter-, minute service.

Description and Tests of Locomotives.

Locomotives 19 and 20.—In view of the heavier trains, &c., it was decided to build more powerful locomotives. The size of the locomotive cab was practically limited to the same overall dimensions as the old locomotives on account of local conditions, and it was therefore necessary to get larger motors into a cab of the same size. In the old locomotives the magnets are very close to the working conductor, and to get more clearance a wheel 31 inches in diameter was adopted instead of a 27 in. wheel. This also allowed an armature of larger diameter to be used. Each locomotive is

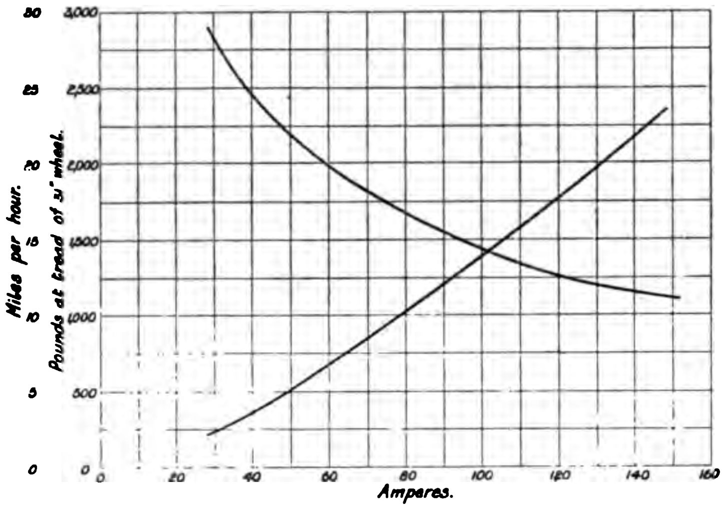
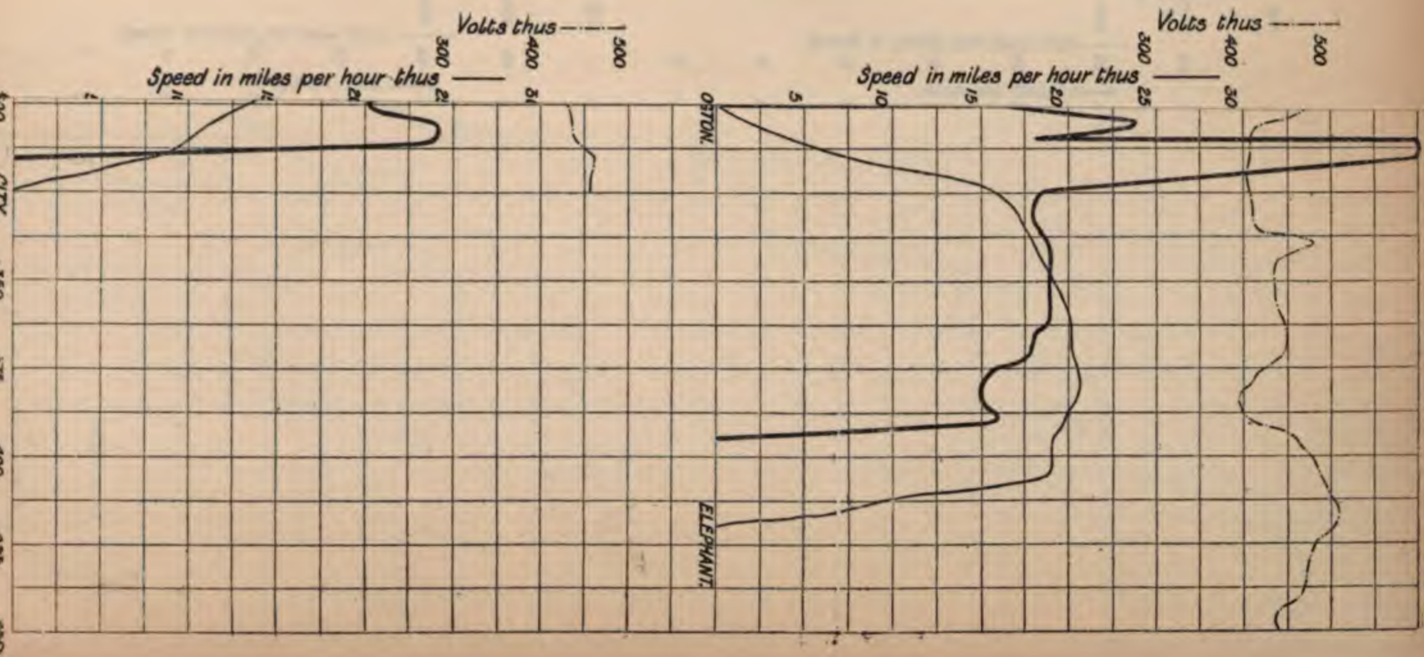


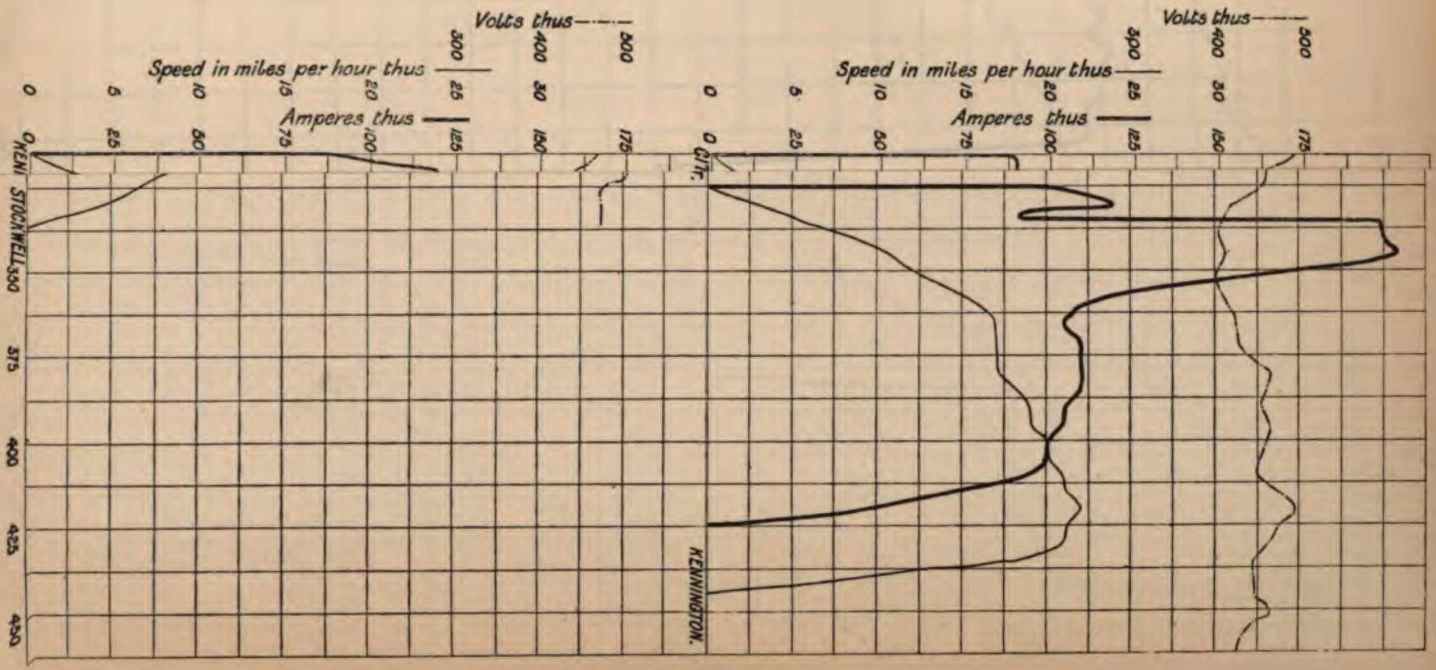
FIG. 51.—Characteristic Curve, No. 10 Locomotive Motor.

fitted with two motors of the four-pole type, the armatures having slotted cores and drum winding. The series-parallel method of control is adopted and, in addition to storage reservoirs for compressed air for the Westinghouse brake, independent electrical air-compressors are used. The weight of the locomotive complete is about twelve and a quarter tons.

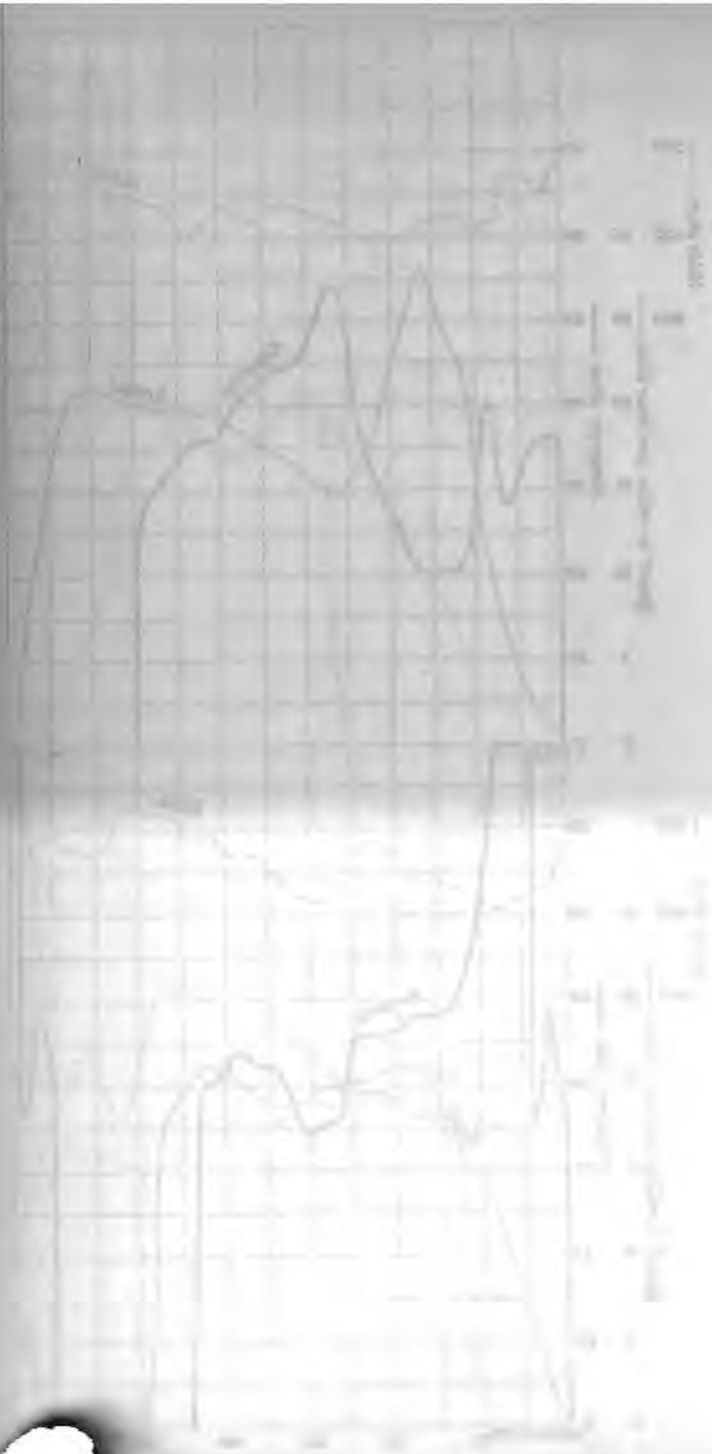
The characteristic curves of the motors of locomotives Nos. 19 and 20 are given in Figs. 51 and 52, from which it will be seen that the tractive force per ampere is considerably higher than in the case of the old locomotives. The "performance diagrams" on the line for a complete round

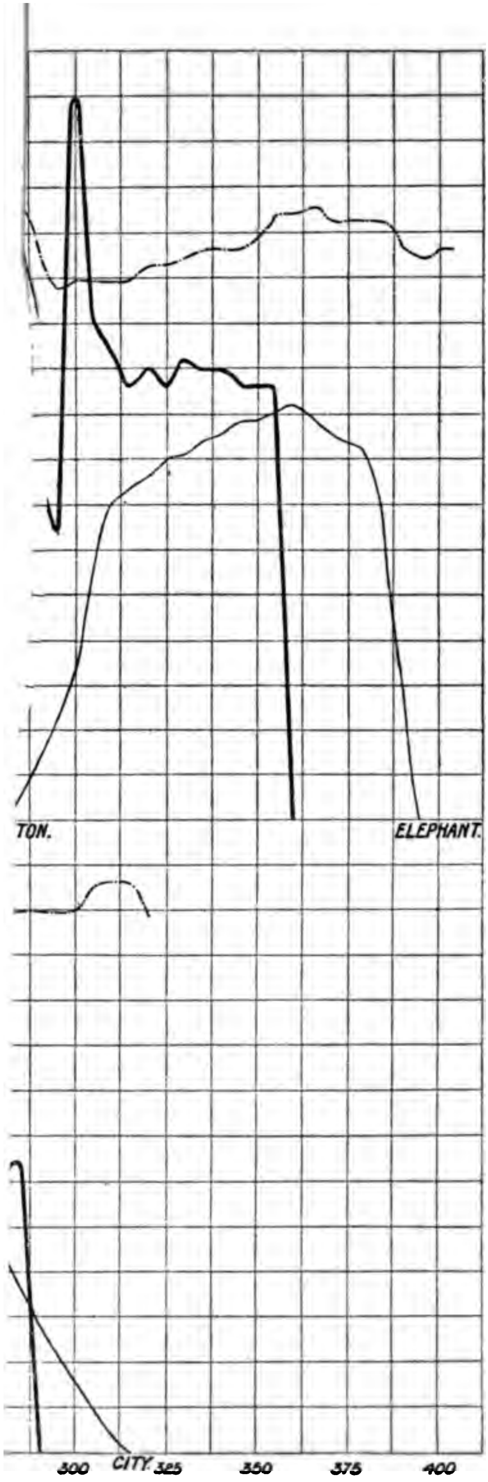


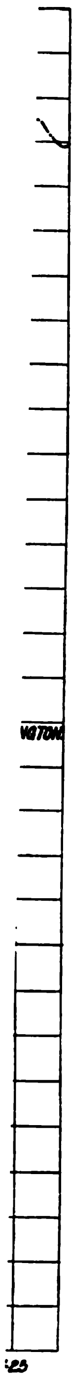
Volts thus
300
400
500
Speed in miles per hour thus
Volts thus
300
400
500
Speed in miles per hour thus
5
10
15
20
25
30
OSTON.
ELEPHANT.
CITY.
350
375
400
425
450

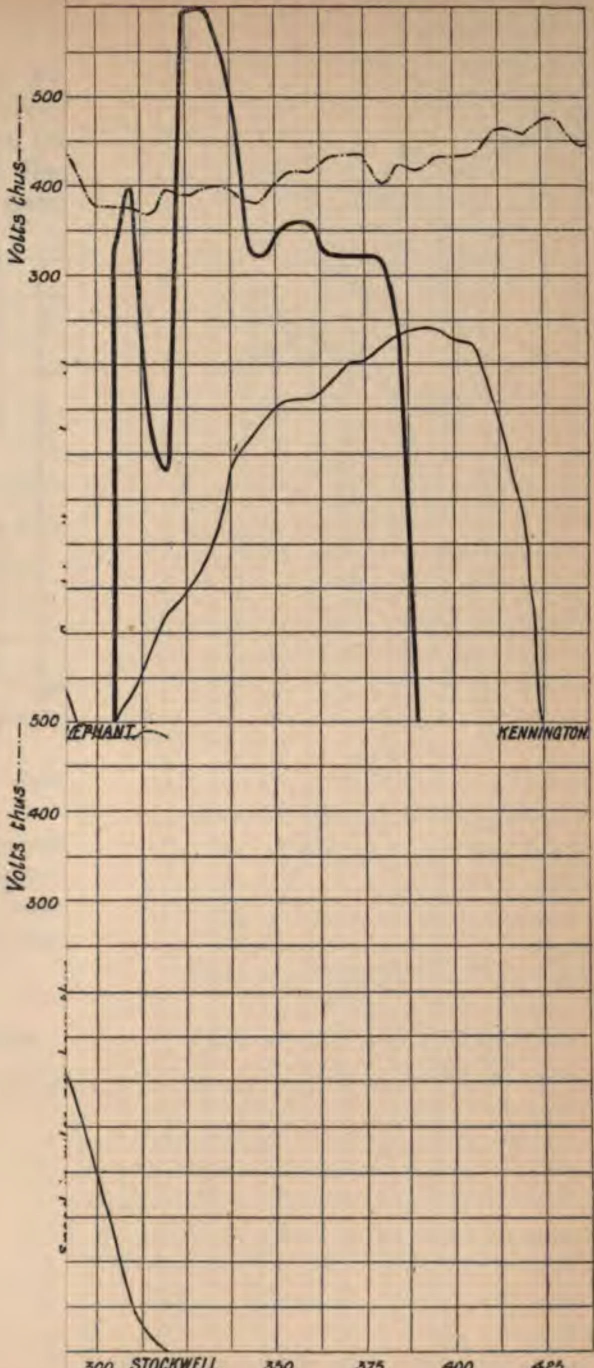


Volts thus
Speed in miles per hour thus
Amperes thus
KENI
STOCKWELL
350
375
400
425
450
Volts thus
Speed in miles per hour thus
Amperes thus
CITr.
KENNINGTON.




TON.
ELEPHANT.
CITY
300
325
350
375
400


·25


Volts thus—·—·—
500
400
300
EPHANT.
KENNINGTON.
Volts thus—·—·—
500
400
300
300
STOCKWELL.
350
375
400
425

trip are given in Figs. 53, 54, 55, and 56. These tests were made with a three-coach train, and as the locomotives were designed for a four-coach train service, they were not working under their most economical conditions. The results of these tests, together with the previous tests of Nos.

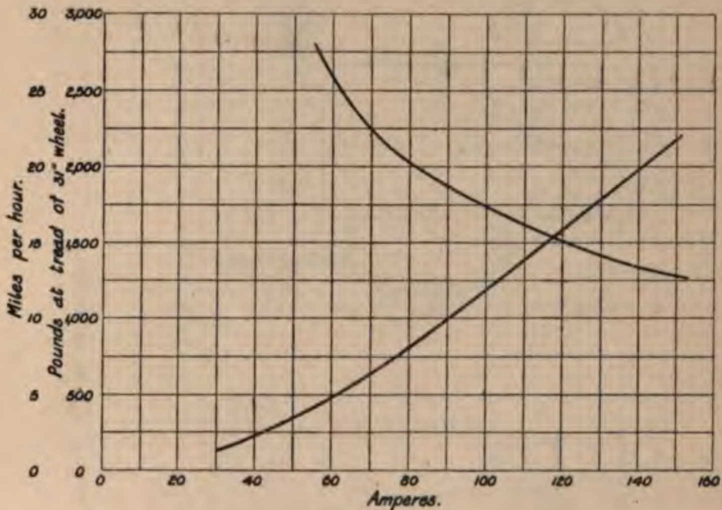


FIG. 52.—Characteristic Curve, No. 20 Locomotive Motor.

3 and 12 locomotives, are given in the following table in kilowatt-hours per "ton-mile." It will be noticed that while the energy per ton-mile is practically constant, there is an increase in the average speed.

No. of Locomotive.	Kilowatt-hour per ton-mile.	Average speed in miles per hour.
12	·0515	13·62
3	·0558	15·60
19	·0552	15·86
20	·0552	16·65

No. 20 locomotive gives the highest average speed, and

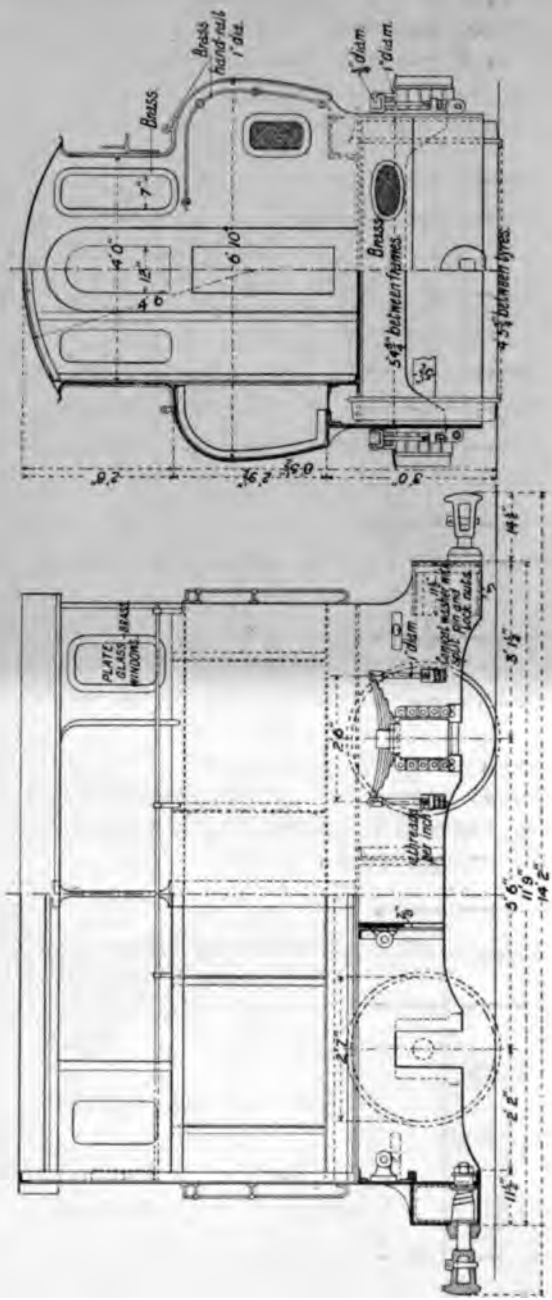


FIG. 57.—Side and End Elevation of Locomotive No. 21.

it may be mentioned here that when this locomotive was ordered the author was in a better position to specify the requirements, than when the order for No. 19 locomotive was placed. The latter locomotive was nearing completion before the former was required, and when the plans of No. 19 were prepared, the author had not gone so far into the question of locomotive design.

Locomotive No. 21.—The design and details of this and the remaining eleven locomotives were based upon the result of the tests and deductions already mentioned.

The general appearance and leading dimensions of the cab are given in Fig. 57, from which it will be seen that the wheels are 31 inches in diameter, and the wheel base is 5 feet 6 inches. The motors are so arranged that the floor of the cab is level, and the driver has considerably more room than in the old locomotives. The steel cylinders forming the air reservoirs are arranged one on each side of the cab, and have a total capacity of 18 cubic feet. Under the reservoir on one side are fitted the Westinghouse brake cylinder and brake gear connected therewith, and the electrically driven air-compressor; while on the other side, the switch-board containing the series-parallel controller, reversing switch, motor cut-out, and main switches and fuse are placed; the regulating resistances are fitted directly under the air-reservoir on this side, close to the controlling arrangements. The drawing, Fig. 58, gives the general arrangement and details of the motor, which is of the four-pole type, with the field magnet coils on the horizontal poles. The magnet is split across the diagonal corners, and the brackets carrying the motor bearings are divided in halves, so that each part of the magnet carries its bracket with it when the halves are separated to get the armature out. The armature is drum wound with the conductors in slots. The characteristic curves of the motor are given in Fig. 59. A description of the controlling arrangement and connections is added below. The weight of the locomotive complete is about twelve and a quarter tons.

A "performance diagram" of this locomotive hauling a four-coach train is given in Fig. 63, from which it will be seen that the time taken to run a level section 2,700 feet long is 130 seconds, the current being shut off 74 seconds after starting, the locomotive coasting for 41·5 seconds, and

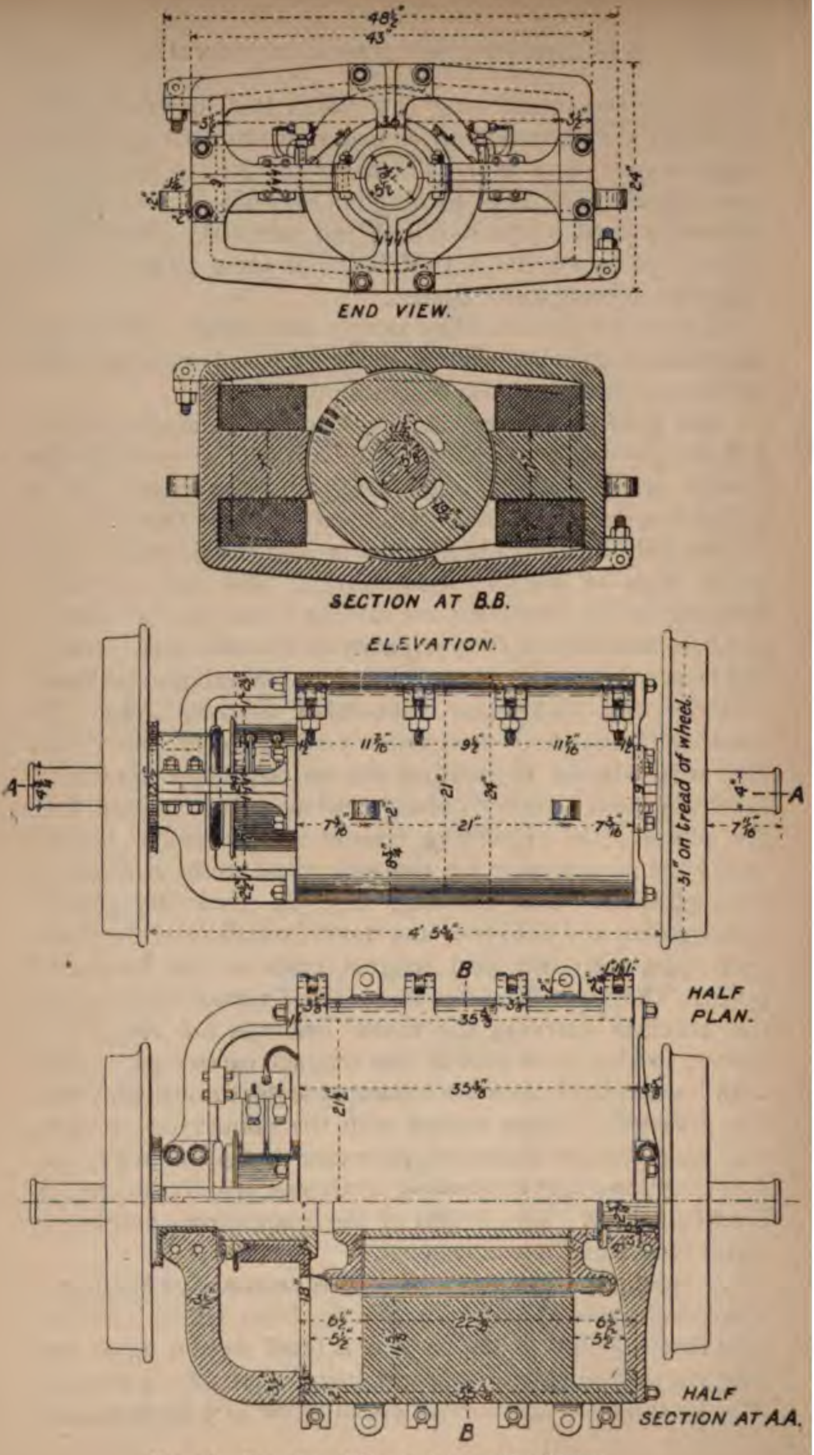


FIG. 58.—Drawing of No. 21 Locomotive Motor.

the brakes being then applied. The watt-hour consumption for the section is 1,360. If the current is kept on until the brakes are applied the section can be run in 122 seconds, the watt-hours being 1,650. This, it will be seen, is very

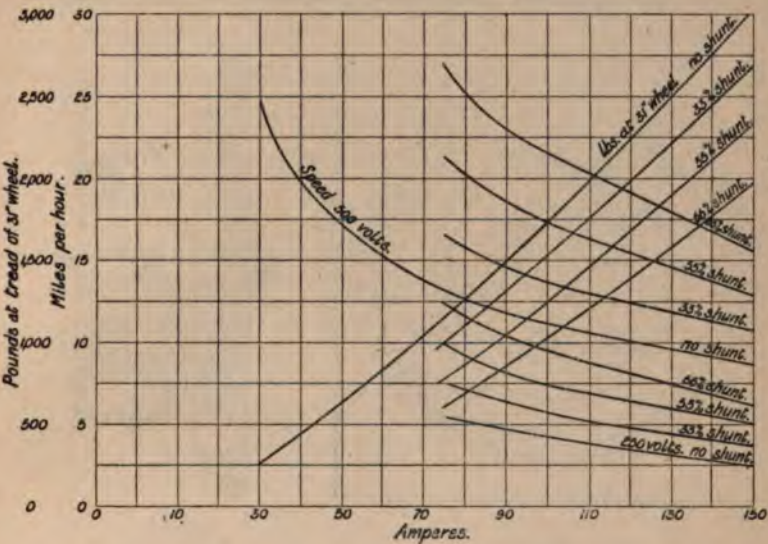


FIG. 59.—Characteristic Curves, No. 21 Locomotive Motor.

similar to the ideal motor No. 6 referred to in the earlier part of the paper, where the time and energy under the same condition were 128·5 seconds and 1,267 watt-hours respectively.

METHOD OF CONTROL.

In electric traction with separate locomotives there are conditions slightly different from those where motor-cars are used. In coupling-up the locomotive to the train the driver cannot always stop in the exact position to enable the coupling to be attached; and it is often necessary to move the locomotive through a mere fraction of an inch. Experience has shown that to avoid bad flashing on the controller contacts when a current of, say, 100 amperes is suddenly switched on and off before the motors have made a fraction of a revolution, the regulating resistance should be divided into a large number of steps. The plain rheostat

switch used on the old locomotives answered this purpose admirably, and in adopting a series-parallel controller this object was kept in view. Further, the drivers were used to this class of switching-gear, and a series-parallel controller, retaining the old form of rheostat switch, was developed. This series-parallel controller consists of two switches

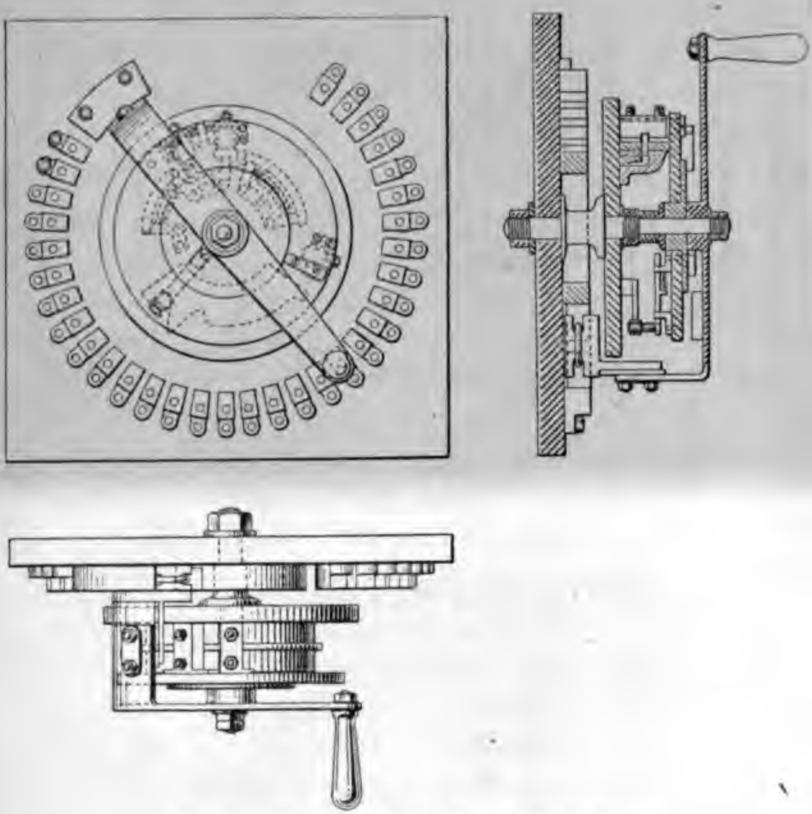


FIG. 60.—Drawing of Series-Parallel Controller.

operated by a single handle, that part of the controller which puts the motors into parallel being moved only at predetermined points. Magnetic blow-outs, or other spark-killing devices, are not used, the motors being put into parallel practically without any flashing on the contacts, and not being put back into the series position again until the current is switched off the locomotive. The drawings and

photographs, Figs. 60, 61, and 62, show the construction and connections of this arrangement. It is not necessary to lock the paralleling part of the switch to avoid short-

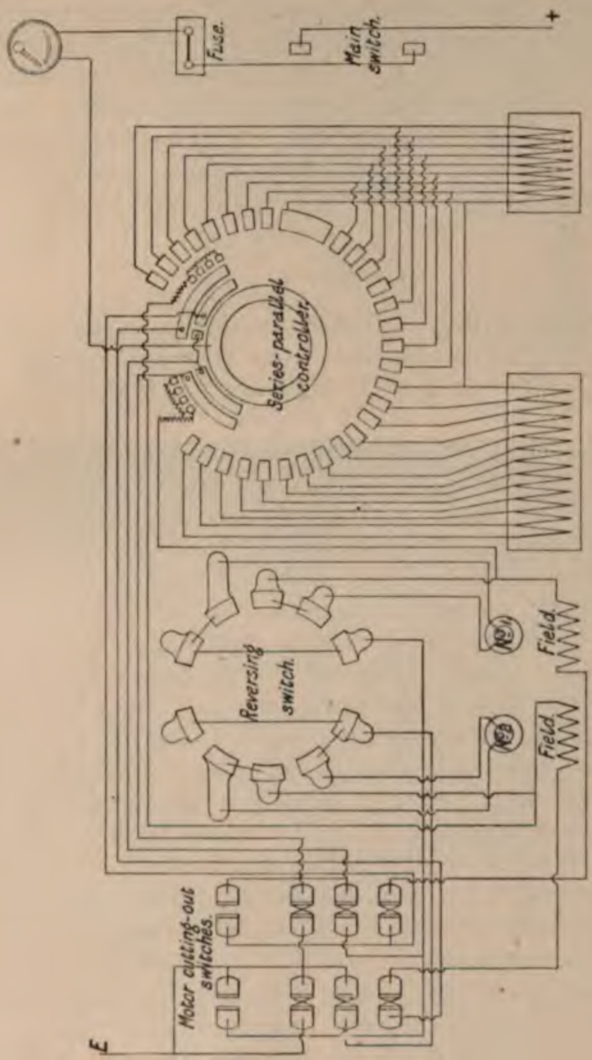


FIG. 61.—Diagram of Connections of Series-Parallel Controller.

circuiting when a motor is cut out of circuit, this contingency being provided for in the switch used for cutting out a motor. The contacts for shunting the field at starting are shown alongside the parallel contacts.

This controller was first used on No. 3 locomotive in the latter part of 1897, and has since given excellent results, having run over 22,000 miles in everyday use. It is fitted to No. 20 locomotive, No. 19 being fitted with the Electric Construction Co.'s controller, similar to the type used on the Liverpool Overhead Railway. The reversing switch which reverses the direction of the current in the armatures is quite independent of the series-parallel controller, and is mounted on a separate base; there is no mechanical interlocking, as in practice it has not been found necessary. A main switch and fuse is also fitted on the same board.

COASTING.

In comparing the calculated "performance diagrams" with those actually obtained from tests on the line, a difference in the method of applying the brakes will be noticed. In the calculated diagrams the brakes were assumed to be applied at the instant the current was shut off, this assumption making the calculation easier. This method is not carried out in practice, as the current is shut off earlier and the locomotive allowed to coast before the brakes are applied. The amount of coasting allowable varies with the gradients in the section.

While on this point it may be interesting to give the result of two round trips with No. 19 locomotive. The first run is that plotted in Figs. 53 and 54. The time taken for the complete journey was ·396 hour, and the average speed 15·86 miles per hour; the energy consumed was 13·32 kilowatt-hours, or ·0552 kilowatt-hour per "ton-mile." In the second run the current was shut off earlier in each section, with the result that the time taken for the complete journey was ·41 hour, and the average speed 15·35 miles per hour; the energy consumed was 12·214 kilowatt-hours, or ·0507 kilowatt-hour per ton-mile. The average speed in the second case was 3·2 per cent. and the energy consumption was 8·15 per cent. less than in the first case.

On the up journey there is a continuous down gradient to the Elephant station, and from the Elephant to Stockwell a continuous up gradient. The average speed from Stockwell to the Elephant in the first and second trips was practically the same (the difference being less than 1 per cent.), while

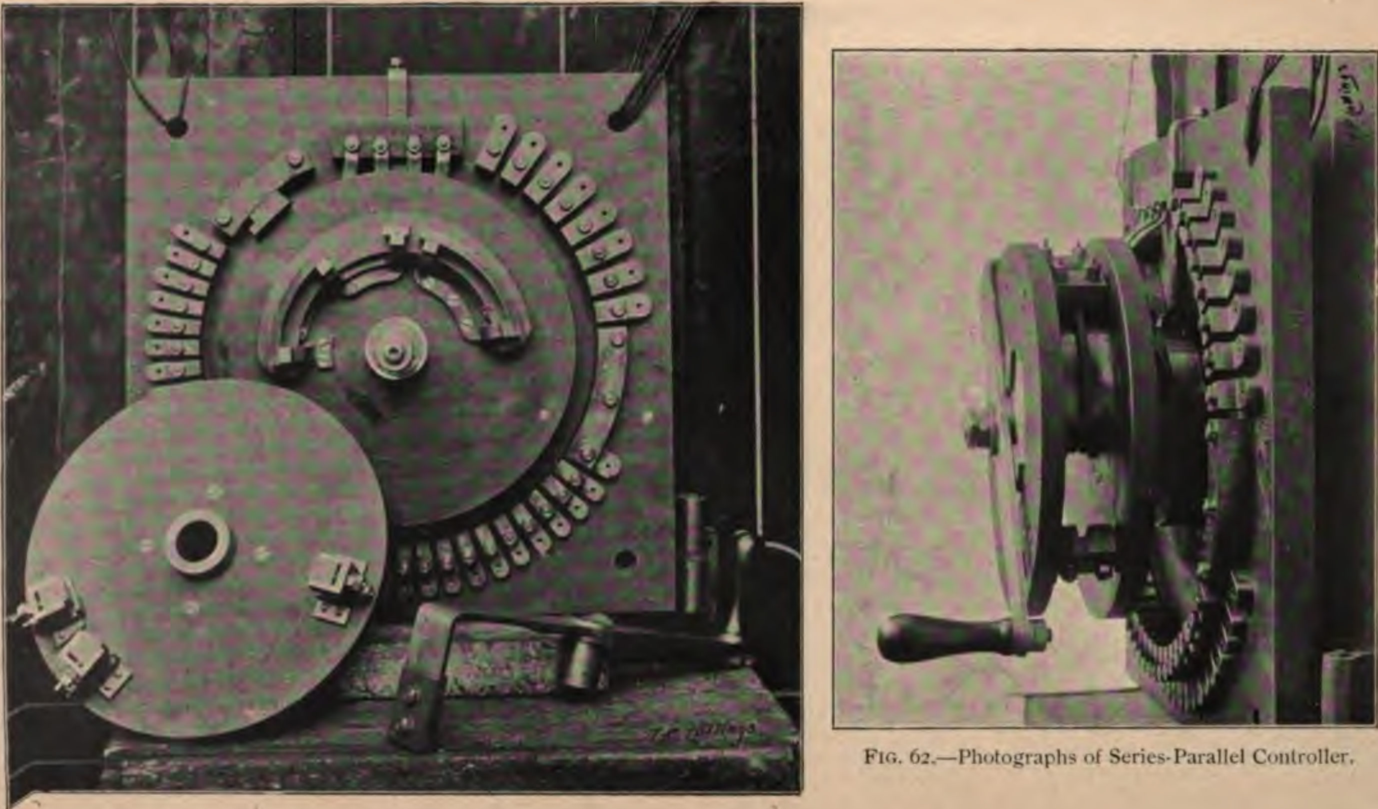


FIG. 62.—Photographs of Series-Parallel Controller.

there is a decrease of 10·95 per cent. in the energy consumption in favour of shutting off the current earlier.

On the down journey from the Elephant to Stockwell the average speed on the first trip was 15·3 miles per hour, and on the second 14·5 miles per hour, showing a decrease of 5·2 per cent., while the energy consumed exhibits a decrease of only 8·25 per cent. in favour of coasting. It will be seen from this that the advantages of coasting on an up grade are not comparable with those to be obtained by following this method on a down grade.

Rapid Acceleration.

The author is aware that rapid acceleration for short sections is very desirable, but in his opinion a point can be reached long before the tractive force becomes equal to the adhesion, when it will not pay.

Apart from its influence on the comfort of passengers, very rapid acceleration necessitates the use of very powerful and heavy motors, and consequently leads to an increase in the ratio of the weight of the empty- to that of the fully-loaded train. Moreover, the starting current is increased several-fold when rapid acceleration is used, and, of course, with it the cost of feeders and generating plant.

For the same section the kilowatts per "ton-mile" will be decreased, but the ratio of the average to the maximum demand upon the generating station will increase, and unless a very careful time-table is followed in starting the trains, the generating plant must work on the average considerably under-loaded. This remark does not apply with such force where a very large number of trains are running as where only a small number are in service. Again, with a rapid acceleration, the wear and tear of the rolling stock will be heavier.

The following example, worked out for a four-coach train and locomotive weighing 49 tons, running in a section 2,700 feet long, will be considered. The time to run the section is taken the same as before, viz., 130 seconds, giving an average speed of 14·25 miles per hour. In the first and second cases "performance diagrams" are worked out for No. 21 locomotive, which has two motors, as already described. In the third case a locomotive is taken with

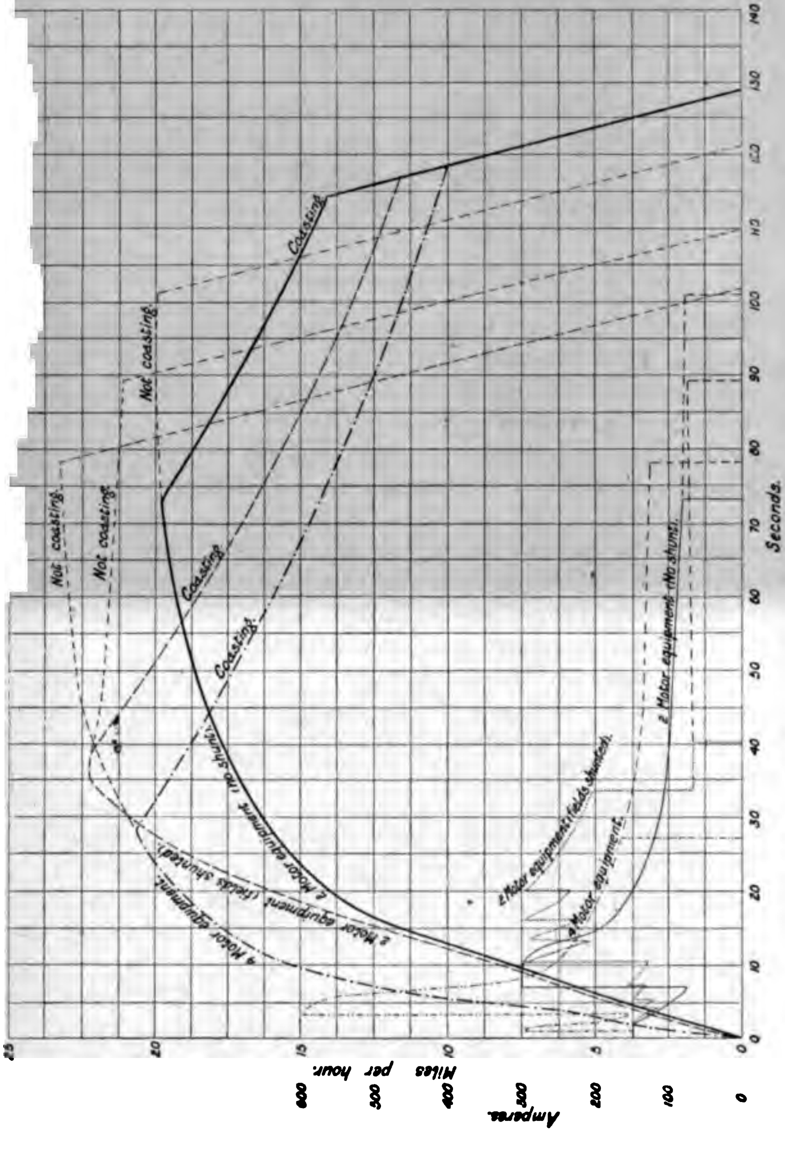


FIG. 63.—Performance Diagrams, 2- and 4-Motor Equipments.

four motors exactly similar to those of No. 21 locomotive; the seating capacity is, however, less, as the same total weight is taken, viz., 49 tons. The three "performance diagrams" are plotted in Fig. 63.

In the first case (2-motor equipment, no shunt) the speed-curve shows that a speed of 12·5 miles per hour would be attained in 16 seconds, which is equivalent to a uniform acceleration of 1·14 feet per second per second. After this point is reached the speed-curve bends over rapidly, a speed of 19·75 miles per hour being attained 74 seconds after starting; at this point the current is switched off and the locomotive and train are allowed to coast for 41·5 seconds; the brakes are then applied, and the run is completed in 130 seconds, the energy consumption being 1,360 watt-hours, or ·0540 kilowatt per ton-mile.

In the second case the starting current is limited to 150 amperes as before, and when the value of the current flowing begins to drop the fields are successively shunted until a speed of 8 miles per hour is attained; the motors are placed in parallel at this point, and the current is limited to 300 amperes, the fields are shunted as when in series, and when a speed of 22 miles per hour is reached the shunt is disused. Here the speed-curve bends over quickly and begins to drop, the motors being then in parallel with their full fields. At 40·75 seconds from starting the current is switched off, and the locomotive and train are allowed to coast for 67·25 seconds; the brakes are then applied, and the run is completed in 130 seconds as before. From the curve it will be noticed that a speed of 17·5 miles per hour is attained in 12 seconds after starting from rest, and this is equivalent to a uniform acceleration of 2·13 feet per second per second. The energy consumed for the run is 1,081 watt-hours, or ·0430 kilowatt-hour per ton-mile.

We will next consider the third case, with the four-motor equipment. Starting with the four motors in series and 150 amperes as before, at 2 miles per hour the motors are placed two in series and these in parallel, the current being limited to 300 amperes. At 6 miles per hour the four motors are placed in parallel and the current is limited to 150 amperes per motor, or a total of 600 amperes. This peak quickly falls, and in 5 seconds from placing the four

motors in parallel the current drops to 300 amperes. At 27·5 seconds after starting the current is shut off, coasting commences and continues for 92·5 seconds, the brakes being then applied, and the run completed in the required 130 seconds. The energy consumed is 946 watt-hours, or ·0377 kilowatt-hour per ton-mile. In 9 seconds from starting a speed of 15 miles per hour is attained, corresponding to a uniform acceleration of 2·43 feet per second per second. From 15 to 20·5 miles per hour the speed-curve bends over more quickly than in the second case considered, but if the fields had been shunted the acceleration could have been maintained uniform until a higher speed was attained.

If in the above cases coasting were not resorted to, but the current were kept on until the brakes were applied, and the negative acceleration in each case were 1·46 feet per second per second, the respective times required to run the 2,700 feet section would have been 122, 111, and 103 seconds, the average speed would have been 15·07, 16·0, and 17·87 miles per hour, and the energy consumed ·0659, ·0628, and ·0745 kilowatts per ton-mile.

Having arrived at the effect of rapid acceleration upon the energy consumed per "ton-mile," we will now consider its effect upon the capacity of the generating station, feeders, &c.

By way of illustration let us assume ten sections of 2,700 feet each, and allow 10 seconds at each station. Take as a basis a 2-minute service, or 30 trains per hour leaving the terminal station. With the four-motor equipment, the seating capacity would be something like 20 per cent less than with the two-motér equipment, on account of the weight of the train and locomotive being taken as the same in each case.

Adopting the first "performance diagram" considered, viz., the two-motor equipment (coasting), as the standard, the following comparison is interesting and instructive. The time required to run the ten sections is 23·15 minutes, including stopping at stations. The maximum output of the generating station would be 2,715 amperes, and the average about 810. Thus the average is about 29 per cent. of the maximum. With the four-motor equipment (coasting), where we get a high rate of acceleration, and a low energy-consumption per ton-mile, the time to complete the run *is the* same, but the maximum output, arrived at as before,

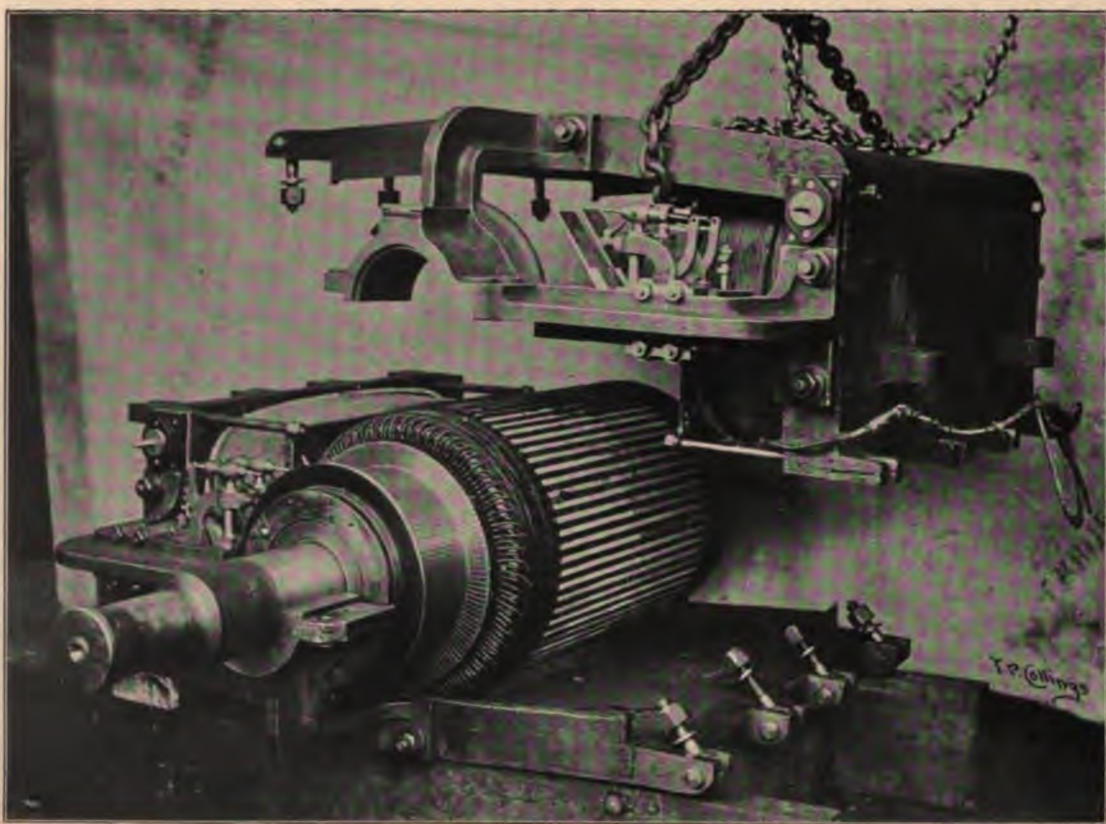


FIG. 64.—Photograph of No. 21 Locomotive Motor.

average about 942, bringing the ratio of average to maximum up to 39·6 per cent.

If the four-motor equipment were used under similar conditions, the time would be 18·67 minutes, or about 20 per cent less than that taken by the two-motor equipment (coasting). The maximum output for this set of conditions would be 3,407 amperes, and the average about 1,118, the ratio of average to maximum here being 32·8 per cent.

In practice the brakes would not be applied immediately the current was shut off, but to compensate for this, shunting the fields at starting and allowing a small amount of coasting for both equipments could be resorted to.

From the above considerations it would seem that to adopt very rapid acceleration at starting to reduce the energy consumed per "ton-mile" is uneconomical in working, and that the first cost of both generating and locomotive plant is about doubled. If, on the other hand, it is necessary to increase the average speed, a medium acceleration at starting will pay better than very rapid acceleration.

Exception may be taken to the author basing so many conclusions on theoretical reasoning. To show that at least some of the conclusions are correct, the calculated and observed "performance diagrams" for No. 3 locomotive on the Stockwell-Oval and Oval-Kennington sections are given in Figs. 65 and 66. The calculated curves are shown by the solid line, and those derived from observation by the broken line. The difference between the ampere curves is to a great extent accounted for by the way in which the series-parallel controller was operated. The method of shunting the motor-fields at starting may also be criticised, and to test the accuracy of the suggestion, tests on the line were made with No. 19 locomotive, the fields being shunted at starting as recommended in the first part of this paper.

In the experiment the fields were shunted in the parallel position only, it being more convenient to arrange the temporary shunts in this manner. Fig. 67 shows the result on the Oval-Kennington section where there is a down gradient, and Fig. 68 was obtained on the Elephant-Boro' section where there is an up gradient of about 1 in 70 after *leaving* the Elephant. In each figure the solid curves show

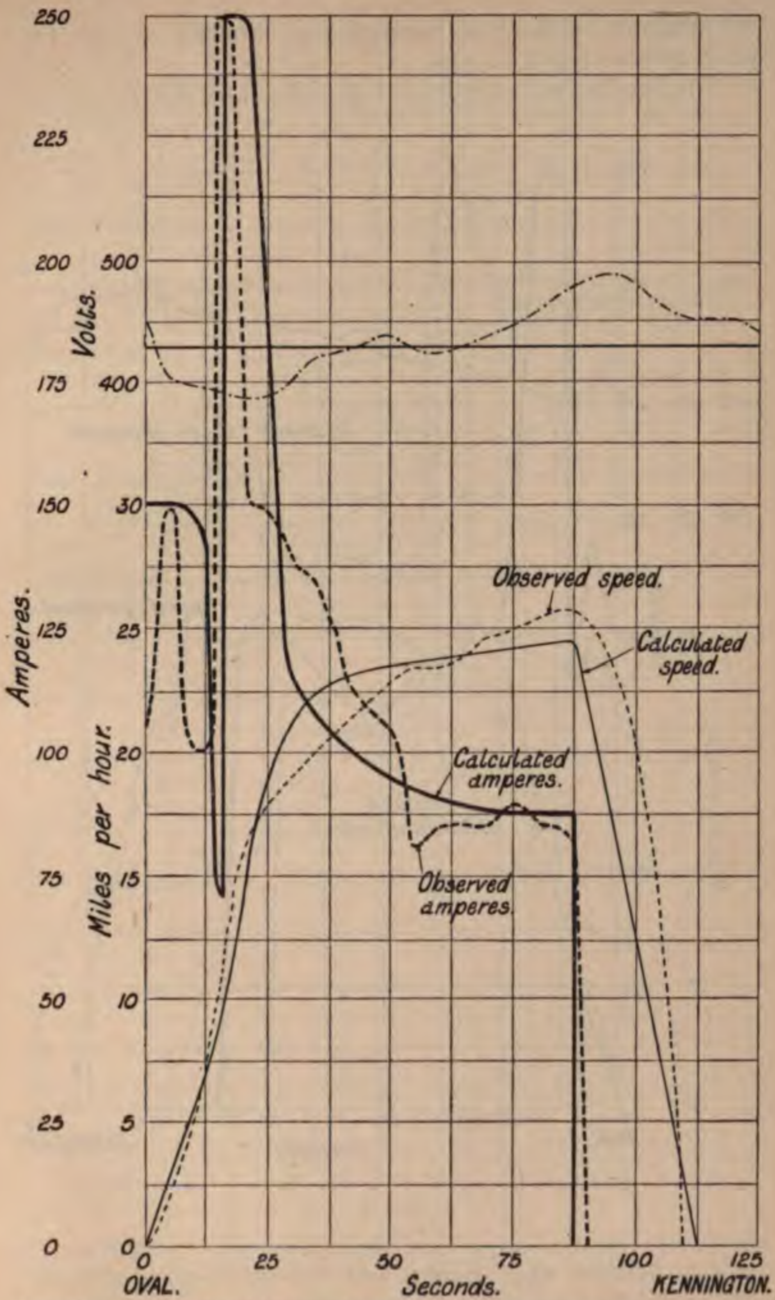


FIG. 66.—Calculated and Observed Performance Diagrams, Locomotive No. 3, Oval-Kennington Section.

the ordinary method of working, and the broken line the effect of shunting the fields.

In conclusion the author begs to tender his thanks to

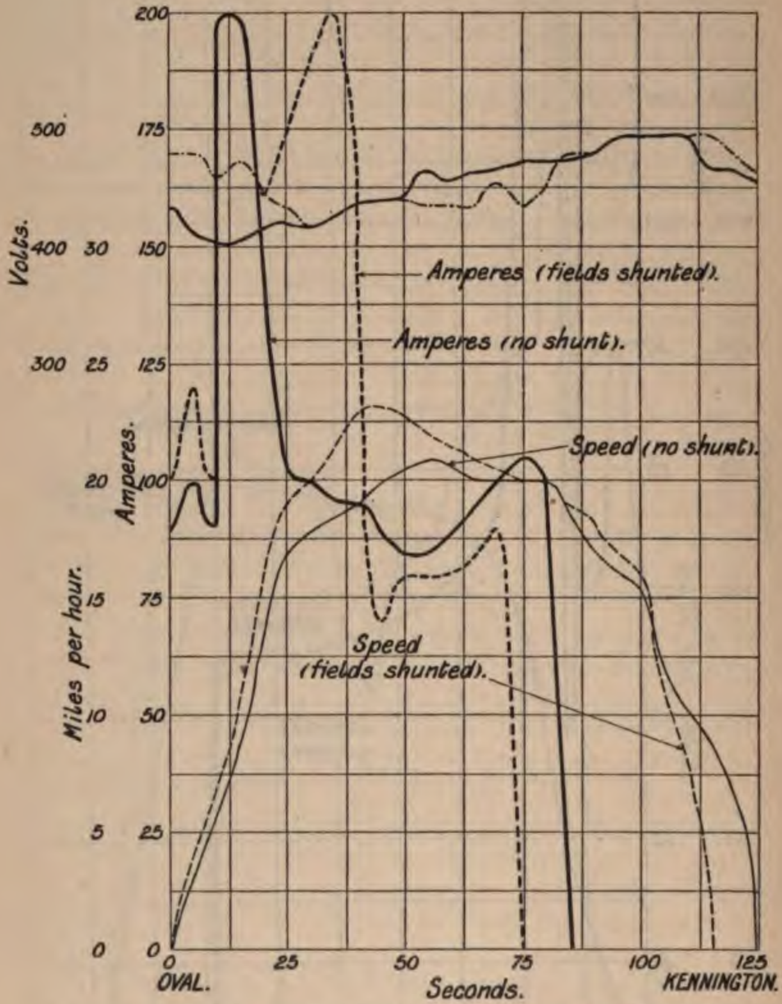


FIG. 67.—Effect of Shunting Motor fields at Starting, Oval-Kennington Section.

Messrs. Rheam and Marsh, and the other assistants, for their valuable help in carrying out the tests and experiments *given in* the paper.

SUMMARY.

1. To determine the draw-bar pull exerted by the various locomotives and arrive at the tractive resistance per ton of

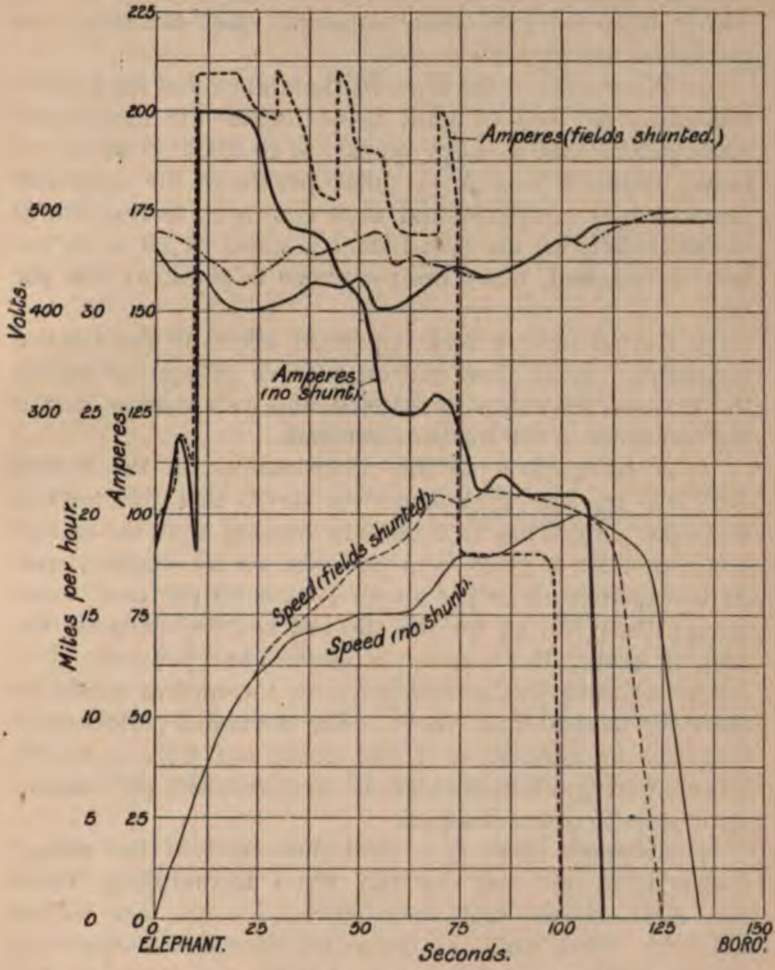


FIG. 68.—Effect of Shunting Motor fields at Starting, Elephant-Borough Section.

train hauled, the dynamometers described were designed and constructed.

2. The motor and locomotive tests show that at starting only about 73 per cent. of the total tractive effort at the

tread of the wheel is given out at the draw-bar, and that the loss at starting is directly proportional to the tractive effort.

3. After the locomotive commences to move, its losses decrease until a speed of about 9 miles per hour is reached, which then becomes fairly constant. But the losses are greater for the higher currents.

4. The results of the draw-bar tests show that the tractive resistance per ton of train is 40 lbs. at the moment of starting, and that it drops quickly to 10 lbs. at 6 miles per hour; between 6 and 13 miles per hour the resistance remains fairly constant, and then continues to rise almost proportionally to the speed until a speed of 26 miles per hour is reached, when the resistance is about 21 lbs. per ton.

5. Curves have a very important effect on the tractive resistance. Tests show that on a curve of 540 feet radius the tractive resistance is doubled, and with sharper curves the resistance is still further increased.

6. A comparison of the performances on the line of Nos. 12, 15, and 17 locomotives shows that the tractive force per ampere has an important bearing upon the energy consumed for a given section. No. 12 locomotive, with its low tractive force per ampere, takes 26 per cent. more energy than No. 15 for the shortest section, while on the longest section this increase is reduced to 6 per cent. For longer sections still, probably No. 12 locomotive would be more economical than No. 15. The calculated performance diagrams for sections of 2,700, 4,000, and 5,280 feet with motors having different values of tractive effort per ampere again goes to prove this fact.

7. Although there is a great difference in the energy drawn from the line by the three locomotives, Table No. XIII. shows that their efficiencies are not widely different. This table also shows the alteration in efficiency of No. 3 by re-winding and adopting the series-parallel method of control.

8. With plain series method of control the value of the starting current has not much effect on the total energy consumed for a given section, as shown by the experiments with No. 15 locomotive, Table I. With the series-parallel *method* there is, however, a very great reduction in the

energy consumed by adopting high-starting currents. The calculated performance diagrams, Figs. 32 and 33, show that by altering the value of the starting current when the motors are placed in parallel there is a saving of 24 per cent. in time and 11·5 per cent. in watt-hours.

9. Diagram Fig. 41 shows that theoretically a higher rate of acceleration at starting is the most economical method of running a short section. The highest rate taken is 1·46 feet per second per second, the lowest that it was possible to adopt being 0·417 feet per second per second. The latter rate requires 27·6 per cent. more energy than the former to run a 2,700 feet section.

10. Diagram Fig. 41 shows that the acceleration at starting should be uniform, and to arrive at this in the calculated performance diagram the motor fields were successively shunted during the starting period. The performance diagrams, Figs. 40 and 42, show the effect of shunting the field during the running and starting periods. For the same motor running a 2,700 feet section there is a gain of over 8 per cent. in favour of the latter method. Experiments with No. 15 locomotive show that by shunting the field during the running period the energy consumed was increased by about 50 per cent. without corresponding gain in time. Figs. Nos. 67 and 68 show the effect of shunting the field of No. 19 locomotive at starting.

11. By trying different numbers of conductors and armature lengths the kilowatt-hours consumption for a 2,700 feet section was reduced from 1,905 to 1,330, as shown by the various performance diagrams and Table XIV. The nett result of the investigation resulted in the design of No. 21 locomotive.

12. Calculation (Fig. 63) and tests show that a high rate of acceleration and a fair amount of coasting is a more economical method of running a short section than very rapid acceleration, necessitating the use of large currents for a few seconds and coasting for a large percentage of the time. The reduction in kilowatts per ton-mile gained by very rapid acceleration is deceptive, and must be considered in connection with increased cost of plant and the cost per kilowatt-hour.

TABLE I.

	Borough to Elephant and Castle.					Elephant and Castle to Kennington.					Kennington to Oval.					Oval to Stockwel[l]				
Starting Current, Amperes	100	120	140	160	180	100	120	140	160	180	100	120	140	160	180	100	120	140	160	180
Miles per Hour Starting	16·0	16·0	16·5	16·5	17·0	13·0	14·0	14·5	14·0	14·75	13·0	13·5	13·5	83·5	13·75	15·0	14·5	15·0	15·5	15·5
Time to Attain, in Seconds	40	35	30	35	35	60	50	45	40	45	50	50	40	35	40	40	35	30	35	30
Running Period, Seconds	80	80	85	80	75	85	85	85	90	85	10	100	100	110	100	170	185	180	185	Signal against.
Total Time of Current in Seconds	120	115	115	115	110	145	135	130	130	130	155	150	140	145	140	210	220	200	220	
Average Speed, Running Period	15·62	15·19	15·9	15·82	15·7	14·1	14·62	15·15	14·5	15·15	13·35	13·1	13·5	13·1	13·87	13·25	12·5	12·86	12·5	
" Current " "	41·4	46·4	47·6	42·1	39·0	50·7	47·4	47	44·7	46·4	55·4	52·2	50·0	51·1	56·9	57·4	60·0	55·6	56·0	
" Volts " "	428	425	440	423	415	426	426	429	430	432	426	411	418	400	447	454	422	413	401	
" Current, Starting Period	81·0	87·4	95·6	92·8	100·0	84·4	101·2	102·5	107·8	113·5	87·6	88·7	98·5	105	110	87·5	96·4	108·3	96·4	121
Starting Period, Watts	34,500	37,200	40,750	39,550	42,640	35,250	43,200	43,750	46,000	48,200	37,600	37,540	41,640	44,500	46,600	37,400	41,200	45,900	41,200	51,100
" " Watt-hours	383·3	333·4	339·5	384·5	414·5	587·5	600·0	547·0	511·0	602·0	515·2	520·0	462·6	432·6	517·7	415·5	400·5	382·5	400·5	-
" " Board of Trade Units ..	·383	·333	·339	·384	·414	·587	·600	·547	·511	·602	·515	·520	·462	·432	·517	·415	·400	·382	·400	Signal against.
Running " Watts	17·650	19,750	20,220	17·940	16,600	21,680	20,290	20,100	19,150	19,860	23,260	21,900	21,000	21,410	23,890	24,520	25,610	23,760	23,920	
" " Watt Hours	392·2	438·2	478·0	397·0	356·0	512·0	479·0	474·0	479·0	469·0	678·0	609·0	584·0	657·0	661·0	1158·0	1336·0	1188·0	1230·0	
" " Board of Trade Units ..	·392	·438	·478	·397	·356	·512	·479	·474	·479	·469	·678	·609	·584	·657	·661	1·158	1·336	1·188	1·230	
Total Board of Trade Units	·7755	·7755	·8175	·7815	·7705	1·097	1·079	1·021	1·006	1·071	1·193	1·129	1·046	1·089	1·178	1·573	1·736	1·570	1·630	
Time in Seconds to attain 11 miles per hour	26·0	22·0	17·5	17·0	15·0	48·0	37·0	30·0	27·0	27·5	35·0	32·5	24·0	22·5	22·5	28·5	26·0	21·0	20·0	20·0
" " " 9 miles per hour	23·5	18·75	16·0	15	13·0	40·0	30·0	21·5	18·5	20·0	28·75	25·0	17·5	17·5	17·5	26·0	23·0	18·0	17·0	17·0

TABLE II. (No. 15 Locomotive.)

Current in Amperes.	Draw-bar Pull at Dynamometer in lbs.	Tractive Effort due to acceleration ± gravity in lbs.	Tractive Effort at Draw Bar at constant speed on level in lbs.	Time from switching on Current in seconds.	Speed of Loco. in miles per hour.	
105	1340	0	1340	0	0	Starting from Kennington Station.
105	1075	201	1276	5	·75	
100	1075	345	1420	10	2·0	
100	1050	417	1467	15	3·5	
100	1050	546	1596	20	5·5	
100	1050	546	1596	25	7·5	
99	1100	316	1416	30	9·0	
100	1220	0	1230	0	0	Starting from Elephant and Castle Station.
100	975	162	1137	5	·5	
95	875	201	1076	10	1·25	
100	820	472	1292	15	2·25	
100	900	590	1490	20	3·5	
105	1150	590	1740	25	4·75	
100	1275	664	1939	30	6·25	
100	1275	590	1865	35	7·5	
120	1700	0	1700	0	0	Starting from Kennington Station.
120	1275	346	1621	5	1·0	
115	1275	590	1865	10	3·0	
115	1475	547	2022	15	5·0	
120	1475	547	2022	20	7·0	
120	1700	0	1700	0	0	Starting from Elephant and Castle Station.
120	1370	245	1615	5	·9	
115	1150	634	1784	10	2·75	
115	1150	692	1842	15	4·3	
120	1375	776	2151	20	6·2	
120	1500	692	2192	25	7·75	
120	1600	734	2334	30	9·0	

TABLE III. (No. 12 Locomotive.)

	Draw-bar Pull at [Dy]namometer in lbs.	Tractive Effort due to acceleration ± gravity in lbs.	Tractive Effort at Draw Bar at constant speed on level in lbs.	Time from switching on Current in seconds.	Speed of Loco. in miles per hour.	
	1460		1460	0	0	No. 3 Train starting from Elephant and Castle Station.
	1450	310	1760	5	1·5	
	1240	400	1640	10	3·25	
	1240	530	1770	15	4·75	
135	1240	530	1770	20	6·25	
140	1450	480	1930	25	7·5	
	1460		1460	0	0	No. 12 Train starting from Elephant and Castle Station.
[illegible]	1350	260	1610	5	1·0	
	1375	260	1635	10	2·0	
[illegible]50	1425	475	1900	15	3·0	
	1475	475	1950	20	4·75	
	1575	475	2050	25	6·0	
140	1575	475	2050	30	7·25	
140	1460		1460	0	0	No. 3 Train starting from Kennington Station.
140	1400	200	1600	5	1·0	
145	1440	350	1750	10	2·5	
140	1350	520	1870	15	4·75	
140	1400	600	2000	20	8·0	
140	1460		1460	0	0	No. 12 Train starting from Kennington Station.
135	1350	100	1450	5	·6	
140	1440	220	1660	10	1·5	
140	1500	320	1820	15	3·0	
135	1440	425	1865	20	5·0	
145	1440	425	1865	25	7·0	

TABLE IV. (No. 15 Locomotive.)

Current in Amps.	Draw-bar Pull at Dynamometer.	Tractive Force from Prony Brake Test.	Traction Force due to acceleration ± gravity	Tractive Force at Draw Bar at constant speed on level.	Average speed in miles per hour.	Duration of Test in seconds.	Section.
34·2	283	300	6·9	277·6	17.7	75	Oval to Kennington
36·8	268	340	8·3	259·7	18·4	105	Stockwell to Oval
43·0	428	480	10·0	438	15·5	45	Elephant to Kennington
48·6	406	620	24·8	430·8	15·6	40	„ „
52·2	488	720	97·0	585	13·1	100	Kennington to Oval
55·4	500	800	122·7	622·7	13·5	105	„ „
57·4	530	850	171	701	13·23	170	Oval to Stockwell
60	570	910	168	738	12·56	185	„ „
60	600	910	173	773	10·25	Instantaneous values taken from analysis of curves.	Elephant to Kennington
70	830	1160	230	1060	12·0		„ „
75	850	1300	288	1138	9·0		„ „
75	825	1300	417	1242	11·0		Kennington to Oval

TABLE V.

Speed in miles per hour.	Speed Test per second.	$\frac{v_1+v_2}{2}$	$s = vt$ in feet.	Total feet.	Accelera-tion Feet per sec. per sec.	Mass × Accelera-tion.	Number of Reading	Change of Gradient.
0	...	...	...	...	...	...	1	...
1	1·46	·73	3·66	3·66	·293	+ 469	2	...
3	4·39	2·92	14·64	18·30	·624	+ 1013	3	...
5	7·3	5·84	29·22	47·53	·582	+ 950	4	...
7	10·22	8·76	43·80	91·33	·582	+ 950	5	4·8
9	13·13	13·17	65·87	157·2	·582	+ 950	6	...
10·5	15·30	14·21	71·07	228·2	1·016	+ 1655	7	...
12·0	17·5	16·4	82·0	310·2	·44	+ 719	8	8·26
12·75	18·6	18·06	90·3	400·5	·224	+ 366	9	...
13·0	19·0	18·81	94·05	494·6	·076	+ 124	10	...
13·5	19·7	19·35	96·75	591·3	·140	+ 229	11	...
13·5	19·7	19	98	689·8	0	0	12	...
13·75	20·1	19·9	99·5	789·3	·080	+ 130	13	...
14·0	20·4	20·26	101·3	890·6	·062	+ 101	14	14·11
14·0	20·4	20·3	102·1	992·7	0	0	15	...
13·5	19·7	20·0	100·3	1093	·144	— 235	16	16·36
13·0	19·0	19·35	96·7	1190	·144	— 235	17	...
13·0	19·0	19·0	95·0	1285	0	0	18	...
13·0	19·0	19·0	95·0	1380	0	0	19	19·4
12·75	18·6	18·8	94·0	1474	·076	— 124	20	...
12·5	18·2	18·4	92·2	1567	·068	— 124	21	...
12·0	17·5	17·8	89·4	1656	·156	— 254	22	22·24
12·0	17·5	17·5	87·5	1744	0	0	23	...
12·5	18·28	17·8	89·4	1833	·156	+ 254	24	...
13·0	19·0	18·6	93·2	1926	·144	+ 235	25	...
13·5	19·7	19·35	96·6	2023	·114	+ 235	26	..
13·5	19·7	19·7	98·5	2121	0	0	27	...
13·75	20·1	19·9	99·5	2221	·076	+ 124	28	...
13·75	20·1	20·1	100·5	2321	0	0	29	...
13·75	20·1	20·1	100·5	2422	0	0	30	30·44
13·75	20·1	20·1	100·5	2522	0	0	31	...
13·0	19·0	19·55	97·7	2620	·22	— 359	32	32·19
12·0	17·5	18·25	91·2	2711	·30	— 490	33	...
8·0	11·7	14·6	72·9	2784	1·19	— 1955	34	...
0	0	5·8	29·2	2804	2·33	— 3819	35	...

TABLE VI. (No. 12 Locomotive.)

Current in Amperes.	Draw-bar Pull at Dynamometer.	Tractive Force from Prony Brake Test.	Tractive Force due to acceleration ± gravity	Tractive Force at Draw Bar at constant speed on level.	Average speed in miles per hour.	Duration of Test in seconds.	Section.
38·75	225	360	54·5	170·3	19·5	45	Stockwell to Oval
43	310	410	14·8	295·2	18·2	100	" "
46·2	308	485	19·36	288·6	17·1	65	Oval to Kennington
65·9	540	780	88·0	628	14·7	...	Kennington to Oval
67·1	645	810	91·5	736	13·45	95	" "
69·1	625	850	114	739	14·45	150	Oval to Stockwell
74·2	720	940	130	850	13·48	165	" "
80	760	1040	179	939	14·0	35	" "
62	525	750	93	615	14·0	45	" "
132	1550	1890	128	1422	9·0	10	" "
68	560	840	134	694	14·5	50	" "
60	450	720	23	470	15·5	50	Borough to City
127	1240	1800	373	1613	10·5	30	Elephant to Borough
126	1250	1785	394	1644	10·5	30	" "
55	450	640	...	450	15·5	25	Borough to City

TABLE VII. (No. 17 Locomotive.)

Current in Amperes.	Draw-bar Pull at Dynamometer.	Tractive Force from Prony Brake Test.	Tractive Force due to acceleration ± gravity	Tractive Force at Draw Bar at constant speed on level.	Average speed in miles per hour.	Duration of Test in seconds.	Section.
37	380	360	46	334	16·5	20	Borough to Elephant
45	475	500	23·4	498·4	14·25	20	Kennington to Oval
48·5	350	565	54	404	14·0	30	Oval to Stockwell
48·75	495	570	86	581	14·25	40	Borough to Elephant
49	520	580	51·9	572	13·5	50	City to Borough
49	400	580	31	431	13·75	55	" "
52	415	640	72	487	13·5	37·5	Kennington to Oval
53	520	660	108	628	14	30	" "
54	460	680	99	559	13·25	40	" "
56	450	720	128	578	13	27·5	" "
56	440	720	119	459	14	30·5	Oval to Stockwell
57·5	570	740	151	721	14	30	Kennington to Oval
67·5	530	925	186	716	12·5	50	Oval to Stockwell
73·5	750	1040	182	932	12·0	15	" "
100	1200	1540	71	1271	8·0	10	Elephant to Kennington
102	1220	1580	225	1445	14·0	10	Oval to Stockwell
120	1440	1920	283	1683	11·0	7·5	" "
120	1550	1920	192	1742	7·5	15	Elephant to Kennington

TABLE VIII.

Speed in miles per hour.	Tractive Force for Train in lbs.				Average of Four Trials.	Average Tractive Force per ton on level in lbs.
	Starting from Elephant. 120 amps.	Starting from Elephant. 100 amps.	Starting from Kennington. 120 amps.	Starting from Kennington. 100 amps.		
0	1050	950	950	975	981	42·0
1	870	650	800	620	742·5	31·5
2	400	375	450	475	425	18·1
3	300	275	300	350	308	13·1
4	375	300	350	250	319	13·5
5	400	350	440	150	335	14·2
6	250	175	450	100	244	10·2
7	260	175	440	100	244	10·2
8	380	175	360	240	289	12·3
9	530	100	220	425	269	11·4
10	...	300	...	325	312	13·3

TABLE IX.

…verage	Drawbar pull at Dynamometer in lbs.	Tractive force due to acceleration ± gravity in lbs.	Duration of Test in seconds.	Average Tractive force per ton on level in lbs.	No. 15 Locomotive. Section of Line.	Train number.
·1	488	168	100	13·6	Kennington to Oval	No. 12 Train
2·56	570	292	185	11·8	Oval to Stockwell	„ 12 „
·35	500	213·5	105	11·75	Kennington to Oval	
13·2	530	295	170	9·95	Oval to Stockwell	
16·8	275	26	105	12·8	Stockwell to Oval	
18·4	268	14·5	105	12·0	„ „ „	No. 5 Train
17·7	283	12·0	75	12·5	Oval to Kennington	„ 5 „
15·86	none	407	65	11·0	Stockwell to Oval	„ 7 „
16·5	„	449	65	12·1	„ „ „	„ 7 „
16·6	„	322	60	11·0	„ „ „	„ 14 „ (2 coaches)
18·84	367	43	85	17·5	„ „ „	„ 1 „
20·7	346 ?	30·3	65	14·05	Oval to Kennington	„ 1 „
19·9	425	24·3	100	17·0	Stockwell to Oval	„ 7 „
19·8	428	35	70	16·7	Oval to Kennington	„ 7 „
22·6	325	72	40	16·9	Stockwell to Oval	„ 1 „
22	390	80	45	20·0	„ „ „	„ 7 „

TABLE X.

Speed, miles per hour.	Starting from Elephant. No. 3 Train.		Starting from Kennington. No. 3 Train.		Starting from Elephant. No. 12 Train.		Starting from Kennington. No. 12 Train.		Average Tractive Force per ton on level.
	Total.	Per ton.	Total.	Per ton.	Total.	Per ton.	Total.	Per ton.	
1	2	3	4	5	6	7	8	9	10
0	1240	47·8	1240	47·8	1300	45	1350	46·6	46·8
1	800	30·8	850	31·5	700	24·3	925	32	29·6
2	550	21·2	720	26·7	650	22·5	780	27	19·4
3	250	9·3	500	18·5	650	22·5	610	21·15	17·8
4	230	8·5	260	9·6	610	21	410	14·2	13·3
5	270	10·4	130	4·8	520	18	275	9·5	10·6
6	100	3·7	100	3·7	400	13·85	260	9	7·5
7	150	5·8	120	4·45	280	9·7	235	8·5	7·1
8	270	10·4	125	4·64	240	8·3	200	6·95	7·5

TABLE XI.

Speed, miles per hour.	Draw-bar Pull.	Tractive Force due to accel. ± gravity in lbs.	Duration of Test in seconds.	Average Tractive Force per ton on level in lbs.	Weight of train hauled, passengers included. Tons.	No. 12 Locomotive.	
						Section of line.	Train number.
14·75	540	226	95	11·8	26·6	Kennington to Oval	No. 3 Train
14·5	625	348	150	11·36	24·37	Oval to Stockwell	No. 3 „
13·45	645	256	95	13·45	28·9	Kennington to Oval	No. 12 „
13·5	720	336	165	14·4	26·6	Oval to Stockwell	No. 12 „
18·2	310	35	100	12·75	24·37	Stockwell to Oval	No. 13 „
19·5	225	128	45	14·5	24·37	„ „	No. 13 „
17·7	308	51	65	13·17	27·25	Oval to Kennington	No. 13 „

TABLE XII.

Section	Number of Locomotive.	Standing Period. Amperes	Standing Period. Time	Running Period. Amperes	Running Period. Time.	Average Amperes for Section at 430 volts.		Total Time Current on.		Total Time in Section, Start to Stop.		Increased Energy fo Section, No. 15 as Standar
	15	96	45	31	120	48·75	51·27	165	162·5	185	182·5	...
	15	91·6	45	39·1	115	53·8		160		180		
	17	106	30	37·2	135	49·7	50·3	165	162·5	185	185	1·5 % (Decrea
	17	96·4	35	38·5	125	51·0		160		185		
	12	96	45	47·7	110	61·6	62·4	155	147·5	180	180	9·54 %
	12	114	40	43	100	63·2		140		180		
	15	85·6	35	36	75	51·7	53·55	110	110	130	130	...
	15	96·2	30	39·9	80	55·4		110		130		
	17	112·4	25	35·6	75	54·8	54	100	107·5	120	127·5	1·5 % (Decrea
	17	107	25	38·2	90	53·2		115		135		
	12	102	45	43·2	55	69·75	69·6	100	102·5	130	130	21·25 %
	12	108	40	46	65	69·5		105		130		
Kennington to Elephant, about 2,800 feet.	15	94	35	33·5	80	51·8	52·75	115	117·5	140	140	
	15	86·2	40	37·5	80	53·7		120		140		
	17	133·5	20	38·75	95	55·3	55·9	115	115	125	135	2·1 % (Increa
	17	112·5	25	41·0	90	56·5		115		145		
	12	113	40	51	65	74·8	73·9	105	105	130	132·5	25·1 %
	12	104	45	50	60	73		105		135		
Elephant to Borough, about 2,725 feet.	15	118·7	30	59·2	115	71·5	73·35	145	147·5	165	165	
	15	109·5	40	62·5	110	75·2		150		165		
	17	135·0	25	66·5	115	79	76·6	140	142·5	170	167·5	·46 % (Increa
	17	120·8	30	62·0	115	74·2		145		165		
	12	143	45	87	85	114	104·9	130	130	160	162·5	26 %
	12	136	40	78	90	95·8		130		165		

TABLE XII. (*continued*).

Section.	Number of Locomotive.	Starting Period. Amperes	Starting Period. Time.	Running Period. Amperes	Running Period. Time.	Average Amperes for Section at 430 volts.		Total Time Current on.		Total time in Section, Start to Stop.		Increased Energy for Section, No. 15 as Standard.
Borough to Elephant, about 2,725 feet.	15	96	30	48	85	60·5	59·4	115	115	135	142·5	...
	15	87·5	35	46·5	80	58·3		115		150		
	17	105	25	41·2	85	55·7	56·3	110	117·5	140	145	3·0 % (Decrease)
	17	89·5	35	44·4	90	57·0		125		150		
	12	133	30	58	70	80·5	74·9	100	105	150	147·5	14·85 %
	12	114	30	53·9	80	69·4		110		135		
Elephant to Kennington, about 2,800 feet.	15	102·5	45	47	85	66·5	66·9	130	132·5	145	147·5	...
	15	101·2	50	47·4	85	67·3		135		155		
	17	109·4	40	46·1	80	67·1	64·5	120	125	145	150	9·2 % (Decrease)
	17	97·3	45	43·4	85	62·0		130		155		
	12	120·5	60	79·2	60	99·5	89·5	120	125	150	150	25·4 %
	12	117	50	56	80	79·5		130		150		
Kennington to Oval, about 2,880 feet.	15	98·5	40	50	100	63·9	64·1	140	147·5	160	165	...
	15	88·7	50	52·2	100	64·3		150		170		
	17	91·6	40	50·7	95	62·4	63·2	135	140	165	167·5	4·5 % (Decrease)
	17	95·4	40	52·3	105	64·1		145		170		
	12	120	40	71	95	85·4	83·4	135	135	160	157·5	21·2 %
	12	118	40	66	95	81·4		135		155		
Oval to Stockwell, about 4,000 feet.	15	108·3	30	55·6	180	63	64·45	210	215	240	242·5	...
	15	96·5	35	60	185	65·9		220		245		
	17	104	30	59·4	160	66·4	64·4	190	197·5	230	235	8·2 % (Decrease)
	17	102·2	25	57·0	180	62·5		205		240		
	12	140	30	65·5	165	78	77·95	195	190	235	225	6·13 %
	12	116	35	69	150	77·9		185		215		

TABLE XII.

Section		Amperes	Time	Running Period. Amperes	Running Period. Time	Average Amperes for Section at 430 volts.		Total Time Current on.		Total Time in Section, Start to Stop.		Increase[d] Energy f[or] Section, No. 15 as Standar[d]
		96	45	31	120	48·75	51·27	165	162·5	185	182·5	...
		91·6	45	39·1	115	53·8		160		180		
		106	30	37·2	135	49·7	50·3	165	162·5	185	185	1·5 % (Decrea
		96·4	35	38·5	125	51·0		160		185		
	2	96	45	47·7	110	61·6	62·4	155	147·5	180	180	9·54 %
	2	114	40	43	100	63·2		140		180		
	15	85·6	35	36	75	51·7	53·55	110	110	130	130	...
	15	96·2	30	39·9	80	55·4		110		130		
	7	112·4	25	35·6	75	54·8	54	100	107·5	120	127·5	1·5 % (Decrea
	7	107	25	38·2	90	53·2		115		135		
	2	102	45	43·2	55	69·75	69·6	100	102·5	130	130	21·25 %
	2	108	40	46	65	69·5		105		130		
Kennington to Elephant, about 2,800 feet.	15	94	35	33·5	80	51·8	52·75	115	117·5	140	140	
	15	86·2	40	37·5	80	53·7		120		140		
	17	133·5	20	38·75	95	55·3	55·9	115	115	125	135	2·1 % (Increa
	17	112·5	25	41·0	90	56·5		115		145		
	12	113	40	51	65	74·8	73·9	105	105	130	132·5	25·1 %
	12	104	45	50	60	73		105		135		
Elephant to Borough, about 2,725 feet.	15	118·7	30	59·2	115	71·5	73·35	145	147·5	165	165	
	15	109·5	40	62·5	110	75·2		150		165		
	17	135·0	25	66·5	115	79	76·6	140	142·5	170	167·5	·46 % (Increa
	17	120·8	30	62·0	115	74·2		145		165		
	12	143	45	87	85	114	104·9	130	130	160	162·5	26 %
	12	136	40	78	90	95·8		130		165		

TABLE XII. (*continued*).

Section.	Number of Locomotive.	Starting Period. Amperes	Starting Period. Time.	Running Period. Amperes	Running Period. Time.	Average Amperes for Section at 430 volts.		Total Time Current on.		Total time in Section, Start to Stop.		Increased Energy for Section, No. 15 as Standard.
Borough to Elephant, about 2,725 feet.	15	96	30	48	85	60·5	59·4	115	115	135	142·5	...
	15	87·5	35	46·5	80	58·3		115		150		
	17	105	25	41·2	85	55·7	56·3	110	117·5	140	145	3·0 % (Decrease)
	17	89·5	35	44·4	90	57·0		125		150		
	12	133	30	58	70	80·5	74·9	100	105	150	147·5	14·85 %
	12	114	30	53·9	80	69·4		110		135		
Elephant to Kennington, about 2,800 feet.	15	102·5	45	47	85	66·5	66·9	130	132·5	145	147·5	...
	15	101·2	50	47·4	85	67·3		135		155		
	17	109·4	40	46·1	80	67·1	64·5	120	125	145	150	9·2 % (Decrease)
	17	97·3	45	43·4	85	62·0		130		155		
	12	120·5	60	79·2	60	99·5	89·5	120	125	150	150	25·4 %
	12	117	50	56	80	79·5		130		150		
Kennington to Oval, about 2,880 feet.	15	98·5	40	50	100	63·9	64·1	140	147·5	160	165	...
	15	88·7	50	52·2	100	64·3		150		170		
	17	91·6	40	50·7	95	62·4	63·2	135	140	165	167·5	4·5 % (Decrease)
	17	95·4	40	52·3	105	64·1		145		170		
	12	120	40	71	95	85·4	83·4	135	135	160	157·5	21·2 %
	12	118	40	66	95	81·4		135		155		
Oval to Stockwell, about 4,000 feet.	15	108·3	30	55·6	180	63	64·45	210	215	240	242·5	...
	15	96·5	35	60	185	65·9		220		245		
	17	104	30	59·4	160	66·4	64·4	190	197·5	230	235	8·2 % (Decrease)
	17	102·2	25	57·0	180	62·5		205		240		
	12	140	30	65·5	165	78	77·95	195	190	235	225	6·13 %
	12	116	35	69	150	77·9		185		215		

TABLE XIII.

Starting Period.

Section.	Electrical Horse-power.	H.P. at Draw-bar.	H.P. at Tread of Wheel.	Efficiency at Draw-bar.	Efficiency at Tread of Wheel.	C^2R loss in Loco. per cent.	Speed Attained, miles per hour.	Time to Attain Speed in Secs.
No. 12 Locomotive.								
Boro' to Elephant	66·7	17·0	[illegible]	[illegible]	[illegible]	23·15%	16·5	30
Kennington to Oval ..	64·8	22·15	36·0	32·4%	46·3%	23·2%	14·0	50
Oval to Stockwell	68·0	24·17	37·15	35·0%	53·15%	25·0%	14·5	35
" "	65·65	10·15	34·85	20·3%	51·0%	21·65%	15·75	30
Average percentage ..				29·0%	52·35%	23·5%		
No. 15 Locomotive.								
Kennington to Oval ..	47·2	14·0	25·15	31·5%	53·15%	12·25%	13·0	50
" " ..	51·2	21·7	33·0	42·1%	66·2%	13·2%	13·5	50
Oval to Stockwell	54·1	11·05	26·0	22·0%	40·8%	13·7%	15·5	35
" "	49·8	8·75	21·8	17·0%	44·0%	12·2%	15·0	40
Average percentage ..				28·4%	53·2%	12·8%		
No. 17 Locomotive.								
Kennington to Oval ..	48·5	21·0	33·1	43·0%	68·2%	19·24%	13·5	40
" " ..	52·15	13·5	28·4	35·[illegible]%	54·5%	10·8%	13·5	40
Oval to Stockwell	55·8	13·0	25·4	23·[illegible]%	46·0%	22·1%	15·25	30
" "	63·2	19·6	39·5	36·6%	62·5%	18·9%	15·25	25
Borough to Elephant	..	..	..	..	..	..	..	..
Average percentage ..				30·5%	57·7%	20·0%		
No. 3 Locomotive.								
Oval to Stockwell	67·8	23·4	37·5	35·0%	59·0%	28·0%	16·75	30
Borough to Elephant	67·7	27·0	45·2	40·0%	66·7%	27·9%	18·5	30
Elephant to Ken'gton	80·8	40·4	54·5	50·0%	67·7%	31·0%	16·75	40
Stockwell to Oval	59·8	23·3	41·1	39·5%	68·7%	23·7%	19·5	40
Oval to Kennington ..	..	..	..	..	..	..	..	..
Average percentage ..				46·1%	65·5%	27·6%		

Running Period.

Section.	Electrical Horse-power.	H.P. at Draw-bar.	H.P. at Tread of Wheel.	Efficiency at Draw-bar.	Efficiency at Tread of Wheel.	C^2R loss in Loco. per cent.	Average Speed, miles per hour	Duration of Test in Seconds
No. 12 Locomotive.								
Boro' to Elephant	30·0	21·5	27·2	69·5%	88·0%	10·0%	16·28	80
Kennington to Oval ..	37·2	28·3	32·5	76·2%	87·5%	13·0%	13·45	95
Oval to Stockwell	30·8	29·2	33·8	73·2%	85·2%	12·8%	14·45	150
" "	41·3	30·8	35·2	74·6%	85·2%	14·2%	13·48	165
Average percentage ..				73·4%	86·4%	12·5%		
No. 15 Locomotive.								
Kennington to Oval ..	31·5	22·25	27·75	70·5%	88·0%	8·0%	13·35	105
" " ..	29·4	20·38	25·75	69·4%	87·7%	7·6%	13·10	100
Oval to Stockwell	34·2	24·6	20·4	72·0%	86·2%	8·7%	12·50	185
" "	33·0	24·6	29·9	74·5%	90·7%	8·2%	13·20	170
Average percentage ..				71·6%	88·15%	8·12%		
No. 17 Locomotive.								
Kennington to Oval ..	27·8	20·3	24·9	73·2%	89·5%	10·5%	13·8	95
" " ..	29·9	20·0	24·35	69·0%	81·5%	10·0%	12·8	105
Oval to Stockwell	32·2	22·3	26·56	68·85%	82·0%	12·3%	12·8	160
" "	..	..	..	..	..	..	..	..
Borough to Elephant	24·2	16·5	22·3	68·0%	92·0%	8·0%	15·3	85
Average percentage ..				69·85%	86·25%	10·2%		
No. 3 Locomotive.								
Oval to Stockwell	59·4	47·0	54·0	79·0%	90·0%	9·35%	18·0	105
Borough to Elephant	47·9	33·5	42·0	69·5%	87·7%	8·6%	20·4	55
Elephant to Ken'gton	49·2	33·25	39·6	67·8%	80·4%	9·7%	17·6	65
Stockwell to Oval	..	..	..	..	..	..	..	..
Oval to Kennington ..	45·75	31·6	41·8	69·2%	87·8%	6·9%	24·75	30
Average percentage ..				70·0%	86·4%	9·0%		

TABLE XIV.

Performance Diagram.	Characteristic Curves.	Motor No.	Time in Seconds	Watt-hour Consumption.	Percentage increase or decrease. Time.		Percentage increase or decrease. Watt-hours.		Total Percentage Time and Watt-hour Consumption.		No. of Conductors on Armature.	Resistance of Motor. Ohms.	
					No 2a Standard.	No. 6 Standard.	No. 2a Standard.	No. 6 Standard.	No. 2a Standard.	No. 6 Standard.			
FIG. 31	FIG. 30	1	130	1905	+ ·77	+ 1·16	+ 7·6	+ 42·8	+ 8·37	+ 43·96	810	·70	Parallel current limited to 250 amperes.
37	34	2	133	1700	+ ·31	+ 3·5	− 3·9	+ 27·8	− 0·85	+ 31·30	972	·75	" " " "
37	34	2a	129	1770	...	+ 3·8	...	+ 33·2	...	+ 37·0	972	·75	" " " "
38	36	3	130	1780	+ ·77	+ 1·16	+ ·56	+ 33·8	+ 1·34	+ 34·9	1,134	1·064	" " " Field shunted Running period
43	36	3a	127·5	1730	− 2·26	− ·78	− 1·16	+ 30·0	− 3·42	+ 29·2	1,134	1·064	Parallel current limited to 300 amperes. Field shunted Starting period
40	39	4	126·6	1625	...	− 1·48	...	+ 21·4	...	+ 19·8	810	·796	" " " "
42	39	4a	128·5	1477	...	0	...	+ 11·05	...	+ 11·05	810	·796	" " " "
45	44	5	132·5	1489	..	+ 3·1	...	+ 11·95	...	+ 14·95	972	·997	" " " "
47	46	6	128·5	1330	...	...	...	...	...	...	729	·663	" " " "

The CHAIRMAN: The discussion on Mr. McMahon's paper must be postponed to the next meeting on the 18th May. I have to announce that the scrutineers report the following candidates to have been duly elected:—

Member:

William Hugh Vincent.

Associate Members:

Ernest Edward Eccles.
Walter Hepworth-Collins.
Walter John Hill.
Arthur H. Johnson.

Foreign Member:

Frank J. Sprague.

Associates:

Charles Henry Brandreth.
Robert Anderson Buchanan.
Samuel Crookes.
Charles Mark Davis.
Charles Ernest Goad.
John Phillips.
Arthur F. W. Richards.
Arthur J. Wray.

Students:

Frank Fletcher.
Francis Harrison Goodall.
Lewis Ernest Sitzler.
Sydney Melbourne Tyson.

The Three Hundred and Thirty-Fourth Ordinary General Meeting of the Institution was held at the Society of Arts, John Street, Adelphi, on Thursday evening, May 18th, 1899—Mr. J. W. SWAN, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on May 4th, 1899, were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council :—

From the class of Associates to that of Members—

Herbert Woodville Miller.

From the class of Associates to that of Associate Members :—

Horace James Bryden.
F. A. Cortez-Leigh.
William Crawford.
C. M. van Cuylenburg.
Herbert C. Gunton.
Edward L. Hanna.
William George Hibbins.
Charles Frederic McInnes.
A. J. L. Rebeiro.
Leigh Robinson.
R. J. Strike.
Thomas Symonds Tuffield.

From the class of Students to that of Associates :—

Francis J. Benton.
S. J. Clay.
A. V. Mason.
G. A. M. Rossetti.
Victor Watlington.

Messrs. H. A. Barnett and M. B. Field were appointed scrutineers of the ballot.

The PRESIDENT : We must now proceed to the discussion on Mr. McMahon's paper. The President.

Professor R. H. SMITH : Before the discussion is opened, may I ask Mr. McMahon to tell us how he measures the velocity ? I cannot find it in the paper. Professor Smith.

Mr. McMahon. Mr. McMAHON: The velocity was measured with an ordinary Bailey tachometer reading miles per hour.

Mr. Grove. Mr. C. E. GROVE: I think we are very much indebted to Mr. McMahon for the valuable results he has placed before us in his paper. It will be useful to every one who has to design locomotives in future. As I was responsible for the design and construction of one of the machines mentioned (No. 20), I have had to go over some of the ground which Mr. McMahon has traversed, and therefore I am in a position to appreciate the enormous value of the results which he has given for the whole of the machines on the line. When Mr. McMahon is recording facts, he is on solid ground eminently his own, and we must accept his results with respectful gratitude; but it appears to me that when he leaves the facts and attempts to explain them, and still more when he proceeds from special explanations to broad theory, he is not quite so happy. I propose to devote my remarks to the criticism of one or two deductions rather than to the actual experiments.

In the first place, I do not find that the expression "draw-bar" pull, and the lessons derived from it, are treated with that precision which might have been expected. It does not seem to be sufficiently clearly recognised in the paper that the draw-bar pull is the total horizontal effort exerted by the locomotive minus that effort which is required to move the locomotive itself. The author on page 520 says: "It is customary in giving the results of a test on a street-car motor to speak of the horizontal effort at the tread of the wheel of a certain diameter, and to call the results so obtained the draw-bar pull." Of course the tread at the wheel of a motor cannot be draw-bar pull. That ought to be recognised first of all. "Experience shows that the actual draw-bar pull at starting, as measured by a dynamometer, is only about 73 per cent. of the actual tractive effort at the tread of the wheel of the motor." The motor in the second case I understand to mean a locomotive. Then on page 528, still on the same subject, the author says: "The draw-bar pull as shown by the dynamometer"—this is a locomotive running from Kennington to the Elephant and Castle—"was 1,075 lbs.; the tractive force due to acceleration plus or minus gravity was 201 lbs., and as this force did not appear at the draw-bar, but was given out by the locomotive at the tread of the wheel, it must be added to the draw-bar pull." There appears to be some confusion here, because if the locomotive is accelerating the train, and is doing work, either with or against gravity, the effect must be shown on the draw-bar. What you do not get on the draw-bar is only the effort the locomotive is exerting on itself. Figure 11 is based on a statement of this class, and Tables II. and III. on pages 595 and 596 follow. In this case the draw-bar pull at the dynamometer in pounds observed is corrected for the tractive effort due to gravity and acceleration. Then the tractive effort at draw-bar at constant speed on the level, given in another column, is, in every case, higher than the observed value of the draw-bar pull. As these results are obtained on sections where the train is accelerating, and is either, after a few moments of acceleration, going up the hill or down a very slight one, it appears to me that the tractive effort at constant speed on a level should be less than the observed draw-bar pull, and

I do not understand why all these figures in column 4 are higher than those in column 1. On page 534, where the author speaks of locomotive losses between Stockwell and the Oval, he gives the average draw-bar pull as 268 lbs. for an average current of 36·8 amperes, and so on. He says: "The average tractive force due to acceleration, plus or minus gravity, was 8·36 lbs., leaving 259·7 lbs. as the tractive force at the draw-bar of the locomotive on the level at a constant speed. From the Prony brake test the tractive force at the tread of the wheel for a current of 36.8 amperes was 340 lbs., leaving 80 lbs. as the locomotive losses, in which are included journal and rail friction, and air resistance." Surely rail friction and air resistance are not locomotive losses, which I should understand to be losses in regulating devices and in the heating of the motors. The rail friction and air resistance must come out of the 268 lbs. given as draw-bar pull. Mr. Grove.

Going back to pages 520 to 526, the author describes motor and locomotive tests, and obtains certain results by measuring the pull by means of a dynamometer of most ingenious construction; but he does not get quite the result that he wishes to have. He makes tests of the motor under statical conditions. On a series of curves with locomotives, Nos. 6, 12, and 17, in which the tractive force, as observed by means of the Prony brake, is compared with the tractive force observed from the draw-bar, there is a difference of something like 30 or 40 per cent. The author calls that "loss." It appears to me that it is not loss. The tractive force of the motor, measured when the motor is doing nothing, simply lying idle and able to turn round under the influence of its current, is the true measure of the force which the motor exerts. When he tests it with the weight of its magnets on its bearings, and then allows for the weight of the whole locomotive on the magnet bearings, as he would get it under running conditions, he accounts for the difference between what he observes under the statical conditions, and what he observes when the locomotive is complete and is pulling the draw-bar attached to a fixed point with a spring balance, but he does not appear to consider that satisfactory. It seems to me that he is really there measuring the statical effort exerted by the motor, but besides what he observes from the spring balance he has got an extra resistance to the turning of the motor, in the shape of weight on the axle, and therefore frictional resistance, which is not measured by the spring balance readings, but which, when it is added to them, quite explains the difference.

At this point I should like to observe that in my opinion the simple torque test has a great superiority over the Prony brake test for measuring what a motor will do. Perhaps I may be allowed to illustrate that by a test which was made on locomotive No. 20 at the works. The accompanying diagram (Fig. A.) shows one of the motors standing with a lever immediately fixed to one wheel, and counter-balanced. The spring balance is able to read about 600 lbs., and its reading multiplied by the length of the lever gives the total torque in pounds-feet which the motor could exert. Then by taking a series of values of the current, we were able to get exactly the curve of torque. The ascending values of the current are shown in the lower line. For the descending values of

Mr. Grove. the current of course there was a little lag in the magnets which gave a curve higher on the plane than the lower one. In order to determine what the true curve should be, two things were done: first, an arithmetical mean of the two readings from these black curves was taken; and secondly, a series of current readings and torque readings was taken at critical points, the value of the current in the second case being kept constant for ten minutes for every test, during which the magnet was tapped with a hammer so as to respond to the magnetic condition. The results were plotted in red spots in the centre diagram, and were found to lie exactly on the curve which is the arithmetical mean of the other two. There are a few black spots in the middle portion of the red (*i.e.*, centre) curve, and they arose from the fact that the observer made rather a big jump in current from one point to another, and then came back to fill in the gap, so that these spots are really on a descending curve. If they had all been taken rising they would have remained in the lower curve as do the others. When the statical torque is measured in that way it is a pure current function

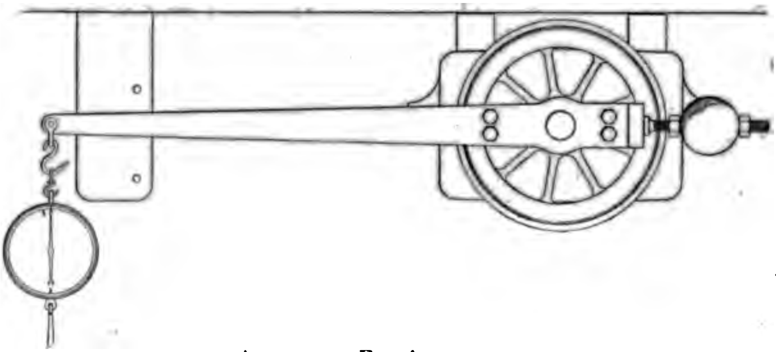


FIG. A.

of the magnetic flux, which, in a series motor as this is, is also a function of the current. Knowing the definite values for torque and current, we can get the actual flux from the magnets into the armature, which is always a difficult thing to determine in any other way. Then, substituting this value in the E.M.F. equation, a speed curve can be plotted across that, which is much more accurate than anything that can be determined by taking five-second readings on a tachometer. Lest it should be objected that this method of testing does not realise the conditions of practice, I should like to point out that when these motors were designed they were calculated from the specification requiring a certain time to be taken in running over sections on the railway with a given weight of train behind the locomotive. The required acceleration to do the journey in a stipulated time was calculated for every section. Then the value of the tractive force was taken at 12 lbs. per ton, and it was not known then whether that was accurate or not; there might be errors there, and, in order to have a margin of power, a margin was added to the required tractive

force which the motors were expected to give, and they gave exactly what they were calculated to do. As stated in the table on the first page of the addendum to the paper, No. 20 locomotive has nearly 10 per cent. greater speed than the other locomotives on the line, which was precisely what it was intended to have, and I think that justifies the harmony between theory and practical result. Mr. Grove.

Knowing this, when I find such statements in the paper as that on pages 520 and 521, where upon a given test a locomotive efficiency comes

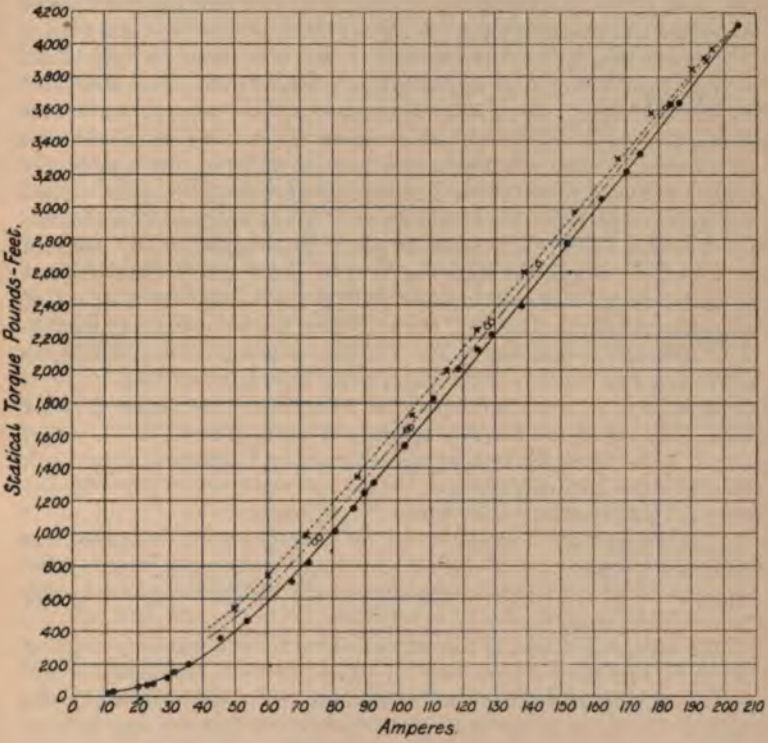


FIG. B.

out at over 100 per cent., I know there must be some error either in the figures or in the method of making the measurements. We may premise that the arithmetical work is right, and then comes the question as to whether the measurements are rightly taken. By the courtesy of the author I was allowed to ride over the line on this locomotive and assist in taking some measurements under the usual conditions. I found they read amperes and volts on dial instruments, and speed by a tachometer belt driven from the axle, and at every five seconds a man gives the time with a watch and whistle. The man has to look at the dials, get the reading on the paper, and be ready to look again at the next

Mr. Grove. five seconds signal, and so on. The needles are vibrating violently, due to the oscillation of the locomotive, and the quantities under measurement are varying the whole time. I am quite surprised that under those conditions any accurate results could ever be obtained. I am ready to allow that men who have done the same thing frequently will do it better and get better results than a stranger would, but I still think there is room in that method of testing for a large margin of error, especially as to what is the instantaneous value of the speed of the locomotive. That error is sufficient, I think, to vitiate these general results and account for the discrepancy between observations of actual draw-bar pull and what it was expected would be the result. I say that with all confidence, because I tried some time ago to make a set of these tests under far better conditions. Three years ago I put a dynamometer car on an inner circle train, and took it to measure the tractive force all round the circle. It was a main-line car, which had the usual automatic apparatus driven from the car axle by gearing, so that the paper speed exactly corresponded with the train speed. There was an electric chronograph which every thirty seconds put a time notch on the diagram by a current which was kept on for two and a quarter seconds, and yet, although we had our record strictly proportionate to the speed of the train, and the precise time marked every half-minute, it was exceedingly difficult to find out in the periods between these half-minutes what the actual velocity of the train was. That is a condition which is far superior to such a condition as the author has under control, and I think if he could see his way to put on an apparatus which would record, if possible, the velocity of the train, in a curve of some kind, the velocity at any instant could be got off, the figures corrected, and many of these curious results would be explained.

[*Communicated.*] Time failed me on the night when this paper was discussed to refer to any other of the many interesting points in the paper, and I take the present opportunity of reading the proof to add the following remarks :—

The author describes at considerable length the effect of re-winding motors of the older locomotives. I gather from the statements of the paper that the armatures of the motors of No. 3, which is one of the original locomotives, and had 540 armature conductors, was first re-wound with 810 conductors of smaller section ; at the same time the simple series resistance method of regulation was given up and series-parallel control introduced (pp. 544, 545), and that afterwards calculations were made of the effect (*a*) of winding the armature with 972 conductors (page 551), (*b*) of winding with 1,134 conductors (page 552), (*c*) of making the armature core 50 per cent. longer with 972 conductors (page 566), (*d*) of making the armature 100 per cent. longer with 729 conductors (page 568), (*e*) of shunting the fields with various shunts during the starting period. [The author says "at starting," but the diagrams 67, 68 show the shunting of the field taking place several seconds after the start. To shunt the field at the precise instant of starting seems to me to throw away part of the starting power of the locomotive and to be equivalent to using a smaller motor.] The first

change from 540 to 810 conductors was actually made, and appears to have resulted in a greatly improved performance of the locomotive. This is very interesting, because, if I remember aright, it was said some years ago, when the City and South London equipment was described before the Institute of Civil Engineers, that the original contractors fell short of the requirements in regard to locomotive performance in consequence of a mistake having been made in estimating the weight of the trains to be hauled, and that in consequence the motors were worked over their power. The entire improvement is claimed for the alteration in the winding, and little or nothing is allowed for the effect of the series-parallel control; but in either case the author is to be congratulated upon the improvement he has made. I infer—but the paper is rather vague, and I am not sure whether my inference is correct—that the changes indicated from (*a*) to (*d*) above were only *calculated* and not actually carried out, and the effect of shunting the fields was only partially carried out. For this reason the results are less convincing than they would be if the records represented actual experience. To enlarge any one of the motors of these locomotives by 100 per cent. or even 50 per cent. of the length of its armature core I take to be physically impossible; and even if it were not, the change is so far-reaching that the motor becomes quite a different machine; and in the absence of data as to the changes brought about in the magnetic densities it is quite impossible to say whether as a whole the change is beneficial or otherwise. I think it may be stated with some confidence that if a modern traction motor were taken and its armature conductors increased by 50 per cent. or more while the iron parts of the machine were left intact, or if the core length were made 1½ times or twice its original dimensions without such alterations being made in the winding, pole pieces, &c., as would turn the motor into a new machine, it would be spoilt for the purpose for which it was intended. The essence of good dynamo and motor designing lies in the proper *balancing* of the electrical and magnetic quantities of the machine; the author appears to neglect this vital consideration, and so his investigations appear artificial and unreal. Mr. Grove.

Although I am afraid my criticism has been somewhat destructive, I am by no means insensible to the value of the paper; on the contrary, I think it is the most useful paper on Electric Traction that the Institution has yet received; and one that will well repay all the demands it makes for careful study.

Mr. A. M. TAYLOR: Three years ago I took up the question of the tractive resistance per ton of train, and worked it out, to some extent, in the same way as the author has done, by deduction from the total tractive effort per ton of train on the level. I am rather surprised to find that my results, which, however, were perhaps somewhat rough, differ materially from those quoted by the author. Mr. Taylor.

The first discrepancy between our results is in Figure 17, which relates to locomotive No. 15, constructed by Messrs. Siemens Bros. The tractive force, as measured on the Prony brake, is there given by the author as 2,400 lbs. with 118 amperes, and about 1,940 lbs. with 100 amperes. I am not here on behalf of Messrs. Siemens Bros., but I think the tests made by that firm under similar conditions on the brake

Mr. Taylor. gave 2,0́o lbs. and 1,700 lbs. respectively. Between 2,060 lbs. and 2,400 lbs. there is a considerable difference, which perhaps the author may be able to explain.

The next discrepancy is in figure No. 29; I have prepared a curve connecting the tractive resistance in pounds per ton of train with the speed, using, as I believe, the same locomotive as that referred to in the paper. The initial value of the tractive force reads about 25 lbs. per ton of train, as against the author's 40 lbs. The lowest figure reached is about 4 or 5 lbs. per ton of train as against the author's 10, and the shape of the speed curve is quite different.

The third discrepancy is in the average which the author has

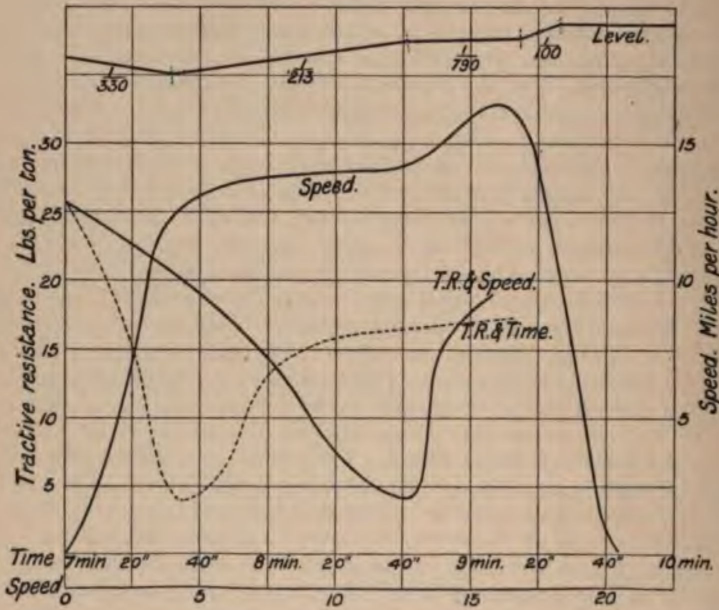


FIG. C.

obtained. He quotes an average between Kennington and the Oval, and with the same locomotive obtains 11·75 lbs. per ton of train as against the figure which I obtained of 15·6 lbs. I obtained the 15·6 lbs. as a speed average. I have plotted in the diagram two curves. The dotted curve represents pounds per ton of train plotted with time, and the full curve, which I have already alluded to, represents pounds per ton of train plotted with speed. Now, I have also plotted another curve, which is not shown on the diagram, viz., pounds per ton of train plotted with distance, and I have taken averages of all three. The average of the time curve was 15 lbs. per ton, and the average of the distance curve was 13·6, that is to say, pounds per ton of train plotted with distance, which is of use in making calculations of the foot-pounds of

work done over the line. The average of the speed was 15·6, and I take it the author's average was also that of speed, so that there is rather a discrepancy there between the author's figures and those which I obtained. The way I obtained my figures was by taking, in the way that Mr. Grove has already explained, the total effort of the motors, and deducting the acceleration forces, dealing with the whole mass of the train, and having nothing at all to do with the draw-bar pull. I agree with Mr. Grove in what he said on those points. Having worked the thing out in detail myself, I know that it means the expenditure of a great deal of time, and that there are a great many sources of error. The principal of these is no doubt in the speed, and in differentiating the speed curves you get the most surprising changes in the acceleration force, and in fact in my figures I had to "cook" the curves considerably to make them agree correctly. The acceleration forces vary so tremendously with the least change in the speed that the tractive resistance was quite negligible in many cases. Mr. Taylor.

Mr. McMAHON : Do you mean the average while the current was on? Mr. McMahon.

Mr. TAYLOR : Yes. Mr. Taylor.

Mr. McMAHON : I will explain it later. Mr. McMahon.

Professor PERRY : In getting these curves, do I understand there was an actual instrument used for measuring this pull? The curves are very different from those which have yet been published, and that is the reason why I ask the question. Professor Perry.

Mr. TAYLOR : No; that curve was obtained by taking the total pull given by the motors at the different observed currents taken at very frequent intervals over the line, and deducting from those gravity pulls, positive or negative as the case might be, and acceleration pulls, positive or negative, the result being the pull shown on the curve. Mr. Taylor.

[*Communicated.*] Referring again to Mr. McMahon's figure No. 29, showing tractive resistance per ton at various speeds, it was brought out in the discussion that this curve deals only with the resistance of that part of the train which is behind the locomotive draw-bar; this reduces the discrepancy between the results obtained by Mr. McMahon and myself, as one would expect that the resistance per ton, taken over the whole train (*i.e.*, including locomotive), would be higher than that of the part behind the locomotive draw-bar.

In this connection there seems to be a slip at the top of page 539. The 406·3 lbs. is, presumably, *the difference* between the total tractive effort of the motors and the calculated pull due to acceleration and gravity, calculated for the whole train and not for the carriages only; for only on this assumption would it be correct to divide by 37 tons the weight of the whole train.

If it were the difference between the draw-bar pull and the acceleration-and-gravity pull, calculated for the rear part of the train only, it should only be divided by the weight of the rear part, and it would then represent only the tractive resistance of that rear part. On the other hand, if it represents the tractive force per ton taken over the *whole* train (including locomotive), it no longer forms a check on the previous tests, as is stated (on p. 537) to be its object; and when divided by the figure of, say, 23 tons instead of 37 tons—as I submit it should be—

Mr. Taylor. it more nearly agrees with my own figure for the tractive force per ton of the whole train, namely, 15·6 lbs. per ton.

There are some other points on which I should like to ask Mr. McMahon for further information. I will refer, first, to page 536. There seems a disagreement between columns 2, 3, 4 and 5 of Table VIII., and column 2 of Table II. Should they not agree, where the speeds are comparable?

Referring now to page 528, I cannot help thinking there is some mistake in the deductions from Fig. 11, viz., in the method of obtaining the corrected pulls. Referring to Fig. 11, we find that the draw-bar pulls, as observed, are as stated in column 2 of Table II.; but the acceleration ± gravity pulls referred to on p. 528 do not appear to agree with those in Fig. 11.

For example, instead of 201 lbs., I find about 350 lbs., and this is obviously calculated, and correctly so, for the rear part of the train only (*i.e.*, excluding the locomotive). Also instead of the 345 lbs. I find about 700, and so on. This refers to column 3 of Table II.

But what I more particularly take exception to is Mr. McMahon's method of getting the corrected pull for constant speed and level track, from which he eventually intends to deduce the tractive resistance per ton.

Surely the observed draw-bar pull *in*cludes the pull required for accelerating the part of the train behind the locomotive, and therefore, to get the pull at constant speed and on the level, we must *subtract*, and not add, the acceleration ± gravity pull from the observed draw-bar pulls?

Column 4 of Table II. is therefore, in my opinion, quite wrong, and if curves Nos. 13, 14, 15 and 16 are based on this principle, they also are, in my opinion, wrong, and of course all subsequent results based directly on them.

There is one other point I would like to refer to, and that is the construction of curve E, representing the variation of tractive force at the draw-bar with the current. So long as Mr. McMahon confines his methods to taking time-averages of the draw-bar pull and of the acceleration ± gravity pull he is, I think, safe in subtracting the latter from the former to get the *average* draw-bar pull on the level at constant speed. But directly he proceeds to argue that 259·7 lbs. (the average pull, so corrected) is the pull which would be obtained with the average current at 36·8 amperes, I disagree with him, because the pull is *not* directly proportional to the current. A reference to Fig. 6 shows that we get a total tractive force of 10 lbs. *per ampere* at 40 amperes, while we get no less than 21 lbs. *per ampere* at 140 amperes.

I think this will be found to be the explanation of what is evidently a difficulty to Mr. McMahon himself, viz., that the locomotive losses increase with the current (*see* p. 534).

If, for example, we get greater pulls per ampere as the currents are increased, the steady current which would truly correspond with the average pull observed would be the smaller the greater the pull became; and this is the current from the pull corresponding to which (as measured on the Prony brake) the average tractive force, corrected for constant

speed and for gradient, should be deducted. Thus with bigger currents and pulls the locomotive losses would be a smaller quantity than is given by the method adopted by Mr. McMahon. Mr. Taylor.

To put it shortly, my contention is that the 259·7 lbs. pull referred to by Mr. McMahon would actually take something under 36·8 amperes, possibly only 32·0 or 33·0 amperes, and that this reduction would be still more marked in the values nearer to the bottom of Table IV. It will be understood that I in no way challenge the figure 36·8 as being incorrect for the *average*.

In conclusion, I wish to join most heartily in the chorus of praise to Mr. McMahon for his extremely valuable paper.

Mr. W. M. MORDEY: I must thank the Author for giving us this mass of practical results, and particularly congratulate him on the perseverance that he must possess to obtain all these experimental results under the very trying and difficult conditions of his line. I hope that some of those results will be criticised to-night by our American friends who have had so much practical experience in these matters. I would draw attention to one important difference between the Author's practice and American practice. When I was in the United States last year, I was told that successful working depended upon the controller, that in fact the controller really controlled everything; and that more failures were due to questions connected with that instrument than to anything else. Therefore, I was interested to see that the Author is using a controller of an entirely different type from that employed in the States in connection with tramway work. It has no magnetic blow-out, and is, in fact, an exceedingly simple thing when compared with American controllers. When the Author was good enough to show me the apparatus, I suggested an explanation, which perhaps may account for it working on his line, whereas it would, I think, be a complete failure on ordinary tramways. I suggest the explanation is that he switches off his current when the motors are running at full speed. In ordinary tramway work the motors are constantly being switched off when they have no back electro-motive force. That makes all the difference. When a motor running at full speed is switched off there is practically no electro-motive force in the circuit; that is to say, the counter electro-motive force is practically equal to the electro-motive force in the line, and consequently the break is very much more easily made than in the case of motors switched off at low speeds. Those who have had practical experience must have noticed the very marked difference between the arc formed when a motor circuit is broken, and that produced on breaking a generator circuit. If, for example, the brush of a motor running at full speed be lifted, no sparking occurs; but if the brush of a generator be lifted under similar conditions a bad arc is formed. The difference is of course due to the fact that in the latter case generation is going on and everything tends to keep the current going; whilst in the former a circuit is opened in which two practically equal electro-motive forces are opposing one another. I pointed out this in a little paper in 1886, and am interested to have this illustration of it. Mr. Mordey.

Mr. Mordey. When I heard this paper, I felt rather like a monkey which had got a nut of which the shell was very hard. I felt it was quite worth the cracking, but supposed the author made his paper not too easy so that we might realise that it was worth some labour to get at the very excellent results which it undoubtedly contains.

Mr. Brousson. Mr. R. P. Brousson: I have listened with very great interest to Mr. McMahon's paper, and I appreciate the amount of work which he must have done to produce it. The author says that with a plain series method of regulation he finds no difference, or very little difference, in the total energy drawn from the line, and also in the time per section. I think this is to be expected, and shows one of the advantages of a series parallel controller. Last year, when Mr. Stotherd and I were making experiments on the trial locomotive for the Central London Railway, we used an oil dynamometer about 10 or 11 inches in diameter, and with several scales reading up from 2,000 to 22,000 lbs. We experienced a difficulty in the tremendous variation in the draw-bar pull, and found it necessary to take readings every two seconds. Like Mr. McMahon, I noticed that we obtained no reading on our dynamometers until about two seconds after the current was switched on. I note that the author takes as his maximum 1·46 feet per second for his acceleration. I do not consider this to be high value, and it is nothing like that which is very generally used in the States at the present time. On the elevated roads in Chicago, for instance, they habitually use a braking acceleration of 2·25 feet per second per second. In New York also the accelerations are higher than he states. With street cars it is even higher still. The present practice is to increase the accelerations very much, and my experience is that you can increase them very considerably without the passengers feeling it. What they do feel is the change in the rate of acceleration. I notice in Figs. 35 and 37 that the author, in switching from series to parallel, allows his current to droop considerably before taking on the parallel position, and consequently he gets a considerable drop in the rate of acceleration. I judge that this will be felt by the passengers, especially when the trains are first getting up speed. From the economical point of view, I believe that if Mr. McMahon were to have slightly increased the resistance in circuit, so as to bring the value of the series current more uniform and get a straight line, representing a more or less constant current on the series position, and so were to better average his current, both in the series and parallel position, and obviate this droop in changing from series to parallel, his results would be even more economical than they are at present.

The author states on page 582 that, from experience, he has found it unnecessary to lock the reversing switch of his controller. This is certainly a radical departure. The time when a reversing switch is most needed is when a man is likely to lose his head, and therefore I do not think that the motor man should have any chance of being able to reverse his motors before he has switched into full series and full resistance.

In the last locomotive described motors with consequent poles are used. My own experience is that this has been a mistake on account

of sparking. Some heavy trains in the States have used motors of this type, but they were given up for this reason. Mr. Brousson.

Mr. McMahon is to be heartily congratulated on the good results he has obtained with his locomotives for the kilowatt hours per ton-mile, viz., ·055. I think that is a very good result indeed, considering the conditions he has to contend with on the South London Railway.

Professor SMITH: I wish to pay a tribute of gratitude and of admiration to Mr. McMahon for the immense amount of work represented by this paper. It is not only the work represented by the taking of the measurements and the experiments he has made; but in this case the work done in making calculations from these experiments is very considerably greater than that involved in taking the measurements themselves. Mr. Grove referred to the great changes in accelerating force, and also to the labour of calculating from observations taken at thirty-second intervals, and Mr. Taylor has confessed that from observations taken at that interval he had to "cook" his results considerably. But here the observations were taken at five-second intervals—that is six times as many in the same period—and there is, therefore, the possibility of much greater refinement in calculations from these observations. That such refinement has been arrived at, and that the labour to attain to it has been undertaken, is shown by the strange shapes that the acceleration curves take through all the diagrams given in the paper. I have frequently advocated the necessity of taking observations at very frequent time intervals in investigating this subject, and I was very glad to hear Mr. Brousson say that he, at least in one case, has taken observations at two-second intervals. I do not think that interval is too short, but that we should make it even shorter. Professor Smith.

I have previously spoken on the desirability of taking direct measurements of acceleration, and would ask, Why not mount an instrument for measuring and indicating directly the acceleration? Why leave it to be a matter of calculation from observations of velocity? I think that acceleration in a suitable instrumental design can be measured with perhaps greater accuracy than velocity can. It can also be done with greater ease in avoiding instrumental errors.

I am especially interested in the design of dynamometers of the kind described by the author, and I admire his design. It is founded upon the principle of the famous Emery testing machine, which was described a good many years ago and demonstrated in London. But the merit of that system of pressure measurement depends to a very large extent upon the adopton of the zero method; that is to say, in order to get any very high degree of accuracy, you must bring your diaphragm and the moving parts of your indicator to the zero position when you take the reading. Mr. McMahon complains in his paper of the jerkiness of the indications, and of the difficulty of taking a fair mean between the points to which the indicator flies about over the dial, and also of the difficulty in preventing the diaphragm being torn at the corners of the joint by the jerks. He tried copper and rubber with a copper web, or a brass wire web in it. This gave way from the chafing at the edges. I think that, in order to make this

Professor Smith.

instrument really successful for accurate measurement in this position where shocks cannot be avoided, it is necessary to have the pressure indicator of such a sort that the flexible part of it makes but a very small movement, so that the piston of the flexible diaphragm at the buffer beam will also move but little. That movement of the indicator must be multiplied by one or other delicate mechanical means in a very large ratio.

Although the paper under discussion has immense merit on the grounds that I have already stated, the difficulty of understanding it has been a little increased by some vagueness of terminology and of expression throughout. For instance, in regard to the expression "draw-bar pull," I think a very unnecessary amount of doubt and difficulty is carried on through a great many pages of the paper by the omission of the obvious consideration of acceleration. Mr. McMahon lets us see his own mind working through difficulties before he seems to recognise the immense importance of this question of acceleration, and then in the latter part of his paper he solves these troubles. It was rather unnecessary, I think, to let his readers work through all the same difficulties. Then, again, there are one or two misprints in the diagrams which produce confusion. He says that 73 per cent. of the torque only is represented by draw-bar pull. That, he says, is the percentage near the moment of starting. If one considers that in connection with the question of acceleration the connection of the two becomes very simple and obvious, because we know that at this time most of the force is being spent on acceleration. We have, according to the weights given, the mass of the train just 70 per cent. of the sum of the masses of the train and of the locomotive, so that we have 30 per cent. of the force spent upon the total acceleration spent on the locomotive and not transmitted to the draw-bar, which has a very obvious connection with the 73 per cent. which has appeared at the draw-bar as mechanical pull. If that had been mentioned at first the explanation might have been a little plainer.

I would, however, chiefly refer to a doubt that I have with regard to diagrams 17 and 24. Here we have co-ordinated points of draw-bar pull and speed in miles, and at first sight these diagrams look very natural and very pretty and all right, because we find the tractive force of the draw-bar going up with the speed in such a manner as those who know a little about it might reasonably expect. Also we see the curve showing the so-called locomotive losses going down with increase of speed in a general manner such as also might be expected by those who know something of journal friction, which diminishes as speed increases up to a certain limit that might be expected to correspond roughly with the limits shown here. But it is explained that these locomotive losses include not only journal friction and rail resistances, but also air resistance. And certainly whether these things, either properly or improperly called locomotive losses, go down or go up, one is certainly rather taken aback to find that as increase of speed goes on, the increase of tractive force at the draw-bar—that is the pulling force upon the train behind the locomotive—precisely balances the decrease of these locomotive losses. One cannot

Professor Smith.

understand how they should so precisely balance each other, the whole tractive force being constant as shown by an absolutely steady uniform line of one height. I rather think some error has come in here in regard to the interpretation of the different quantities that have to be dealt with in arriving at the results. Taking it in one very simple way, the locomotive losses described here are of absolutely the same nature as the train resistance, which is overcome by the draw-bar pull, except with regard to the journal friction and the electrical losses. I think journal friction goes down, and the others go up, and one would expect that locomotive losses, as defined by Mr. McMahon in his paper, would go up instead of going down. But of course it is easy to get different speeds with the same current. One method of doing that is followed here, namely by making use of different accelerations due chiefly to different gradients connected with the constant torques produced by constant currents. The locomotive losses in the diagrams given in the paper are derived by subtracting the draw-bar pull from the tractive force represented by the electro-magnetic torque. So that, derived in that way, there is no possibility of anything but this equation coming out, the increase of the one exactly counterbalancing the decrease of the other. Such different speeds of course can be obtained with the same current and the same armature torque, but at these different speeds with the effect of acceleration and level eliminated, we have surely, under definite air conditions and definite rail conditions, a definite train resistance with each of these speeds; and the excess over that train resistance of this driving torque at the armature is spent first in overcoming frictional and rail resistance, and then in producing simply acceleration. Mr. McMahon has subtracted the acceleration of the locomotive alone, and he has done so rightly, because, arguing from the armature torque back to the draw-bar pull, one has nothing whatever to do with the mass acceleration of the train behind the draw-bar. But, on the other hand, he has calculated corrections for gravity, and he has differentiated between train gravity and locomotive gravity, and it is rather interesting to look at his curves to see the differences of the shapes of the two curves of locomotive gravity and train gravity, the difference being chiefly due to the greater length of the train passing from the level on to the gradients, and so on. I am rather afraid that he has subtracted the train gravity as well as the locomotive gravity. Of course in arguing back from the armature torque to the draw-bar pull, one must in making corrections for gravity deal only with the locomotive gravity, and not the weight of the whole. He has correctly dealt with acceleration, but he has not dealt with gravity in the same way as the acceleration, and I think he has made a mistake.

Calling the electro-magnetic torque reduced to a force acting at the periphery of the driving wheels by the name "armature pull," the equation is evidently :—Armature pull − up-grade component of loco. weight − loco. acceleration − loco. friction, rail and air resistance = Draw Bar Pull = up-grade component of train weight + train acceleration + train friction, rail and air resistance. To obtain the locomotive friction, rail and air resistance from the measured arma-

Professor Smith.

ture pull and the measured draw-bar pull, one must clearly subtract from the difference between these two both the grade-component of the locomotive weight and the locomotive mass acceleration; but neither the train weight nor its acceleration come into this calculation.

Professor Carus-Wilson.

Professor C. A. CARUS-WILSON: One point of special interest in this paper is the attempt to find out the tractive resistance in tunnels. The curve in Figure 29, if I understand it correctly, includes all resistance behind the draw-bar.

Under these circumstances, we do not know what is the air resistance experienced by the locomotive itself. When Mr. McMahon publishes his paper, it would be well if he could add any data which he may have as to the air resistance experienced by the head of the locomotive itself, because in tunnels that constitutes a large part of the total air resistance; the friction of the air on the sides of the cars is a small proportion of the whole resistance offered by the air to the passage of the train. In the case in question, the cross sectional area of the locomotive is 56 per cent. of that of the tube. Under these conditions, one would not expect the air resistance to be serious at the speeds at which the experiments were made. Any further data that Mr. McMahon may be able to give as to the estimation of the air resistance at the head of the train would, however, be of great value.

In the first series of experiments made, the Author tried to increase the efficiency of the locomotive by simply increasing the number of turns on the armature. Under these circumstances, it is clear that in order to cover the given distance in the stipulated time, it is necessary to shunt the fields. The effect of shunting the fields is to make the characteristic straighter than you can get it from the simple magnetic curve of the motor; the efficiency at full load is slightly increased, and that at light loads considerably diminished.

Referring to the new locomotives which the Author has designed, it is not surprising to find that he has not got a better ratio of kilowatt hours per ton-mile. This is fully accounted for by the use of a larger driving-wheel—of 31 inches instead of 27 inches in diameter. The increase in the diameter of the driving-wheel is a great disadvantage in the new motors, but the author was obliged to make the change in order to get the necessary clearance, as he was tied down to the use of a gearless motor.

Professor Wilson.

Professor E. WILSON [*communicated*]: Mr. McMahon's paper is of great interest at the present time, as it contains valuable information as regards the working of electrical locomotives when equipped with two-pole motors. I have taken an additional interest in the paper from the fact that it deals with the performance of a Siemens locomotive (No. 15), with the design of which I was associated, and about which I have published considerable matter.[1] In 1892 Mr. A. Siemens published a series of curves taken on this and another locomotive (No. 16) supplied by his firm,[2] and I made use of these curves in the book just referred to. The following table gives the comparison between Mr.

[1] See *Electrical Traction*. London: Edward Arnold.

[2] See *British Association Communications*, August, 1892.

McMahon's data (Table I.) and the result I obtained from Mr. Siemens' curves, all in connection with No. 15 locomotive :— Professor Wilson.

Names of Stations.	Mr. McMahon's Results.								Mr. Siemens' Results.							
	Starting Current in Amperes.	Miles per hour at end of Starting Period.	Starting Period in Seconds.	Running Period in Seconds.	Total Time of Current in Seconds.	Board of Trade Units.			Starting Current in Amperes.	Miles per hour at end of Starting Period.	Starting Period in Seconds.	Running Period in Seconds.	Total Time of Current in Seconds.	Board of Trade Units.		
						Starting Period.	Running Period.	Total.						Starting Period.	Running Period.	Total.
Boro' to Elephant & Castle	120	16	35	80	115	·333	·438	·775	120	16	30	85	115	·344	4·56	·8co
Elephant & Castle to Kennington..	140	14·5	45	85	130	·547	·474	1·02	135	14	50	76	126	·53	·428	·958
Kennington to Oval	120	13·5	50	100	150	·52	·609	1·12	120	13	40	96	136	·43	·767	1·197
Oval to Stockwell .	120	14·5	35	185	220	·40	1·34	1·74	120	14	30	145	175	·32	1·31	1·63

The difference in the two tests is that in Mr. McMahon's case the current was kept substantially constant during the starting period, whereas in Mr. Siemens' curves the current was allowed to drop after its initial maximum value. The weight of locomotive and train in the case of Mr. Siemens' curves was 34½ tons, and it was loaded with 34 passengers, which bring up the total weight to about 36½ tons. Mr. McMahon does not state if his train had passengers, so that the total weight, if unloaded, would be about 34½ tons.

At page 526 Mr. McMahon states that the dynamometer did not register any pull until "one or two" seconds after switching on the current. This is an interesting statement and shows the importance of the time taken to acquire magnetism. The cause is well known, and is due to induced currents in the iron cores. Its remedy in series motors is lamination in the magnet limbs. In smaller magnet limbs than Mr. McMahon deals with I have found it took several seconds to fully acquire the flux density which would be predicted by the characteristic curve of the motor for a given current.

With regard to tractive and air resistance the author finds at an average speed of 13·35 miles per hour 11·75 lbs. per ton on level. In my calculations I assumed 10 lbs. per ton, and added 5 lbs. per ton for curves. Fig. 29 is specially valuable, showing as it does the variation of tractive resistance in terms of speed.

I think one of the most valuable results of Mr. McMahon's experiments is precise information as to difference between the draw-bar pull actually realised and what would be predicted from Prony brake tests made on the motor when taken out of the locomotive. This was the information I so much wanted when writing about these motors, and in connection with which I had to make an assumption. I may mention that I found an average efficiency of about 70 per cent. for the

Professor Wilson.

round journey with locomotive No. 15 and three-coach train, starting from the City station and returning to the same. Mr. McMahon has examined the average efficiency for this locomotive in Table XIII., and finds between Kennington and Oval 58 and 88 per cent. as the average starting and running efficiencies. Between Oval and Stockwell he finds 46 and 88 per cent. for these efficiencies.

I am sure Mr. McMahon's paper will be read with great interest by electrical traction engineers.

Mr. McMahon.

Mr. McMahon, in reply, said: The way in which you have received this paper gives me great pleasure. If I had been a little more particular in the manner in which I used the term "draw-bar pull" a good deal of confusion and useless discussion might have been avoided. If in the fourth columns of Tables II. and III., and the fifth column of Tables IV., VI., and VII., I had substituted the words "tread of the wheel" for "draw-bar," it would have been really more correct. In fact, in the manuscript the words "Tractive force at the tread of the wheel on the level at constant speed" were used, and they were subsequently altered when correcting the proof. However, any person carefully reading page 520 cannot, I think, fail to see what is meant. This will perhaps make it clear to Mr. Grove why the figures in column 4 are higher than those in column 1. Again, he objects to the air resistance, journal and rail friction, &c., to the extent of 80 lbs., being called locomotive losses. He must not forget that the locomotive is here treated as a locomotive with all its losses, and not as a pair of motors and regulating resistances without the cab, &c. Mr. Grove also objects to the difference between the tractive force obtained from a Prony brake test for a given current and the tractive force, or pull at the draw-bar of the locomotive, for the same current when pulling from a fixed point, being called "loss." Well, looked at from the view of the useful effort a locomotive can exert on the train draw-bar at starting, it certainly seems to me that it is a loss.

Mr. Grove has evidently not carefully read pages 520 and 521, or he would not assume that a test showed an efficiency of over 100 per cent. for a locomotive. It is clearly stated that if one assumes the tractive force at the draw-bar to be the same as the tractive force at the tread of the wheel (as found from a Prony brake test of one of the motors from the locomotive) the efficiency will come out at over 100 per cent.

The torque test described by Mr. Grove was used in the experiments given in Fig. 7, and is represented by curve "C" in the tests of locomotives Nos. 12, 15, and 17. In this case, instead of bolting the iron arm to the wheel (we did bolt the iron on at first, but we found it more convenient to use the following method), we made the rope used for the Prony brake test fast on the rim of the wheel, and used a lever with a spring balance as shown in Fig. 7. I do not quite agree with Mr. Grove that he gets a better result; at least I have found that the values of the magnetic field obtained in this manner in different experiments do not agree with the same accuracy as when the Prony brake is used, and I think this is due to the enormous friction in the bearings when the armature is not allowed to revolve, or is only

allowed to revolve the small amount due to the elongation of the spring of the spring balance. Mr. McMahon.

Mr. GROVE: Allow me to say that this observation was taken when the armature was just on the point of revolving. Mr. Grove.

Mr. McMAHON: I made most careful trials, but found that the results did not come out so well with that method as the other. Mr. Grove, among other speakers, questions the possibility of getting reliable results from 5-second readings. I think I am safe in saying that there were some hundreds of thousands of these 5-second readings taken for the locomotives, and they agreed with wonderful accuracy; and this is borne out by the results set down in Table V., where the calculated and actual distance travelled agree within 1 per cent. It may be a rough and ready test, but a rough and ready test which comes out with only 1 per cent. of error I think may be depended upon for a considerable amount of accuracy. Mr. McMahon.

Regarding Mr. Grove's communicated remarks, the method of shunting the motor fields was fully explained in the paper, and I cannot see how he can assume that any person in the slightest degree acquainted with traction would suggest shunting the motor fields "at the precise instant of starting." His inference is quite correct regarding the changes indicated from *a* to *d*, but I cannot say the same for his statement regarding the enlarging of the motors. There is nothing physically impossible about increasing the armature practically 100 per cent., and absolutely no difficulty in getting an increase of 50 per cent. in the length of the armature core. While on this point I may mention that the length of No. 17 locomotive armatures was increased nearly 20 per cent., and the change on the whole was beneficial. Again, the armatures of No. 21 locomotive are about 33 per cent. longer than those of No 12, and the tractive force at the tread of the wheels of the former is about three times that of the latter. I agree with Mr. Grove's statement regarding radical alterations in the modern traction motor, but then I was not dealing with a modern traction motor.

I am not altogether unfamiliar with the essential points of good motor and dynamo design, and have not, as he assumes, neglected these important considerations in my investigation.

Some of Mr. Grove's criticisms must, I think, arise from a superficial reading of the paper.

Mr. TAYLOR: I should like to ask if Mr. McMahon has integrated the speeds, because I did that over twenty-four very elaborate series of tests published before the British Association, and I did not find one of them agree with the distance traversed except the one from which I took my readings. Mr. Taylor.

Mr. McMAHON: I think, if I remember the tests, that they were 10-second readings. Mr. McMahon.

Mr. TAYLOR: No, they were 5, I think. Mr. Taylor.

Mr. McMAHON: I think you will find they were 10-second readings, as when these tests were made 10-second readings were considered close enough. Mr. Taylor refers to the difference in the values of tractive force found by myself for one of the motors of No. 15 locomotive, and that which I believe was found at the works. In a copy of the works Mr. McMahon.

Mr. McMahon. test which I have the fields were separately excited; but in my case they were not separately excited, so that, I think, may account for the difference. In fact, I was at a loss to find out where the difference lay, until I noticed there were two different currents; one was the armature current, and the other the field current. Mr. Taylor seems to place more reliance on his curve of tractive resistance and speed than on mine.

Mr. Taylor. Mr. TAYLOR: No, I do not.

Mr. McMahon. Mr. McMAHON: Perhaps I put it too strongly. What I wished to point out is that I also tried to arrive at a tractive resistance curve in the same manner, but my results were not successful, and therefore I resorted to the dynamometer method. Mr. Taylor finds the initial value of tractive force from his curve to be 25 lbs. per ton. My value of 40 lbs. per ton at the moment of starting is taken from the test of the locomotive pulling against a stop. I think he must have made some mistake in allowing for gravity, &c., to get such values as 4 or 5 lbs. per ton as against my 10 lbs. Again, the shape of his curve proves that something is wrong, as it is hardly possible for the tractive resistance to jump from 5 lbs. per ton at 13 miles per hour to 15 lbs. per ton at about 13·75 miles per hour. In dealing with the average results between stations I divided my curve into two periods, the starting and running period. Mr. Taylor takes an average over the whole time, and I think in this manner he can account for his 15·6 lbs per ton against my 11·75 lbs. per ton between Kennington and the Oval stations. There is another proof that my speed and tractive resistance curves cannot be very far wrong in the fact that I arrive by experiment at a curve connecting tractive resistance and speed, and I use the values obtained from this curve again in calculating the performance on the line of motors which are not yet built; and when these motors are run on the line the calculated and observed performance diagrams agree, as shown by the figures on the screen, within narrow limits, and any difference may be traced to the fact that the driver did not use his controller as I calculated it was to be used. Mr. Taylor says that he had to resort to "cooking." Perhaps that may account for the difference in the curves.

With regard to Mr. Mordey's remarks as to the difference between the conditions of a street car and a locomotive, I quite agree that there is a difference, but I think that the same condition obtains in a locomotive as in a street car. When you have to set back in coupling up to a train, it is often necessary to move only a fraction of an inch. I have also noticed that difference in sparking, in switching off, say in coming into a station, and breaking the circuit of the dynamo. The reason that no magnetic blow-out is used in this series-parallel controller is due to the fact that the paralleling contacts are not moved until the moment you want to put the motor into parallel, and once the motors are in parallel you leave them in parallel until the current is entirely cut off—you do not take them from the parallel position to the series position while a current is flowing. Mr. Brousson refers to the results on page 508 in the first series of experiments. I did not find much difference for using various starting currents. He said these

experiments were made with a series-parallel controller. But these experiments were made with an ordinary series switch and with a series-parallel controller. As shown in some of the calculated diagrams afterwards, there is a very great difference in the value of the starting current. Mr. McMahon.

Mr. BROUSSON: That is what I meant. Mr. Brousson.

Mr. McMAHON: I understood you to say series switch in the first instance. The reason why an acceleration of 1·46 feet per second was adopted was that we thought from actual working it seemed to be quite high enough, and if the driver stopped at a greater rate than that people complained of being thrown against one another. If they make this complaint when stopping, they will naturally complain of the same thing when starting, so that if a high rate of negative acceleration is objected to, there is not much good in spending energy in starting too quickly. In stopping we sometimes get a negative acceleration as high as 2·25 feet per second; but I think to adopt that acceleration at starting, unless one is compelled to do so by competition, is not economic, as one has to pay too much for it. This is pretty clearly shown by the calculation in Figure 63 in the addendum. This figure shows that, with the 4-motor equipment, an enormously high starting current has to be used to get this acceleration. Mr. Brousson also referred to the droop in the speed curve, or the acceleration not being uniform. I think he referred more particularly to the diagram numbered Figure 37. That was one of the first calculated performance diagrams; in fact it was plotted before one quite knew what the results would be. I think Figure 47, motor 6, shows that the speed line is almost straight, and it can be taken that the acceleration is fairly uniform during the whole accelerating period. We find no locking necessary between the starting and the reversing switches; the main brake switch is close to the series-parallel controller, and the reversing switch is some distance off, so that the driver finds it more convenient to fly to the main switch than to the reversing switch. I think that simplicity in the controlling arrangements is one of the first things which should be aimed at. Mr. Brousson also refers to the consequent poles in the motors of the No. 21 locomotive type. Unfortunately that locomotive is not yet running, and it is difficult to say what the result will be, under test; however, one of the motors behaved very well indeed. There are other locomotives built in the same way, and there is no extraordinary sparking. The commutators do not always run with that delightful blue colour which some people want, but we do not find much difficulty with them. Mr. McMahon.

Professor Smith advocated the use of recording instruments rather than trusting to actual observations. The only experience I had of recording instruments on a locomotive was with recording ammeters and voltmeters in the very early days, soon after the City and London Railway was opened. If we kept the instrument sensitive the pen made a very thick line on the paper, due to the vibration of the locomotive; then the curve got blotted and formed a line almost half an inch thick. Later a more sensitive instrument of the best make was

Mr. McMahon.

tried, and the results were not anything like so accurate as those obtained from 5-second readings. With regard to Professor Smith's suggestion to use a zero method, I am afraid it would prove rather troublesome with a dynamometer in the draw-bar of the locomotive.

To calibrate the dynamometer it is fixed in a cradle attached to the hook of a crane and weights applied to the other end; the gauge indications were observed for different weights, and from these a curve was plotted. After one or two runs the dynamometer was taken off the locomotive, put in the cradle again, and re-calibrated, and we could not find any difference between the two curves. The whole movement on the diaphragm is very small indeed, not more than the eighth of an inch from no pull up to the maximum amount the dynamometer would stand. It is quite true in the earlier experiments I saw difficulties, and the reason I carried those difficulties through in the paper was this. Everybody who gets hold of the paper is not so well up in these matters as Professor Smith. There are many students and others who like to follow the matter through all stages. In many cases one reads a paper written by some eminent authority, and finds that he assumes that his readers know everything about it. I think it is a great pity that they do not give some account of their difficulties and so lead you along. That is what I tried to do. With regard to the 73 per cent. efficiency of the locomotive, to which Professor Smith objects, if he will consider that test refers to a locomotive pulling from a fixed point he will see that the question of acceleration does not come in. I cannot help thinking that if Professor Smith reads that part of the paper carefully again, where he assumes that the effect of gravity and acceleration are not properly treated, he will arrive at the conclusion that everything is in order.

Professor Carus-Wilson refers to the air resistance on the front of a locomotive. That is a very difficult thing to arrive at. We made a sort of wind gauge on the same principle as the locomotive dynamometer, only of course very delicate, and secured it to the front of the locomotive. The amount of vibration in the locomotive made the results unreliable. The pointer of the gauge flew all over the scale, although the wind board was only a very light frame two feet square, covered with canvas. The gauge was then held in the hands of the observer, who sat on the front of the locomotive, and in this manner the effect of vibration was somewhat minimised. The result of four or five tests carried out in this manner indicate that the air resistance in the front of the locomotive itself was between one and one and a half-pounds per square foot at a speed of about 14 miles per hour. I give this result for what it is worth—I doubt it myself, but that is all I have been able to obtain.

These experiments seemed to indicate that a column of air was moving in front of the locomotive. At regular intervals there are cross passages between the two tunnels, and when approaching a cross passage the air pressure went down almost to nothing. After passing the cross passage the pressure rose to the maximum above referred to. This, I *think*, was caused by the cross passage allowing a free outlet for the air

Mr. McMahon. in front of the approaching locomotive, whilst immediately afterwards a fresh column of air was set in motion.

Professor Perry. Professor PERRY: I should think at some future time it would be good to try the locomotive by itself, and find the power required to drive the locomotive at various speeds without a train.

Mr. McMahon. Mr. McMAHON: Then you cannot measure the draw-bar pull so well.

Professor Perry. Professor PERRY: I would only take power and speed.

Mr. McMahon. Mr. McMAHON: We have already done this, but at one speed only. It would, however, I think, be worth trying again as Professor Perry suggests. The reason why we used a larger wheel was to get more clearance between the bottom of the magnets and the working conductor. I think a great many people go to extremes to get another ½ per cent. efficiency in the motor, quite neglecting that it may cost 5 or 10 per cent in the all-round efficiency of the locomotive. The method of shunting the field was calculated. But diagrams Figs. 67 and 68 are the result of experiments, and prove that the method is feasible in practice.

[*Communicated.*] Before Mr. Taylor sent in his communicated remarks, he seems to have read parts of the paper a little more carefully; but I think if he followed my reasoning a little more closely, he would not require the explanation which follows. There is really not a slip at the top of page 539. The 406·3 lbs. is not the difference between the total tractive effort of the motors and the calculated pull due to acceleration and gravity. In this particular test the locomotive and train were running on a down gradient, the current being switched off at a certain point and coasting allowed; the 406·3 lbs. is the total tractive effort due to gravity and kinetic energy stored up in the locomotive and train, and it is perfectly correct to divide by the total weight, viz., 37 tons, the locomotive in this particular case being treated as a carriage as to air resistance, friction, &c. It must be remembered that the current was switched off for the experiment, and therefore any additional friction in the motor bearing due to magnetic pull, &c., can be neglected.

Regarding the disagreement between the columns 2, 3, 4 and 5 of Table VIII., Mr. Taylor does not appear to appreciate the fact that in tests of this nature one can only get at results by taking a large number of tests and from these the average. There are a number of small points which cause the tractive resistance to vary considerably, and as an instance I may mention that the condition of the couplings between the carriages have a very important bearing on this point. If the couplings on one train are screwed up tighter than another, the tractive resistance of the first will appear a little higher than the second, and this is enough to account for the disagreement in many cases. The same remark applies to column 2 of Table II. The apparent mistake on page 528 is really Mr. Taylor's, because he scales one curve and reads my value which refers to another. In Table II. are given two sets of tests starting from Kennington, and the figure 350 lbs. which Mr. Taylor thinks is correct, I give in the observation as 346 lbs., and not 201 lbs., the latter figure referring to the first set of figures given in the table.

In refe to Mr. Taylor's remarks regarding column 4, Table II., re are h dealing with the tractive force at the tread of the wheel what I have called by the rather misleading title, tractive force at the raw-bar) of the locomotive, and the force due to the acceleration of he train does not concern us. The actual pull being measured by the ynamometer, the tractive force due to acceleration ± gravity (for the comotive alone) must be added to the value found from the dyna-ometer in order to get the total tractive force given out by the comotive on the level at constant speed. As regards the curve E, the act that this curve runs almost parallel to the Prony brake curve A, shows that my average values of tractive force for the current are not far wrong.

The PRESIDENT: I am sure that you will all thoroughly agree that we are greatly indebted to Mr. McMahon for having brought before us at a very opportune moment this most interesting and valuable paper, representing as it does an immense amount of painstaking work on a subject of very great importance. Up to the present time we have been singularly backward in the use of electricity as a means of traction, but now we appear to be making a new departure. I noticed that the Chairman of the House of Commons Committee dealing with the question of the London railways asked a witness this question: "Are there not a large number of underground electric railways in the air at this moment?" It conveyed the fact that we do appear to be waking up to the advantage of electric traction, and this paper of Mr. McMahon's comes opportunely. It has elicited a most useful discussion, and altogether constitutes a very important addition to our proceedings. I ask you, gentlemen, to give expression in the usual way to your thanks to Mr. McMahon.

The resolution was carried by acclamation.

The PRESIDENT: I have to announce that the scrutineers report the following candidates to have been duly elected:—

Member:

Winder Elwell Goldsborough.

Associate Members:

Felix John Howitt. | Edward Badham Thornhill.

Associates:

Cyril George Mancha Bennett. | Walter Joseph Higley.
William Augustus Harris. | James Noel Cairncross Holroyde.
Newell Hosgood.

Students:

Athol Garnett Elliott. | Reginald W. Martin.

The Twenty-Seventh Annual General Meeting of the Institution was held at the Society of Arts John-street, Adelphi, on Thursday evening, May 25th, 1899, Mr. J. W. SWAN, F.R.S., President, in the chair.

The minutes of the Ordinary General Meeting held on May 18th, 1899, were read and approved.

The names of new candidates for election into the Institution were announced.

The following transfers were announced as having been approved by the Council :—

From the class of Associates to that of Members—

Walter John Murphy. | Henry Wall Wilkinson.

From the class of Associates to that of Associate Members—

Lawrence Birks.
C. B. Callow.
J. Enright.
W. Jamieson.
G. J. T. Parfitt.
Geo. Armstrong Pearson.
Keith Robinson.

From the class of Students to that of Associates—

John Robert Craig.
Frank Graham.
Edward Audrey Short.
Frank Trist Woodley.

Messrs. C. T. Mitchell and E. G. Phillips were appointed scrutineers of the ballot for the election of members.

The PRESIDENT : I will now ask the Secretary to read the Council's report of the business of the Session.

The SECRETARY read the Annual Report of the Council, as follows :—

REPORT OF THE COUNCIL TO THE ANNUAL GENERAL MEETING, MAY 25TH, 1899.

PRELIMINARY.

In accordance with No. 57 of the new Articles of Association, to which allusion will be made later, the date of the Annual General Meeting has been changed from December to the end of the session, and in conformity with No. 1 of the Articles of Association no Annual General Meeting was held in December, 1898.

The period covered by the present Report extends, therefore, over a space of seventeen months, commencing with the Annual General Meeting of the 9th of December, 1897.

ELECTIONS AND TRANSFERS.

The Council, exercising the right conferred upon them by the Articles of Association of electing one Honorary Member annually, had, in 1898, the satisfaction of announcing the election of Mr. Henry Wilde for that year. Under the Articles of Association in force prior to 1899, continuance in professional practice was a disqualification for Honorary Membership; but with the introduction of the new Articles this disqualification ceased to exist, and the Council were gratified by the consent of Lord Kelvin, the senior Past-President of the Institution, to accept nomination as the first Honorary Member under the new rules.

A new professional class (that of Associate Members) has been created this year, and elections and transfers to this class are now, for the first time, recorded in the Annual Report.

The total of the additions to the register during the period covered by the Report has been 667, comprising 2 Honorary Members, 37 Members, 29 Associate Members, 11 Foreign Members, 354 Associates, and 234 Students; and 36 candidates have been approved for ballot to-night.[1]

104 Associates have been transferred to the class of Members, and 412 to that of Associate Members, whilst 109 students have been transferred to the class of Associates.

DEATHS AND RESIGNATIONS.

It is with deep regret the Council have to report that the death roll for the past seventeen months is longer than it has ever been previously, even when the unusual duration of the period under report is taken into account, and that it includes the names of some of the oldest and most distinguished members.

[1] As these candidates were all duly elected, the numbers representing additions to the register since January 1st, 1898, were, at the end of the meeting, as follows: Honorary Members, 2; Members, 38; Associate Members, 35; Foreign Members, 6; Associates, 370; and Students, 246; making a total of 703.—ED.

The Institution has in this time lost 41 of those who were on the register at the date of the last report, viz. :—2 *Past-Presidents*, Latimer Clark and Dr. John Hopkinson ; 3 *former Members of Council*, Sir James Douglass, Captain Sir Douglas Galton, and Colonel R. Raynsford Jackson ; 1 *Local Honorary Secretary*, P. Christian Dresing ; and 11 *other Members*, viz., Sir W. Anderson, Lord Sackville Cecil, W. H. Davies, R. O. G. Drummond, R. P. Eidsforth, D. W. Gott, R. S. Hampson, W. T. Newitt, Dr. E. Obach, Colonel H. L. Wells, R.E., and G. K. Winter ; 4 *Foreign Members*, J. Kruesi, E. Lacoine, O. B. Shallenberger, and C. H. Summers ; 18 *Associates*, H. H. Beit, S. H. Blake, G. A. Boettger, J. Brown, W. H. Brownlee, H. E. Carr, T. B. Groves, W. E. Hayne, W. B. Hutchinson, G. A. Jones, J. King, A. McGeoch, J. P. McGregor, F. Mercer, W. Robinson, J. Ross, E. Rothwell and S. Trott ; and 2 *Students*, C. V. Morgan and C. P. Walker.

It was with satisfaction that, on the occasion of the death of Dr. John Hopkinson, the Council received a telegram expressing the sympathy of the Associazione Elettrotecnica Italiana, and from the American Institute of Electrical Engineers and the South African Society of Electrical Engineers letters conveying special notes of condolence. These messages were regarded, not only as tributes to our revered Past-President, and as evidences of the world-wide acknowledgment of his professional and personal distinction, but as proofs of the catholicity of the sympathy between sister Institutions, and of the fellow-feeling that exists between Electrical Engineers in all lands.

Seven Members, 4 Foreign Members, 30 Associates and 10 Students have resigned during the period under review.

PAPERS.

In addition to the valuable and interesting inaugural address of the President, the following papers have been read during the seventeen months under review :—

DATE. 1898.	TITLE.	AUTHOR.
Jan. 27.—	Notes on the Electro-chemical Treatment of Ores containing the Precious Metals	Maj.-Genl. C. E. WEBBER, C.B., Past-President.
Feb. 9.—	An Electrolytic Process for the Manufacture of Parabolic Reflectors	S. COWPER-COLES, Member.
" 24.—	On the Manufacture of Lamps and other Apparatus for 200 volt circuits	G. BINSWANGER BYNG, Member.
Mar. 24.—	The Cost of Generation and Distribution of Electrical Energy	R. HAMMOND, Member.
April 28.—	Earth Returns for Electric Tramways	H. F. PARSHALL, Member.
"	Note on Electric Tramways	Major P. CARDEW, R.E., Member.
April 28.—	Note on Return Feeders for Electric Tramways	A. P. TROTTER, Member.

May 5.	—The Prevention of Interruptions to Electricity Supply	L. ANDREWS, Associate.
„ 12.	—A Magnetic Balance for Workshop Tests of Permeability	Prof. J. A. EWING, Member
	The Registration of Small Currents used for Electric Lighting or other Purposes	A. H. GIBBINGS, Member.
„ 26.	—The Design of Electric Railway Motors for Rapid Acceleration..	Prof. C. A. CARUS-WILSON, Member.
v. 10.	—Rotatory Converters	Prof. S. P. THOMPSON, Vice-President.
c. 8.	—Improvements in Magnetic Space Telegraphy	Prof. OLIVER LODGE, Member.
„ 12.	—Electric Intercommunication in Railway Trains	W. E. LANGDON, Vice-President.
„ 22.	—Telegraphy by Magnetic Induction	S. EVERSHED, Associate.
	Ætheric Telegraphy	W. H. PREECE, C.B., Past-President.
1899.		
Jan. 26.	—Rules for the Regulation of the Wiring of Premises for connection to Public Supply Mains	J. PIGG, Associate Member.
	The Regulation of Wiring Rules	C. H. WORDINGHAM, Member.
	The Institution Wiring Rules	R. E. CROMPTON, Past-President.
Feb. 16.	—Electric Traction by Surface Contacts	MILES WALKER, Associate.
Mar. 2.	—Wireless Telegraphy	G. MARCONI, Member.
„ 9.	—The Wehnelt Electrolytic Contact Breaker (Demonstration)	A. A. CAMPBELL SWINTON, Member.
	The Induction Motor..	Prof. E. WILSON, Member.
„ 23.	—The Hissing of the Electric Arc	Mrs. AYRTON.
April 13.	—Experiments on Alternate Current Arcs by aid of Oscillographs (paper published in February, and discussed April 13).	W. DUDDELL and E. W. MARCHANT, Associates.
„ 27.	—Capacity Measurements of Long Submarine Cables	J. ELTON YOUNG, Member.
May 4.	—Electric Locomotives in Practice, and Tractive Resistance in Tunnels, with Notes on Electric Locomotive Design	P. V. MCMAHON, Member.

The average of the attendances at Meetings has been much higher than usual. The number of members and visitors who sought to gain admission to the Meeting of March 2nd was so greatly in excess of the accommodation available, as to necessitate the repetition of the paper and demonstration.

An unusually large number of valuable papers having been presented, the Council decided that, with the consent of the authors, several of the communications should be taken as read, or be read only in abstract, so that more time should be available for the discussions.

The illustration of papers by experiments has added greatly to the interest of many of the Meetings, and the excellence of these demonstrations has formed a special feature of the proceedings of the past session.

The Publications of the Institution.

The principal change to be recorded in the publications of the Institution is the issue without extra charge of a separate volume of Abstracts to every member entitled to receive the publications, and the consequent omission of abstracts from the Journal. This change had been in contemplation for some time, and a Committee of the Council had, in collaboration with members of the Physical Society of London, drawn up a scheme for the issue of a joint volume of Abstracts by the two Societies, whereby the duplication of abstracts by the two bodies would be avoided.

It was seen that by this co-operation every member of each Institution would have at his disposal a much more complete summary of, and index to, current work in Electrical Engineering and Electrical and Physical Science than was hitherto available, and that at the same time the efficiency of the publications for a given outlay would be greatly enhanced. A joint Committee, consisting of representatives of the two Institutions, was therefore formed, and, with Mr. Swinburne as Editor, the separate issue of *Science Abstracts* was commenced in January, 1898. The publication has since then appeared at monthly intervals, and has, the Council believe, presented a reasonably complete summary of the principal papers published in Europe and America in the various departments of Electrical Engineering and of Physical Science. In January last, Mr. W. R. Cooper succeeded Mr. Swinburne in the office of Editor of the Abstracts.

The cost of the publication has been borne for the most part by the two Institutions principally concerned, their respective contributions having been determined in accordance with the report of the joint Committee originally appointed in 1897. The Institution of Civil Engineers, the Royal Society, and the British Association have, however, recognised the value of *Science Abstracts* as a work for general reference, and have accordingly contributed towards the expenses of publication.

With reference to the Journal, it is to be noted that the increase in the number of papers and discussions has more than accounted for the pages devoted in previous years to Abstracts, so that the volume of Proceedings published in 1898, although it contains no Abstracts, has over 120 pages in excess of that published in 1897.

The following papers appear in the Journal as Original Communications:—

In Part 133.—Sparkless Reversal in Dynamos	By H. N. Allen.
In Part 136.—Feeder Machines	By Reginald Wood, Associate.
Electric Welding of Tramway Rails ..	By Reginald J. Wallis-Jones, Member.
In Part 138.—Note on Electric Traction by Three-Phase Alternating Current..	By E. Kilburn Scott, Associate Member.
In Part 139.—Note on Standard Frequency	By L. B. Stillwell, Member.
In Part 140.—A Rotary Converter	By Prof. E. Wilson, Member.

Annual Premiums.

The number and value of the Premiums have this year been increased as announced in the last Report, and the Council have made the following awards for papers read during the Session 1897–98; the premiums being presented at the first meeting held in the year 1898, viz.:—

Premiums for 1897–98.

The Institution Premium, value £25,

to Mr. H. F. Parshall, Member, for his paper on "Earth Returns for Electric Railways."

The "Paris Electrical Exhibition Premium," value £10,

to Mr. R. Hammond, Member, for his paper on "The Cost of Generation and Distribution of Electrical Energy."

An Extra Premium, value £5,

to Mr. Leonard Andrews, Member, for his paper on "The Prevention of Interruptions to Electricity Supply."

A Premium, value £10,

for an "Original Communication," to Mr. H. N. Allen, for his paper on "Sparkless Reversal in Dynamos."

The Senior "Students' Premium," value £10,

to Mr. J. M. Donaldson, Student, for his paper entitled "Notes on the Dover Tramways."

The second "Students' Premium," value £5,

to Mr. M. Solomon, Student, for his paper on "Hertz Waves and Wireless Telegraphy."

The third "Students' Premium," value £5,

to Mr. E. E. Tasker, Student, for his paper on "Alternating Current Motors."

The "Fahie Premium" was not awarded, as no paper had been read during the Session 1897–98 on the subject of telegraphy or telephony.

The following awards have been made for the Session 1898–99, and the premiums will be presented in November at the first meeting of the coming Session, viz.:—

Premiums for 1898–99.

The "Institution Premium," value £25,

to Mr. P. V. McMahon, Member, for his paper on "Electric Locomotives in Practice, and Tractive Resistance in Tunnels, with *Notes* on Electric Locomotive Design."

The "PARIS ELECTRICAL EXHIBITION PREMIUM," value raised to £20,

to Mr. W. DUDDELL and Mr. E. W. MARCHANT, Associates, for their paper entitled, "EXPERIMENTS ON ALTERNATE CURRENT ARCS BY OSCILLOGRAPHS."

Two "FAHIE PREMIUMS," value £10 each,

none having been awarded in 1898, one to Professor OLIVER LODGE, Member, and one to Mr. G. MARCONI, Member, for their papers entitled respectively, "IMPROVEMENTS IN MAGNETIC SPACE TELEGRAPHY," and "WIRELESS TELEGRAPHY."

Two EXTRA PREMIUMS, value £10 each,

one to Mrs. AYRTON for her paper on "THE HISSING OF THE ELECTRIC ARC," and the other to Mr. J. ELTON YOUNG, Member, for his paper on "CAPACITY MEASUREMENTS OF LONG SUBMARINE CABLES."

The Senior "STUDENTS' PREMIUM," value £10,

to Mr. W. G. ROYAL-DAWSON, Student, for his paper on "ALTERNATING CURRENTS OF VERY HIGH FREQUENCY."

The second "STUDENTS' PREMIUM," increased in value to £10,

to Mr. M. GARDNER and Mr. R. P. HOWGRAVE-GRAHAM, Students, for their paper on "THE SYNCHRONISING OF ALTERNATORS."

The third "STUDENTS' PREMIUM," value £5,

to Mr. LEONARD WILSON, Student, for his paper on "THE EFFECT OF GOVERNORS ON THE PARALLEL RUNNING OF ALTERNATORS."

An extra "STUDENTS' PREMIUM," value £4,

to Mr. L. R. MORSHEAD, Student, for his paper on "ENCLOSED ARC LAMPS."

An extra "STUDENTS' PREMIUM," value £3,

to Mr. H. M. DOWSETT, Student, for his paper on "ELECTRICITY METERS."

SALOMONS SCHOLARSHIP.

The Council in 1898 awarded Salomons Scholarships of £50 each to Mr. TOM ROLLS RENFREE, of King's College, and to Mr. H. J. TOMLINSON, of University College, London. The award for 1899 has been made in favour of Mr. H. J. THOMSON, of the Central Technical College.

STUDENTS' CLASS.

1 cter of the papers read at the Meetings of the Students' een maintained at the higher level of the last few sessions, ne lance of Students has shown marked improvement. The of a ial lantern by Mr. Edmunds has added to the facilities for illustration of papers, and so contributed much to the success of ing

he following places were arranged for the Students by y during the past seventeen months, viz. :—

The ric Lighting Station of the St. Pancras Vestry.
" " " Shoreditch Vestry (two visits).
The Manchester Square Station of the Metropolitan Electric Supply Company.
The Bankside Station of the City of London Electric Lighting Company.
The works of Messrs. Easton, Anderson, and Goolden (two visits).
" " the Electric Welding Company (two visits).
" " the Incandescent Electric Lamp Company.
" " the Langdon-Davies Electric Motor Company.
" " the Central London Railway.
" " Messrs. Siemens Bros. and Company.
" " the W. T. Henley's Telegraph Works Company.
The Davy-Faraday Laboratory of the Royal Institution.

The Council attach great importance to these visits, on account of their educational value, and desire to record their high appreciation of the facilities, generously afforded by the various firms and engineers concerned, of visiting the works under their control.

ANNUAL DINNER.

The Annual Dinner was again held in the Hotel Cecil, the date being the 7th of December, and the number of members present was in excess even of those at the Annual Dinner of 1897.

ANNUAL CONVERSAZIONE.

By the kind permission of the Trustees of the British Museum, the Conversazione was again given in the Natural History Museum, and the attendance of members was the largest on record.

ANNUAL ACCOUNTS AND FINANCIAL POSITION.

In accordance with the special Resolutions amending the Articles of Association, the beginning of the Financial Year is now made once more identical with the date at which the annual subscriptions fall due. The Statement of Accounts and Balance Sheet this year represent,

therefore, the financial position of the Institution on December 31st, instead of on September 30th, 1898, as would have been the case under the old Articles. Hence the period covered is fifteen months instead of twelve months, a fact which renders it difficult to compare the Statement of Accounts now published with that issued with the last Report, for whereas expenditure is comparatively evenly distributed over the different quarters of the year (although that of the last quarter may be, and in this case is, above the average) the income, so far as it is derived from subscriptions and entrance fees, is comparatively low in the autumn quarter, because the majority of subscriptions are paid early in the year, and there are relatively few elections in November and December.

The sum of £725 5s. 11d. has been invested on account of life compositions, leaving on December 31st a balance of £173 0s. 6d., which has since been invested.

Taking into account the relatively heavy expenditure incurred on account of *Science Abstracts* and premiums, the necessary increase in expenses in other directions (towards meeting which, however, the abnormally large sum paid in respect of entrance fees has contributed greatly), your Council have every reason to be satisfied with the financial position of the Institution.

The estimated realisable amount of subscriptions outstanding on the 31st of December, 1898, is £475, and none of this is taken into account in the Balance Sheet now presented. This amount should, of course, be less than if the Report were made as for the 30th of September. More than £345 of this £475 has already been received.

Building Fund.

Following the precedent of previous years the Council have transferred £500 to the Building Fund, so that, with the dividends received during fifteen months, the total amount standing to the credit of the Fund on December 31, 1898, was £5,277 0s. 6d., against £4,639 13s. 7d. on September 30, 1897.

Local Honorary Secretaries.

The Council greatly regret that, by the death of Mr. P. Christian Dresing, the Institution has been deprived of the services of a valued Local Honorary Secretary. They have appointed Mr. J. L. W. V. Jensen in his place as Local Honorary Secretary and Treasurer for Denmark.

The return of Professor C. A. Carus-Wilson to England left a vacancy in the Honorary Secretaryship for Canada; this has been filled by the appointment of Mr. Hartley Gisborne; and Mr. M. G. Simpson has succeeded Mr. J. J. Allen as Local Honorary Secretary for India.

The Council wish here to record their grateful thanks for the services rendered by Mr. Allen and Professor Carus-Wilson, since their appointment in 1892.

Address to the Queen.

On the occasion of Her Majesty's Birthday this year, the Council unanimously resolved—

"That a humble address from the Institution be laid before her Majesty the Queen in terms of congratulation on the completion of the eightieth year of her Majesty's age, together with an expression of hope that she may long be preserved to reign over these realms."

The Articles of Association.

The desirability of making certain changes in the rules of the Institution had been under discussion for some time, and in the early part of the period under report the Council, in considering these proposals, determined to examine the whole of the Articles of Association, and to recommend to the Institution for adoption such alterations as might appear to be desirable. A Committee of the whole Council and two Sub-Committees appointed thereby held many meetings, and finally made their Report. The Articles were re-drafted so as to embody the new proposals; the draft was then submitted to Special General Meetings of Members held on the 3rd and the 18th of November, and was approved by them. A Special Resolution was duly passed, and the new Articles of Association came into operation on the 1st of January, 1899.

The new Articles have been published, and full reference was made to them by the President at an Ordinary General Meeting. It is therefore unnecessary here to enter into detail as to all the changes adopted. A few special points demand notice in this Report.

First, as to the qualifications of Members, the Council being anxious to raise the status of membership, had, for some time past, interpreted with increasing stringency the clauses relating thereto in the then existing Articles of Association; and accordingly the corresponding clause in the new Articles was so drafted as to make it clear that high professional qualifications are required of candidates for admission to this class. The qualifications demanded of Associates were, however, left practically unchanged, and it was believed that the status of this class would therefore remain as before, and that the Associate class would, as has always been the case, include, not only professional Electrical Engineers, but others whose connection with Electrical Engineering is less intimate.

On account of the higher qualifications demanded of those seeking admission to the class of Members, it seemed desirable to form an intermediate class, which should enable these Associates who are qualified to do so, to join a class intended to include only Electrical Engineers and Electricians at an age earlier than would otherwise be possible. The professional class of Associate Members was therefore created, corresponding with the class similarly designated in certain other engineering institutions, and the number of elections and transfers made to this new class during the first five months of 1899 amounts to 441.[1]

[1] Or, including those elected at this meeting, 447.

With the expansion of the work of the Institution there has been, of necessity, an increased expenditure to provide for, and concurrently there has been a diminution in the interest obtainable on capital to be invested. The fees payable for life composition no longer yielded a return commensurate with the expenditure incurred per member, it was therefore found necessary to raise the fee to 40 guineas, but the position of those who had already compounded was left undisturbed, whilst it was provided that the new rule would not come into operation until the date of this Annual General Meeting in the case of Foreign Members, Members and Associates who were on the register on December 31st, 1898. Similarly, it was found necessary to raise the subscription of Students from 10s. 6d. to one guinea per annum.

In addition to the changes already referred to in this Report, it should be mentioned that provision is now made for the formation of Local Sections for the holding of meetings at populous centres outside London, also for the issue of diplomas to Members and Associate Members, and for the use of a common seal.

The Council wish to place on record its appreciation of the laborious service generously rendered to the Institution by the Honorary Solicitors in the execution of the difficult and responsible task of remodelling the Articles of Association.

The Secretaryship.

It was mentioned in the last Report that Mr. Webb would retire from the Secretaryship on the 12th of February, 1898, and that Mr. McMillan would take over the duties of the office on that day. The Council have now to report that the change was duly made, and they take this opportunity of expressing once more their appreciation of the services rendered to the Institution by Mr. Webb during his twenty years of official work.

THE LIBRARY.

THE REPORT OF THE SECRETARY.

I have to report that the accessions to the Library during the year numbered 82; all of these were kindly presented by the authors or publishers.

The supply of specifications of electrical patents and that of abridgments of specifications relating to electricity and magnetism are continued by the kindness of H.M. Commissioners of Patents, and an arrangement has now been made whereby the specifications of all electrical patents published during any week are placed on the Library table on the following Monday morning.

The periodicals or printed proceedings of other societies received regularly are, with some additions, the same as last year, as may be seen by the list appended hereto.

The number of visitors to the Library in the seventeen months from January 1, 1898, to the date of the Annual General Meeting has been 1,132, of whom 95 were non-members. The corresponding numbers for the twelve months ending December 31, 1897, were 715 and 78 respectively.

APPENDIX TO SECRETARY'S REPORT.

TRANSACTIONS, PROCEEDINGS, &c., RECEIVED BY THE INSTITUTION.

ENGLISH.

Asiatic Society of Bengal, Journal and Proceedings.
Cambridge Philosophical Society.
Engineering Association of New South Wales.
Greenwich Magnetical and Meteorological Observations.
Institute of Patent Agents, Transactions.
Institution of Civil Engineers, Proceedings.
Institution of Mechanical Engineers, Proceedings.
Iron and Steel Institute, Proceedings.
King's College Calendar.
Liverpool Engineering Society, Proceedings.
Municipal Electrical Association, Proceedings.
Northern Society of Electrical Engineers, Proceedings.
Physical Society, Proceedings.
Royal Dublin Society, Transactions and Proceedings.
Royal Engineers' Institute, Proceedings.
Royal Institution, Proceedings.
Royal Meteorological Society, Proceedings.
Royal Society, Proceedings.
†Royal Society, Philosophical Transactions.
Royal United Service Institution, Proceedings.
Society of Arts, Journal.
Society of Chemical Industry, Journal.
Society of Engineers, Proceedings.
South African Society of Electrical Engineers, Proceedings.
University College Calendar.

AMERICAN AND CANADIAN.

American Academy of Science and Arts, Proceedings.
American Institute of Electrical Engineers, Transactions.
American Philosophical Society, Proceedings.
Canadian Society of Civil Engineers, Transactions.
Engineers' Club of Philadelphia, Proceedings.
Franklin Institute, Journal.
John Hopkins University, Circulars.
Library Bulletin of Cornell University.
Nova Scotia Institute of Science, Proceedings.
Ordnance Department of the United States, Notes.
Technology Quarterly.
Western Society of Engineers, Journal.

† Presented by Professor D. E. Hughes, F.R.S. (Past-President).

BELGIAN.

Association des Ingénieurs Électriciens sortis de l'Institut Électro-Technique Montefiore, Bulletin.
Société Belge d'Électriciens, Bulletin.

DANISH.

Den Tekniske Forenings Tidsskrift.

FRENCH.

Académie des Sciences, Comptes Rendus Hebdomadaires des Séances.
Société Française de Physique, Séances.
Société des Ingénieurs Civils, Mémoires.
Société Internationale des Électriciens, Bulletin.
Société Scientifique Industrielle de Marseille, Bulletin.

GERMAN.

Verein zur Beförderung des Gewerbfleisses, Verhandlungen.

ITALIAN.

Associazione Elettrotecnica Italiana, Atti.

RUSSIA.

Section Moscovite de la Société Imperiale Technique Russe.

LIST OF PERIODICALS RECEIVED BY THE INSTITUTION

ENGLISH.

Cassier's Magazine.
Electrical Engineer.
Electrical Review.
Electrician.
Electricity.
Engineer.
Engineering.
English Mechanic and World of Science.
Illustrated Official Journal, Patents.
Indian and Eastern Engineer.
Industries and Iron.
Invention.
Lightning.
Mechanical Engineer.
Nature.
Philosophical Magazine.

AMERICAN.

Electrical Review.
Electrical World and Electrical Engineer.
Electricity.
Journal of the Telegraph.
Physical Review.
Scientific American.
Street Railway Journal.
Western Electrician.

AUSTRIAN.

Zeitschrift für Elektrotechnik.

FRENCH.

Annales Télégraphiques.
L'Éclairage Électrique.
L'Électricien.
L'Industrie Électrique.
Journal de Physique.
Journal Télégraphique.

GERMAN.

Annalen der Physik und Chemie.
Beiblätter zu den Annalen der Physik und Chemie.
Electrotechnischer Anzeiger.
Electrotechnische Zeitschrift.
Zeitschrift für Instrumentkunde.

ITALIAN.

Ellettricità.
Giornale del Genio Civile.
Il Nuovo Cemento.

SPANISH.

La Ingenieria.

The PRESIDENT: To-night we reach the end of the longest and fullest session in the history of the Institution. Of the results of that session you have just heard a report, so complete that but little is required from me either in the way of supplement or comment. You will have gathered from it that the session has been in many respects exceptional. Its close marks the beginning of a new order of things—a new departure in the time of the opening and closing of future sessions, and in the period at which the accounts are made up.

It has seen the first working of the new rules embodied in the new Articles of Association. So far as there has been experience of the operation of these changes there is good reason for believing that in every instance the new rules have acted beneficially, and that the advantages you aimed at obtaining will be realised.

The creation of the new class of Associate Members finds complete justification in the large number—the Secretary says 441, but I trust that before we depart to-night that number will be enlarged by the election of the candidates for admission to that class—who are to be balloted for to-night. The greater stringency exercised under the new rules with regard to the qualification for admission to the grade of Member, and to that of Associate Member, has not tended in any degree to check the growth of the Institution, but, as was hoped, has led to a higher appreciation of membership, and to an increase in the number of candidates. As the Report has told you, the increase that has taken place in membership is much greater than ever before in an equal period, even if the period be reckoned as a double one (and it is nearly so). Counting all grades, we have added 667 members as against 216 last year—that is, a triple increase for less than a double period. But if

the session is exceptional in the number of members we have gained, it is equally exceptional in the number and eminence of those we have lost. We have heard the long catalogue read out, and we recall with a fresh pang of regret how many illustrious names are included in it—names of those whose genius and attainments shed enduring lustre on the Institution they helped so well to build up and sustain.

The premiums, you will observe, are this year awarded largely in recognition of remarkable communications in the department of Telegraphy. The subject of *Wireless Telegraphy*, which, as Dr. Lodge says, "it insists on calling itself," was dealt with in four most able and instructive papers; and in the valuable paper of Mr. Elton Young, a branch of the very important subject of *Submarine Telegraphy* received attention, though not perhaps all the attention it deserves, for it came at the end of the session when there was some slight sense of overcrowding. The Fahie premium, which was not awarded last year for want of a suitable telegraphic paper, has this year been awarded in duplicate. The Salomons Scholarship Fund has, during this long session, been sufficient to provide for a third scholar, University College, King's College, and the Central Institute each nominating a fitting recipient.

On the character of the papers read during the session it is not possible to speak at length. But you will agree with me in the opinion that we have never before in any session had a series of communications so numerous, so varied, and, as a whole, so important, whether regarded from the scientific point of view, or the practical. A marked feature has been the beautiful manner in which several of the papers were illustrated. I refer more particularly to those of Mrs. Ayrton and of Messrs. Duddell and Marchant. In the department of practical electrical engineering we had at the beginning of the session a very excellent paper from Mr. Hammond, and at the end of the session a no less excellent paper of the same type, consisting of records of actual experience in practical working on a large scale, in the paper of Mr. McMahon. It was no easy matter this session to make due recognition of meritorious papers by premium awards; there were so *many more* deserving such recognition, than there were awards to be distributed among them. One help out of the difficulty was to consider certain of the papers *hors de concours* by reason of the official position held, either now or formerly, by the authors. The papers to which I refer will be easily recognised. We have not yet seen any result from the facilities given through the new rules for the formation of provincial branches. I trust, however, we shall see results in the near future. The idea of a yearly excursion of the Institution to one of the large provincial towns, or to some locality of special engineering interest abroad, has, you know, this year taken shape in the project of a gathering at Zürich, of which you have had particulars. I am glad to be able to announce that a sufficient number of members have signified their intention to take part in that excursion to ensure its realisation. I wish and hope that it may bring to all those who join in it a large measure both of profit and of pleasure, and be the forerunner of many meetings of the kind.

Altogether, gentlemen, you will see that the Institution is in a flourishing condition ; its business, already large, is an ever-increasing quantity. We have held countless Committee Meetings, and not a few Council and General Meetings. And yet I must confess to a feeling of dissatisfaction at the comparatively poor result of our activity looked at from a national standpoint. In several of the larger branches of electrical engineering, especially those of railway and tramway construction, we have not held our own either abroad or at home. Looking at the forward position we formerly occupied in relation to railway construction and equipment all over the world, and to the widely different position we hold in relation to the modern electrical development of railway and tramway construction and equipment, it seems to me that, much as we are doing, we are not doing enough, and that it is an urgent matter for us to discover the cause of our backwardness, and to apply the remedy. And the sooner we do this, the better for our country's welfare and our own. I now beg to move : "That the Report of the Council as just read be received and adopted, and that it be printed in the Journal of the Proceedings of the Institution." I shall be glad to hear any remarks on the subject of the Report.

Mr. C. O. GRIMSHAW : I beg to second the motion for the adoption of the Report.

The motion was then put and carried *nem. con.*

The PRESIDENT : You have each received a copy of the Annual Statement of Accounts, and I think you will agree that it is not necessary to have that statement read. I propose, therefore, that we take the accounts as read.

The motion having been put was agreed to.

The PRESIDENT : The accounts are presented in the same form this year as last ; they have been examined by your auditors and found to be correct. You will observe that, whereas last year's accounts covered a period of twelve months, the accounts before us to-night cover a period of fifteen months. This difference arises from the change you made in the rules, regulating the sessional period, and according to which the accounts are now made up to the end of the year, instead of, as they were last year, to the 30th of September. This difference is, of course, exceptional, and in future you will have the twelve months' accounts made up to the 31st of December of each year. This exceptional feature in the session's accounts to some extent masks the actual position, and makes it appear not quite so good as it really is—for while we have the outgoings proportional to the fifteen months' period, we have little more than the income belonging to twelve months, for the subscriptions fall due in January.

Since the accounts were closed, the receipts have been so large that your Treasurer has been able to invest a sum of over £1,450, the proceeds chiefly of the subscriptions due in January and life compositions. I think there is every prospect that the Chancellor of our Exchequer will be able next year to present to you a budget even better than that we have before us to-night, and that, I think you will agree, is a very good one. It shows a state of healthy growth, of progress, and of prosperity. The amount received on account of new

BALANCE SHEET, 31ST DECEMBER, 1898.

LIABILITIES.	ASSETS.

subscriptions and entrance fees is considerable. Next year we shall equally feel the benefit of the increased revenue arising from increased membership, and also from the changes made in the amount of individual subscriptions under the new rules. On the other side of the account, there are several items showing increased expenditure. Retiring Allowance is an extra: it arises out of the action you took last year in connection with the retirement of Mr. Webb and the appointment of Mr. McMillan.

There is a rather large increase of expenditure under the heads of "General Printing and Stationery" and "Postages of Journals and Notices of Meetings." These items account for over £250 of the increase. This is consequent upon the increased number of meetings, both of the ordinary kind and also of those extraordinary meetings—and the printing incidental to them—necessitated by the new Articles of Association, and to enormously increased correspondence. Then there is an item of extra expenditure under the heading of "Contribution to *Science Abstracts*," amounting to £624 17s. 3d. This is, no doubt, a large amount, but it represents an expenditure which, in one form or another, I am afraid we cannot avoid, that is to say, cannot avoid if we do our duty—as I hope we always shall do it—to the general body of our members; not only those resident in the neighbourhood of London, but those who are in the Provinces and in foreign lands, and who consequently cannot attend our meetings, and only come into close contact with us and receive much benefit from us through our publications.

The discontinuance of the publication in the body of the Journal of abstracts of the electrical work going on all over the world, is not a consequence of any want of appreciation of the value of such abstracts—it is rather the result of a belief entertained by the Council that these abstracts possess, for a very large class of our members, a value that can hardly be over-estimated. If our members are to be kept up to date in relation to scientific progress, abstracts we must have, and they must be as full and complete as possible. But after that point is quite settled, there still remains the question how best to obtain and publish these abstracts. That question is not quite settled; it is still engaging the consideration of the Council, and I feel sure you may safely leave it in their hands to do the best that is possible for the Institution. As stated in the Report, we last year got our abstracts through the medium of a separate publication—*Science Abstracts*—and we have arranged to get them in that way to the end of this year. I am not sure what may be done after that—whether an arrangement of that kind may be continued, or whether we shall revert to our former practice of publishing abstracts along with our Journal. In any case, I feel sure there will be no falling off in this part of the publication of the Institution, and therefore I am afraid we cannot, in the future, look for any large diminution of expense under the head of "Abstracts." If we publish them ourselves and do it with the thoroughness necessary to meet your wants, the expense cannot be much less than that incurred on this account this year. You will notice that among the extra expenses of the year, an unusually large sum has been devoted to

premiums. Finally, you will be glad to see that notwithstanding the numerous and heavy additional charges I have called attention to, it has been found possible to add £500 to the Building Fund. I now move : "That the Statement of Accounts and Balance Sheet for the fifteen months ending December 31st, 1898, as just presented, be received and adopted."

Mr. E. TREMLETT CARTER : I have much pleasure in seconding the motion. From a personal perusal of the Statement of Accounts I am able thoroughly to concur in your statement, sir, that it shows a very highly satisfactory financial condition of affairs of the Institution.

The motion was put from the chair and carried *nem. con.*

General WEBBER : It is always pleasant to return thanks for hospitality received, especially when that hospitality has been ungrudgingly given. And it is my pleasant task to remind you, that although we are not to-night present in that beautiful building where we so frequently receive the hospitality of the Institution of Civil Engineers, we are assembled to return them the thanks that are due for that hospitality which, when we consider it, is of such importance to our regular meetings. No one who recalls how we use that hospitality, both as regards the comfortable and commodious Council Room, the beautiful Hall, and the other rooms in which we refresh ourselves after our meetings, but must feel that we owe a great debt of gratitude to the parent Society. Gentlemen, I beg to move the following resolution :—"That the members of this Institution hereby express their sincere and cordial thanks to the President, Council, and Members of the Institution of Civil Engineers, for their kindness and liberality in allowing the meetings of this Institution to be held in their Lecture Hall." I should like to add that some emphasis to this vote of thanks might be attached, because we all recall that the President of the Institution of Civil Engineers, who has been in office during a considerable part of the time covered by the proposed vote I have the honour to submit to you, is also celebrated for his career as an electrical engineer, and has also been twice President of our Institution.

Mr. H. G. WOOD : I have much pleasure in seconding the resolution.

The resolution was carried unanimously.

Mr. A. A. C. SWINTON : I have much pleasure in proposing another vote of thanks. It is very fortunate that when the Institution of Civil Engineers are unable to grant us their hospitality, there is the Society of Arts to which we may repair, and where we may hold our meetings. I believe it is a matter of some interest that this hall of the Society of Arts was one of the very earliest of public buildings in London to be furnished with electric light. I will not detain you any further, but beg to move : "That the Members of this Institution desire to thank most cordially the President, Council, and Members of the Society of Arts for their kindness in allowing the meetings of this Institution in May to be held in their rooms."

Mr. W. R. COOPER : I have much pleasure in seconding the resolution.

The resolution was carried unanimously.

Professor J. PERRY: I beg to propose, "That the thanks of this Institution are due to the Local Honorary Secretaries and Treasurers for their kind services during the past year." I once was a Local Honorary Secretary myself, in succession to Professor Ayrton, in Japan, and I then felt that I was very greatly honoured in being asked to perform the duties of a Local Honorary Secretary. I am certain that all the Local Honorary Secretaries and Treasurers have that feeling. At the same time very valuable services are rendered to us by them, especially when we consider that a great number of our members are abroad. It behoves us, therefore, to give our thanks formally to the gentlemen who perform these functions for us.

Mr. R. W. HUGHMAN: I have great pleasure in seconding this resolution. It is the only way in which we can recognise honorary services, and I am quite sure we shall do it very heartily.

The resolution was carried unanimously.

Mr. W. M. MORDEY: There is no member of this Institution who works harder for it than Professor Ayrton. Whatever he does he does with very great energy, and, I need not say, with very great ability. He either writes some of the best papers brought before the Institution, or he educates the people who write them. He not only takes an active part in the meetings, but he is also one of the hardest workers on the Council and on the numerous committees which carry out important work. To all those he adds the work of Honorary Treasurer, and it is in connection with that work that I have very great pleasure in moving: "That the thanks of this Institution are due to Professor Ayrton for his kind services as Honorary Treasurer during the year."

Mr. J. W. LEYSHON: I have very much pleasure in seconding this resolution. It is a very happy combination of science and finance in this particular instance, and I am sure, as business men, all of us appreciate the great work there is necessarily involved in preparing and keeping together the various financial affairs of this ever-increasing and prosperous Institution.

The PRESIDENT: I am sure it is not necessary for me to add anything to what has been said in support of this resolution, because we all realise so thoroughly the value of the duties of our Honorary Treasurer, and the admirable way in which they are discharged.

The motion was carried by acclamation.

Professor W. E. AYRTON: I have to express my thanks to you, gentlemen, and to Mr. Mordey for the very kind way in which he has, in proposing this resolution, showered thanks and flattery on my unworthy self. My duty is to look after the funds and the Balance-sheet, and it is not necessary for me to say very much about them. Your President has analysed the Balance-sheet so carefully and so well that you must be quite familiar now with the very flourishing condition of the affairs of this Institution. He has drawn attention to the fact that we are spending a very large sum—£624 a year—on the *Science Abstracts*; but he has, on the other hand, drawn your attention to the fact that we are investing money very rapidly on behalf of Life Compositions, as you will see half-way down in this Balance-sheet, and

also on behalf of the Building Fund. He has also pointed out to you that during the early part of this year there has been invested a further sum—which, of course, does not appear in this Balance-sheet, since that is only made up to December 31st last—of over £1,450. The financial condition of this Institution, in fact, shares with electrical engineering generally in undergoing an extraordinary development such as the world has never before seen in engineering affairs.

Mr. R. Kaye Gray: I have very much pleasure in proposing a vote of thanks to the Honorary Auditors. You have all seen by the financial statement which has been laid before you how great their work must be. There is really an enormous amount of detail, and I am sure I am only expressing your feelings by tendering the very sincere thanks of the Institution to these gentlemen. I beg to move: "That the thanks of this Institution be given to Mr. F. C. Danvers and Mr. E. Garcke for their kind services as Honorary Auditors during the past year."

Mr. Alan Williams: I have very much pleasure in seconding that resolution.

The resolution was carried unanimously.

Sir H. Mance: I am glad to find that in the Annual Report we have noted the exceptional obligations that we are under to our Honorary Solicitors. I have now, in accordance with our usual custom, much pleasure in moving the following resolution: "That the best thanks of the Institution are due to Messrs. Wilson, Bristows, and Carpmael for their very kind services as Honorary Solicitors during the past year." I daresay many of us occasionally approach our own legal advisers with a certain amount of reluctance, but I can assure you that during the many years I have been connected with the Council I have never experienced such a feeling when requiring the assistance of Mr. Bristow, whom we generally consult in connection with the legal business of the Institution. As a general rule I am glad to say there is not much legal work to be done, but the period under report has been an exceptional one. There has been an immense amount of work in connection with the revision of the Articles of Association; we have had to refer every alteration, and also articles as a whole, to the Honorary Solicitors, in order that we might make no mistake; and I think we have every reason to be greatly obliged to them for the trouble they have taken. They have also acted as our Honorary Solicitors in connection with our Benevolent Fund. There have been cases where they might very fairly have made a charge, but they have always omitted to do so. I am sure you will support a hearty vote of thanks to our Honorary Solicitors.

Mr. Fentum Phillips: I am quite sure every member who has read our new Articles of Association will feel with me that they must have entailed a great deal of labour not only on the Council, but also on the part of our Honorary Solicitors. For this reason it gives me very great pleasure to second this vote of thanks.

The President: There is no doubt that we are under very exceptional obligation to our Honorary Solicitors, especially during this year. We are always under obligation to them from year to year, but this year

has been a year of unusual call upon their kindness in helping us in legal matters.

The resolution was carried unanimously.

The SECRETARY read the results of the elections to the various offices of the Institution for the coming year, as follows :—

President.

Professor SILVANUS P. THOMPSON, D.Sc., F.R.S.

Vice-Presidents.

Professor JOHN PERRY, D.Sc., F.R.S.
W. E. LANGDON.
JAMES SWINBURNE, M. Inst. C.E.
ROBERT KAYE GRAY, M. Inst. C.E.

Members.

S. L. BRUNTON.
Major P. CARDEW, R.E.
Professor J. A. EWING, F.R.S.
W. P. J. FAWCUS, M. Inst. C.E.
JOHN GAVEY.
ROBERT HAMMOND, M. Inst. C.E.
A. J. LAWSON.
P. V. LUKE, C.I.E.
E. MANVILLE.
W. M. MORDEY.
J. S. RAWORTH, M. Inst. C.E.
R. PERCY SELLON.
A. A. CAMPBELL SWINTON, M. Inst. C.E.
HERBERT TAYLOR, M. Inst. C.E.
C. H. WORDINGHAM.

Associate Members and Associates.

S. EVERSHED.
R. W. WALLACE, Q.C.
ARTHUR WRIGHT.

Honorary Auditors.

FREDERICK C. DANVERS.
E. GARCKE.

Honorary Treasurer.

Professor W. E. AYRTON, F.R.S., Past-President.

Honorary Solicitors.

Messrs. WILSON, BRISTOWS, & CARPMAEL.

The PRESIDENT : I have to announce that the scrutineers report the following candidates to have been duly elected :—

Member :

Mrs. Hertha Ayrton.

Associate Members :

Capt. George Pelham Aufrère Acworth, R.E.
Frederick Hardress Chaplin.
William Alfred Dyer.
Charles Thomas McKinlay.
Arthur James Mayne.
William Andrews Tester.

Foreign Member:

Col. Federico Pescetto.

Associates:

P. F. Anley.
Edwin Harry Baxter.
Arthur Ernest Charles Burgess.
Cecil Fowler.
Arthur Christian Gibbons.
Hubert Berger Graham.
James Gray.
Ernest Benson Greenall.
William Charles Grinyer.
Cecil Harry Hainsworth.
Herbert Johannes Mulder.
Rustom K. Nariman.
John Stanley Plumtree.
Robert Coryton Roberts.
Charles Wrake Sims.
Wallace James Webber.

Students:

Leonard Alfred Creasy.
Ramon Fernandez.
John Thompson Haynes.
Laurence Joseph Kettle.
Robert Hector Logan.
Gerald William Mayne.
Charles Herbert Millar.
Thomas Pitt Miller.
Capt. W. J. Underwood.
Walter Talboys Wheeler.
Francis Powell Williams.
Francis Nöel Younger.

The PRESIDENT: There is one special feature in this list that I should like to mention, and that is that it includes the name of the first lady elected to the Institution. I am very glad to have been President during the session which has seen that innovation. I have to announce to you, gentlemen, that the business of the session is now closed, and I heartily wish that when the next Annual Meeting takes place that the Secretary may have as good a report to read to you as that which he has read to-night.

ALTERNATING CURRENTS OF VERY HIGH FREQUENCY.

By W. G. ROYAL-DAWSON, Student.

(Abstract of a Paper read before the Students' Section of the Institution, April 18th, 1899.)

IN alternate current circuits, where the frequency does not exceed 100 or 200 cycles per second, the reactance due to the self-induction of straight wires, or to coils of a few turns, but without iron cores, is too insignificant to be taken into account when compared with the non-inductive or ohmic resistances, which are generally large in such circuits.

That the reactance due to self-induction may become of some importance in circuits where the frequency is as low as 100 $\sim$, the coefficient of self-induction or inductance must be relatively large with respect to the ohmic resistance. When the resistance is too large to be neglected, we have for the governing factor of the current, with the voltage constant,

$$\sqrt{R^2 + p^2 L^2}$$

which represents the amount of resistance offered to the flow of current by the leads and apparatus contained in the circuit, and is usually called the *impedance*. In the above expression, $p = 2\pi n$, where n is the frequency.

The effect of self-induction in any circuit where the current is varying is to set up reactions which oppose the forces tending to produce any change.

Thus, while the current is growing, the back E.M.F. of self-induction opposes the impressed E.M.F. and, as it were, chokes back the current, so that it does not rise so quickly to its final value as it would do were self-induction absent. If the impressed E.M.F. changes sign before the current has had time to reach its maximum steady value, we shall never

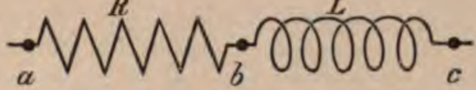


FIG. 1.

have flowing in that circuit so large a current as we should have were the E.M.F. to change more slowly, or to remain constant. This applies more especially to those circuits in which the rise and fall of potential follow a simple sine law.

Let us consider the case of a non-inductive resistance, R, Fig. 1, placed in series with a coil, L, having a large coefficient of induction.

r— ly across the points a and c a certain alternating potential, —ll we shall have a current flowing through the coil L and the istance R. The amount of this current, as registered by an ampere-ter, will be the same wherever the instrument is placed—that is, at a, b, or c.

But the sum of the volts, when measured by a voltmeter across the ints $a\,b$ and $b\,c$, will not be equal to the volts applied across $a\,c$, but ll always be in excess of these.

As every one knows, the reason why the sum of the two separate ges does not equal the applied voltage is due to the fact that the nt in the coil, having self-induction, is out of phase with the volts ross its ends; and we must therefore take, not the arithmetical sum, it the vector sum of the two voltages, which give us, as a resultant, on ompleting the volt-triangle, the volts plied across $a\,c$.

Capacity in an alternate current ci it also produces reactance, but the nature of this reactance is oppo e in sign to that produced by self-induction; that is to say, inductance causes the current to lag behind the volts, while capacity causes it to lea l in phase.

It need hardly be said that condensers can only be used in those circuits where the current is continually varying, and the larger their capacity the smaller will their reactance be, and, therefore, the greater will be the flow of current.

I will now come to the most general case of all, and the one wherein lies the secret of the efficient working of all alternate current systems, whatever be the frequency. It is the case where we have resistance, inductance, and capacity to deal with, either in series, parallel, or some other combination. The expression for the current when we have resistance, inductance, and capacity in series is—

$$C = \frac{V}{\sqrt{R^2 + \left(pL - \frac{1}{pK}\right)^2}}$$

For the condition of maximum current $pL = \frac{1}{pK}$, when we have left—

$$C = \frac{V}{R}.$$

(An example was then given to show the great importance of observing the conditions for obtaining the maximum current in any alternate-current circuit. This example was followed by a series of curves, showing the relation between the inductance, capacity, and frequency and the current; also other curves were shown, giving the relation between the inductance and capacity at different frequencies to obtain the maximum current.)

Since the voltage across a coil of wire having a coefficient of self-induction, L, is,

$$V = C p L,$$

where C=current, $p = 2\pi n$, and since we can increase L to any value without affecting the amount of the current, by adjustment of the

capacity, it is at once evident that we can raise the voltage across the terminals of the coil to many times that impressed at the ends of the system.

Again, working with a fixed inductance and current, we can raise the voltage by increasing the frequency, at the same time adjusting the capacity to its critical value. Theoretically, it is possible in this manner to raise the voltage across the ends of a coil of wire in series with a condenser of the right capacity to any number of times that applied across the ends of the system; but practically this cannot be done, owing to the great losses occurring in the apparatus and in the medium surrounding it. When excessively high frequencies are used, such as are obtainable by the discharge of Leyden jars through short, thick lengths of copper rod, the impedance offered by even a few inches of such rod when bent into a nearly closed loop is so great, that the difference of potential of its two ends is sufficient to cause a spark to pass between them.

A series of experiments was then exhibited, in which it was shown that when alternating currents of low frequency (100 $\sim$ per second) were passed through two Leyden jars, having their outer coatings connected by means of a thick rod of copper bent into a U form, the impedance offered by the loop was practically nothing.

But when currents having a frequency of the order of a million complete oscillations per second were used, the potential difference existing between two points near the middle of the loop was great enough to light a 15 c.p. 50-volt lamp. The existence of this potential difference was next shown by the sparks that passed between two wires leading from the same points.

The subject of syntony was then dealt with, and the necessary conditions to be observed in the syntonising of two circuits were pointed out.

To show the importance of syntony, an experiment was performed in which a 10-volt lamp was lighted only when the critical conditions were observed. In principle, the experiment is exactly similar to Dr. Oliver Lodge's syntonic Leyden jars.

It was explained that in all the experiments that had been performed, the voltages used were comparatively low, and the frequency of the order of a million cycles per second. It was then proposed to transform these low potential high frequency currents into high potential high frequency currents, and the principle of this transformation was dealt with, slides being shown.

The function of Tesla's high frequency high potential coils is to produce alternating electrostatic fields, which vary in sign with great rapidity, setting up corresponding stresses in the ether, and capable of transmitting in this manner power to a distance without the use of intervening wires. The effects produced by these high potential discha
are almost purely electrostatic in their character; that is,
exceedingly high potentials, but next to no current. We ar
a matter of fact, with alternating electrostatic forces,
sign at the rate of several hundred thousand, or milli

(An experiment was then performed showing a large Tesla oil-coil in action. A description of the construction of the coil was given.)

The intermittency of the sparks from the coil was explained, followed by many methods for obtaining an almost continuous stream of sparks from the secondary of the Tesla coil, each method being accompanied either by diagrammatic slides, or photographic slides of several of Dr. Nikola Tesla's high frequency alternators.

A new three-phase system devised by the author was then explained.

The latest method of working the Tesla coil by means of Wehnelt's liquid interrupter was exhibited.

The author expressed a hope that the time would not be long in coming when all wires connecting the source of supply with the place where the electric energy was required, would be done away with, and we should use instead that one and only source and transmitter of electric energy—the *Ether*.

Experiments were then shown, in which electrodeless vacuum tubes became luminous when held anywhere in the field between two metal sheets, ten feet apart, when the charges on the sheets were set rapidly alternating by means of a large Tesla coil.

Another experiment was shown, in which an incandescent lamp, 16 c.p. 50 volts, was lighted when placed in series with the operator's body and a spiral of copper rod, through which Leyden jar discharges were taking place.

Finally, the author expressed his sincere thanks to Dr. Silvanus Thompson for the great kindness he had shown towards him while he had been preparing the matter and apparatus for the paper. The author also offered his best thanks to Mr. Gorick not only for preparing the lantern slides, but also for assisting during the reading of the paper.

THE SYNCHRONISING OF ALTERNATORS.

By M. R. Gardner and R. P. Howgrave-Graham, Students.

(Abstract of a paper read before the Students' Section of the Institution, March 22nd, 1899.)

In this Paper the authors dealt with the actual apparatus used and the methods employed in the Synchronising of Alternators, rather than with the practical details and difficulties which arise in a lighting station.

To make the subject clearer some of the more important points in the parallel running of alternators are touched upon.

When coupling alternators so that their outputs may be combined, the conditions to be fulfilled differ, in most respects, from those in the case of continuous-current machines.

The conditions are three in number, and are as follows :—

1. The machines must be in synchronism ; that is, they must be running at such speeds that their frequencies are the same.

2. They must be in phase, or in step ; that is, they must all give a maximum impulse of the same sign to the same bus-bar at the same moment.

3. The voltage should, in most cases, be the same ; but this condition is the least important one, as the excitation of one machine is sometimes deliberately lowered to throw more load on to the others.

Mention of running alternators in series has not been made, because it is an impossible thing to do unless they are rigidly connected on the same shaft or by other mechanical means. This is very unusual, and in most cases impracticable ; besides which, machines so run are practically one machine.

A few words of explanation of this impossibility will not perhaps be amiss. Let us assume two alternators of equal electromotive forces and the same frequency. Let the curves A and A_1 (Fig. 1) represent the electromotive force waves of the armatures of two independently driven alternators connected in series, and having identical frequencies and pressures, and suppose them to be running synchronously, but to be out of step by a small angle. The curve E represents the algebraic sum of the ordinates of A and A_1, and hence also the resultant electromotive force of the two machines. The current curve C lags behind the E.M.F. curve E on account of self-induction. Now the total work done in the circuit equals the product of the sums of the E.M.F. and current waves respectively—that is, the product of the ordinates of the curves E and C. The work done in the circuit by either machine equals the products of the ordinates of its E.M.F. and current curves—that is of its E.M.F. curve, and the curve C which is common to both. Since the pressure wave A_1 of the lagging machine is nearer to the curve C than that of the leading one, the products of its ordinates and those of the curve C will clearly be larger than those of A and the curve C. Therefore the lagging machine A_1 is doing more work than the leading machine A. If the machines were rigidly connected this would continue indefinitely ; but as it is, the lagging machine which is working harder, naturally tends to slow up and lag more, thus supplying more work still to the circuit, and lagging more still. So they go on until their pressure waves are exactly opposed, when they are in stable equilibrium but are doing no work. If started in step they will do equal work until the mechanical or electrical disturbance slows up one of them, no how little, and then they will fall more and more out of step oppose.

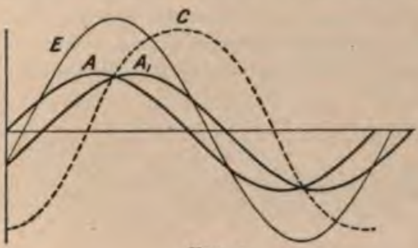


FIG. 1.

Alternators in Parallel.

The possibility of running alternators in parallel is due to their reversibility, or ability to act as synchronous motors. We know that an alternator if connected up to another which has been brought up to synchronism and voltage will run it as a motor, and that there will be a powerful tendency in the motor to keep in synchronism with the generator. In fact, an ordinary single-phase motor cannot do otherwise than keep in synchronism, if it is to work at all. The same thing happens with alternators in parallel, for if for any reason one slows up, the other immediately pumps current into it, hurrying it up to synchronism. So strong is this effect, that if the machines are thrown in parallel, when in direct opposition of phase, if they are nearly in synchronism they will right themselves in time.

This will be returned to later on.

For parallel working the speed must be uniform, and if the cranks of the engines are crossed, each machine will tend alternately to speed up and speed down, for when one engine pulls, the other is at its dead point. With bad governing and insufficient flywheel weight, the armature reactions will set up a kind of hunting or pumping action, and a

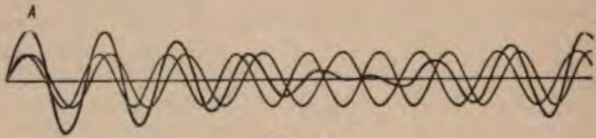


Fig. 2.

consequent fluctuation of the lights with each period of the engine's revolution. Two- or three-crank engines will of course obviate this.

This part of the subject may now be left, as it has been so excellently dealt with by Mr. Wilson in his paper on "The Parallel Running of Alternators."

We will pass on now to the results of putting two alternators, out of synchronism, in series with a negligible load such as a lamp.

It may elucidate matters to liken the two machines to two pendulums of slightly different lengths. If started in phase they will soon get out of phase, until, finally, they swing in opposite directions, and in an equal interval of time they will come into phase again.

Two alternators in series as described behave in precisely the same way. At one moment the resultant voltage equals the algebraic sum and the arithmetical sum of their voltages, and as time goes on they will oppose, and the algebraic sum will equal the arithmetical difference.

The resultant alternating voltage will therefore periodically grow and die away as shown in the curve A in Fig. 2.

This curve A is obtained by taking the algebraic sum of the ordinates of the two fundamental curves. The more nearly the machines are in synchronism, the slower will the fluctuations be. This effect is very important, as it is made use of in most synchronising apparatus.

A model was shown to illustrate it. The model consisted of two

commutators, one with fixed speed, and the other variable, being driven by a small motor. Each was supplied with a continuous current, and they were connected in series with a lamp.

Let us now consider the effect of throwing in two machines in parallel, regardless of step, having got them into synchronism first. Immediately they are thrown in, a current will oscillate between them, which, if they directly oppose in phase, may be enormous, especially in ironless armatures. This rush of current will, if the machines stand it, pull up one a little, and put them out of synchronism, starting the see-saw action which will bring them into phase, and motor actions will start between them, finally bringing them into synchronism. All this may sometimes take several seconds to accomplish, during which time the lights blink and go up and down. This limits the use of this method absolutely to motor circuits, and also to machines with ironclad armatures.

In the case of ironless armatures like Ferranti's, Mordey's, or Siemens', there would be such a rush of current as not only to destroy the armatures by mechanical shock, but perhaps to burn them out.

In the case of ironclad armatures, especially those with big teeth, the self-induction is so large that the current is limited, and besides, all mechanical strain is thrown on the iron and none on the conductors.

Commercially, however, machines are hardly ever thrown in together regardless of step.

It may be well to mention that machines of the same type and waveform are most easily synchronised.

Methods of Synchronising Alternators.

The simplest form of synchronising apparatus is shown in Fig. 3. A_1 is the machine already running and connected to the 'bus-bars. A_2 is the machine to be thrown in. When the switch of A_2 is off, and the auxiliary synchronising switch (not shown) is on, the machines are in series with the lamps L L. As A_2 comes up to speed the light of the pilot lamps will flicker, and the nearer to equality the frequencies come, the slower will be the flicker, until the light takes about two seconds to grow and die away. All this time the operator has been carefully regulating the voltage by means of a rheostat in series with the field coils. When the vibrations get to the right speed, a matter of judgment and experience, the operator waits till the light in the lamps is nearly a minimum, and then switches in sharply.

Fig. 3.

When the machines are out of phase as regards the synchronising circuit, which is a series one, they are in phase as regards the 'bus-bars,

allel one. The machines are now in phase but slightly onism, and one will hurry the other up, and the other lown, and thus they will equalise. They will now quietly l. It is a delicate job to synchronise, and requires great rience.

other methods depending on the same principle. In Fig. 4 the primary P of a transformer is put where the lamps were before, and one or more lamps are put in the secondary circuit, the mode of operation being the same as before.

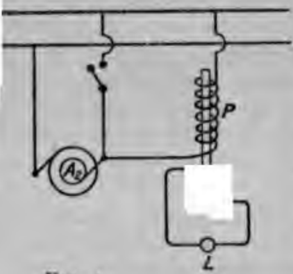


FIG. 4.

This is for high tension systems, as also are all the following.

Fig. 5 shows a method in which a transformer with two primaries is used, one being connected to the 'bus-bars or to A_1, and the other to A_2. This method admits the use of a double pole switch. The arrows show the directions of fluxes and E.M.F.'s at the right moment for switching in.

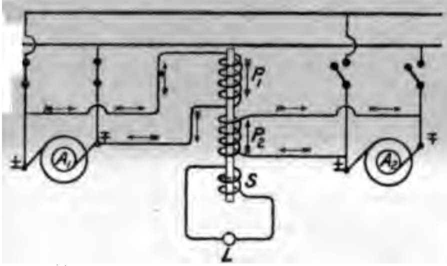


FIG. 5.

Another way is to have two separate transformers as shown in Fig. 6. If the secondaries are in series, the switching-in moment will be that of minimum light; if cross-connected, that of maximum light.

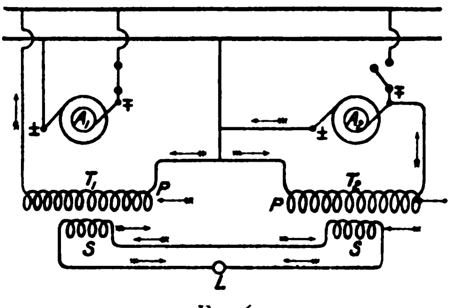


FIG. 6.

In any one of these cases, one or more telephones may be used instead of lamps, the noise growing and dying away just as the light does with the lamps. A telephone was substituted for the lamp in the revolving commutator model.

The General Electric Company instituted the use of large iron-wire cored electromagnets with iron diaphragms in front. These were absolutely unconnected with each other, and

depended on the interference of sound waves, one "sounder" being connected to the 'bus-bars and the other to the machine to be thrown in. They indicated synchronism when the note was clear and without beats, and the machines were then thrown in regardless of step.

This type of apparatus, then, is only suited for motor-circuits in which ironclad armatures are used.

A Cardew voltmeter is very useful sometimes, if used instead of, or even as well as an incandescent lamp. It can be read quickly enough, the exact position of the needle is easy to follow, while the lamp is less so because of eye-fatigue.

Prof. Ayrton mentioned at the Institution of Electrical Engineers in 1894 an electrostatic synchroniser, for the purpose of doing away with transformers in high-tension stations.

A diagram of connections is shown.

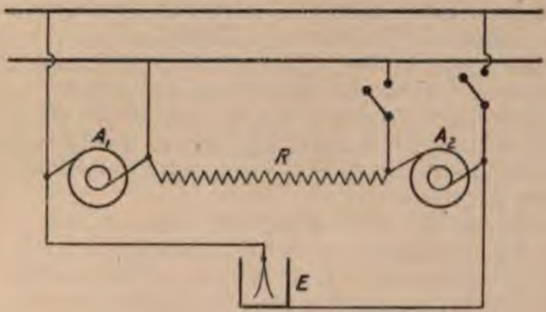


FIG. 7.

R is a resistance, almost as large as you like—say a lead-pencil line on a piece of slate or ebonite. E is any dead-beat electrostatic voltmeter, such as a gold leaf electroscope, which might be made with leaves even a foot and a half long to be visible far off. In such an apparatus the moment for switching in is when the leaves remain closed for some time.

Prof. Ayrton claimed that this arrangement was more dead-beat than a lamp, that it does away with expensive transformers, that it may be as large and visible as you like, and needs no calibration or scale, and may, as a voltmeter, be as wrong as you like. Lastly, he claims that it is perfectly and absolutely safe.

Sometimes little auxiliary synchronising 'bus-bars are provided. Any machine to be synchronised can be connected to them, the procedure being as usual, except that when the right moment comes, instead of being put on to the mains, the machine is put on to these 'bus-bars, between which it forces a current of 10 or 15 amperes through choking coils. This current is sufficient for synchronising, and when the machine runs quietly and steadily, it is put on to the mains by another switch.

Optical Synchroniser.

It was noticed, some time ago, that if an alternating arc lit up the ternator driving it, the flywheel appeared stationary or revolving owly backwards according to the number of its spokes.

If the armature is painted round with as many white spots as there re alternations per revolution, an arc driven by it will cause the circle spots to appear stationary, because the beats of light come every time pot reaches the places where the stationary nodes are. Thus, if a beat alls on a spot A, by the time the next beat comes, B, which is behind A, has got to where A was in the first instance. If the e arc lamp lights a similar machine, also painted with spots, but running a little more slowly, the spots will move a little less than one space r each beat, and the circle will thus appear slowly to rotate backwards—that is, opposite to the real direction of rotation. This method is never used for absolute synchronising, but just to give a rough idea of how the speed is going, a pilot lamp apparatus being used as well.

A model was shown illustrating the revolving-spot phenomena, by means of rotating discs painted with spots, and a disc with slots cut in it and a light placed behind. This disc also rotated, and they were all driven by a motor, all the phenomena being reproduced.

As regards this method and that of using telephones, it would be interesting to quote the words of Dr. Silvanus Thompson, in the discussion on Mr. Mordey's Paper of 1894, before the revolving spots were ever used at all. He says :—

"I remember very well watching the machines built by Ganz and Co. in the station at Rome. The alternators were set in one line. You can look through one machine at another, and you can see when the machines are synchronised by merely observing the rotating magnets of one machine across the rotating magnets of the other. They will appear to be stationary when absolutely in phase, just as two perforated discs of cardboard do with holes, when they are spun round at equal speeds, one being watched through the other. It is quite possible, I think, that some optical synchroniser might be found which would be more satisfactory than either acoustic or electrical synchronisers used with lamps."

The authors venture to suggest that what Dr. Thompson foresaw, before the use of revolving spots was known, may, at some future time, be more fully realised than it is now, and that the optical method, now in its infancy and only used as a rough guide, may be the method of the future.

OBITUARY NOTICES.

Sir WILLIAM ANDERSON, K.C.B., F.R.S., was born of English parents in St. Petersburg on the 5th of January, 1835, and was educated primarily in the commercial High School of that city, and afterwards in the engineering department of King's College, London. After a brilliant college career he was apprenticed for three years to Sir William Fairbairn in Manchester, and then, when barely of age, became a partner in the firm of Messrs. Courtney, Stephen & Co., bridge-builders and engineers in Dublin. In 1864 he joined the firm of Easton and Amos, afterwards Easton and Anderson, in London. The works of the firm, at that time in Southwark, were subsequently removed to Erith, where Mr. Anderson, as he then was, assumed sole professional charge. His scientific and engineering attainments, and his wide knowledge of collateral subjects, were universally recognised, and in 1889 he was appointed Director-General of Ordnance Factories, with control of the various departments in Woolwich Arsenal, and of the small arms factories in Enfield and Birmingham, and of the gunpowder factory at Waltham Abbey. The appointment of a civilian to this responsible post aroused keen opposition, which was however in time dispelled by the pre-eminent ability and unfailing tact of the new Director-General, whose personal charm of manner and gentleness of disposition endeared him to all who had the privilege of his acquaintance. His service to the nation was recognised in 1897 by Her Majesty, who in that year bestowed upon him the order of Knight Commander of the Bath. Sir William Anderson was a Fellow of the Royal Society, a Member of the Institution of Civil Engineers, and an Honorary D.C.L. of Durham University. He died at his official residence in Woolwich, on the 11th of December, 1898.

He was elected a member of this Institution on the 9th of January, 1890.

Lord SACKVILLE ARTHUR CECIL.—It is not often that the sources of information for an obituary notice afford lessons of so generally a useful kind as those to be found in the life of the late Lord Sackville Cecil. It is more so because the exceptional circumstances of his birth were the smallest factor in the success of that part of his career which affords the most useful lesson to our young engineers.

Those advantages of parentage, natural aptitude, and training which were of service to Sackville Cecil (allied to half the old aristocracy of the kingdom) may be found in these kingdoms in every sphere of educated life.

His father, the second Marquess of Salisbury, was a man who allowed no considerations of personal ease or amusement to stand in the way of duty. As an engineer and mechanic he personally directed the works on his own estates, and with foremen of his own training rebuilt those parts of Hatfield that had been destroyed by fire. His mother, since known as Mary Countess of Derby, (second daughter of the fifth Earl de la Warr), has been distinguished for rare personal

ability, and for exceptional instinct in being able to appreciate worth in every rank of life, and in using that instinct as a guide to influence the careers of many. His father gave him at Wellington and Cambridge the school and university training of the son of an English gentleman, with intervals of private tuition, and—himself a student of education, and a statesman whose labours influenced the growth and progress of education in the nineteenth century—in addition gave that training supervision.

During part of the time when he and his brothers were living with a tutor in the old half-ruined manor house at Cranbourne, their father systematically mapped out their course of study, and, much after the fashion of colonial life, made them provide their comforts and luxuries by their own personal labour and exertion.

Under such early associations, during which he spent more of his time amongst his father's workmen than in the recreation field, his destiny to become an engineer was inevitable.

In the shops of the Great Eastern Railway Company, and on the footboards of their locomotives during the reign of one of the kings of railway management—the late Samuel Swarbrick—he continued his practical education as a mechanical engineer. The training so begun was continued in the carriage department of the Great Northern Railway's works at Doncaster. As chief electrician under Sir Charles Bright, in connection with the laying of the submarine cable between Marseilles and Boma, at the mouth of the Congo, he commenced his practical career as a telegraph engineer.

The writer first made his acquaintance when engaged in 1872–3 on the telegraph arbitrations between the Post Office and the railway companies, and can recall the ingenious system of abridged writing by which Lord Sackville enabled Mr. Swarbrick, who was almost quite deaf, to follow and understand the procedure. The former sat beside, and a little in front of the latter, writing on a tablet, and by glancing at the signs in use between them, which were put down with ease and rapidity, the latter was kept nearly as well informed of what was going on as if his sense of hearing had been perfect.

As an Assistant General and Traffic Railway Manager, between 1878 and 1880, he was at all hours to be seen at the busiest places on the platforms quietly watching and silently systematising and guiding, and no one would have guessed the position, official or social, of the wearer of the unbuttoned tweed jacket and turned down collars, with the grave, earnest face and occasional quiet smile, who rarely interfered but had his eye on everything.

He had entire charge of the complicated arrangements for the transfer of the City terminus of the Great Eastern Railway from Bethnal Green to Liverpool Street, a task which required powers of generalship and organisation, and a physique capable of continuing at work for thirty-six hours at a stretch, such as are not often met with. In Germany his characteristics would have infallibly led him up to the highest ranks of the general staff of the army, which result implies the rarest combination of distinguished qualities.

As General Manager he for five years devoted his railway experience

to the Metropolitan District Railway Company, where he was said to be an "odd and peculiar man" and "very popular."

Since 1885 he gravitated back to his first attachment, namely, the applications of electricity, especially to submarine telegraphy, and at the time of his death he was a Director of the Eastern, the Brazilian Submarine, the Pacific and European Telegraph Companies, and of the Globe Telegraph and Trust Company, as well as Chairman of the Exchange Telegraph Company. He was a man with an immense capacity for work, keen intelligence, great conscientious rectitude, and of the simplest habits of life. He left a considerable fortune, almost entirely the result of his own labour.

He died of pneumonia at his residence, Oast House, Hayes Common, on the 29th of January, 1898, at the age of 50, and, after having been cremated at Woking, was buried near the church of St. Audrey, which had been erected by himself.

He was elected a Member of this Institution in 1872.

C. E. W.

JOSIAH LATIMER CLARK, F.R.S., M. Inst. C.E.—The name of Latimer Clark is identified with the evolution of electric telegraphy by land and sea. This Institution will, however, always associate him primarily with the formation of its predecessor, the Society of Telegraph Engineers, of which, in 1875, he became the fourth president.

Born at Great Marlow on March 10, 1822, he was a younger brother to the late Mr. Edwin Clark, whom he at one time assisted in his civil engineering practice. Latimer Clark studied chemistry at an early age, and his first connection with technical work was in the chemical manufacturing industry in a large Dublin establishment.

Soon, however, he became impressed with the activity in the railway construction of the "forties," which resulted in his joining, with many other young engineers, the great army of railway surveyors. Latimer Clark assisted his brother Edwin—the Resident Superintending Engineer under Robert Stephenson—in the construction and erection of the Britannia Tubular Bridge across the Menai Straits. His work here covered three years—1848 to 1850. Here it was that Latimer Clark was, in conjunction with his brother, in the habit of firing a time gun by electricity every evening at eight o'clock. This attracted the attention of the chairman of the then newly-formed Electric Telegraph Company (the late Mr. John Lewis Ricardo, M.P.), who soon afterwards, in 1850, invited him, as well as his brother Edwin, to join the staff; the result being that Edwin Clark became their chief engineer, and Latimer assistant engineer. This was Latimer Clark's introduction to the subject of electric telegraphy—then in its infancy. In his new position he superintended the construction of much of the telegraphic system of this country, and three years later, on the retirement of his brother, became engineer-in-chief, which position he held for seven years—*i.e.*, till 1861—when, on his private professional practice promising to become considerable, he became consulting engineer to the company, remaining so till the Government took over the telegraphs of the United Kingdom in 1870. Clark's talents as a discoverer—largely

represented in patents—soon made themselves apparent. In 1853 he invented an exceedingly ingenious camera for taking stereoscopic pictures with a single lens (*vide* "Journal of the Photographic Society," May 21, 1853). In those days it was practically impossible to obtain a pair of lenses of the same rapidity and of identical focal length. This was followed in 1854 by the ingenious and important system of applying air pressure to the conveyance of letters, parcels, or telegraphic messages, which he invented and introduced into practice through the Pneumatic Despatch Company—a system which was afterwards developed by the late Mr. C. F. Varley, and which is now so extensively used by the Post Office for the transmission of telegrams from branch offices to the General Post Office. About this time, too—independently of similar investigations by the late Sir Charles Bright—Latimer Clark conducted a long series of experiments on the retardation of electricity through underground telegraph wires laid between London, Leeds, and Liverpool, which went to show that the "rate" of flow or variation in the strength of an electric current was in no way influenced by mere potential difference—or tension, as it was then termed. It was in connection with this latter demonstration that Latimer Clark first came prominently to the fore. At the request of Professor Airy, his experiments were repeated before Professor Faraday, and subsequently formed the subject of a lecture at the Royal Institution in January, 1854 (see Faraday's "Experimental Researches," pp. 508–517). Two years later (1856) he introduced his well-known double cup insulator for land telegraphs, and a short time after his differential galvanometer.

At first the Electric Telegraph Company confined their attention to constructing and working land telegraphs. Latimer Clark, however, also acted in a technical capacity to the International Telegraph Company, and was thus associated with the laying of some of the earliest submarine cables from this country to Holland, Belgium, &c., between 1853 and 1855. About the latter date the "International" Company was absorbed by the Electric Telegraph Company under the style of the "Electric and International Telegraph Company," with Latimer Clark as engineer-in-chief.

In 1857 he became associated with the then Astronomer Royal (the late Sir J. B. Airy) in devising a method for indicating Greenwich mean time throughout the country. At this time also Mr. Clark suggested to Professor Airy the establishment of magnetic observatories furnished with wires to act as feelers in connection with the approach of storms.

In 1860 Latimer Clark served on the joint Committee instituted by the Board of Trade and the Atlantic Telegraph Company to inquire into the construction of submarine cables. In addition to the assistance he was able to give the Committee as an ordinary witness, and by making investigations, Clark put in an elaborate supplementary report, on the laws which govern the propagation of the electric current in long submarine cables, resulting from his previous investigations on the subject already alluded to. This report constituted Appendix II. in the Blue Book, and was certainly one of the most valuable pieces of evidence drawn forth.

In June, 1860, Latimer Clark became professionally associated with the late Sir Charles Bright. The combination proved a strong one, for the firm of Bright & Clark served as supervising engineers to the construction and submersion of most of our early telegraph cables. In the first year of the partnership Mr. Clark joined with Sir C. Bright in a paper read before the British Association on "Electrical Standards and Units," which led to the formation of the Committee on Standards of Electrical Resistance which has since done much useful work.

As engineers to the first Persian Gulf cable to India in 1863, Messrs. Bright and Clark conducted a series of experiments with the core of that cable to ascertain the effect of temperature on the insulation resistance of gutta-percha. From those experiments, which were described in a paper read by Sir Charles Bright before the Institution of Civil Engineers on "The Telegraph to India," the formula for correcting the resistance to a standard temperature was arrived at. This is now in everyday use by cable electricians. Though the bituminous composition for protecting cables, patented in 1858 in the names of Clark, Braithwaite, and Preece (known as "Clark's Compound"), did not prove a success—and had to be abandoned after a single trial, mainly on account of the method of application adopted, the Bright and Clark system,[1] first used on this same Indian Government cable, has ever since been universally employed for preserving the iron sheathing wires. When returning from one of the Indian Telegraph expeditions, in September, 1869, Mr. Clark had the misfortune to be shipwrecked in the s.s. *Carnatic* off the island of Shadwan in the Red Sea; and it was only his great physical strength that enabled him to swim ashore, despite a dislocated shoulder and a broken collar-bone.

Messrs. Bright and Clark acted as engineers to the Anglo-American Telegraph Company for the purposes of the second and third Atlantic lines. After the two cables (of 1865 and 1866) had been successfully laid, Latimer Clark, on behalf of his firm, tested and sent signals through the entire length.

On September 25, 1868, the partnership of Bright and Clark was dissolved, and a little later Latimer Clark joined with Mr. H. C. Forde and with Messrs. Charles Hockin and H. A. Taylor in the consulting firm now existing under the name of Clark, Forde, and Taylor, and in association with his partners supervised the manufacture and laying of some 100,000 miles of submarine cable in all parts of the world.

It is not, however, only electricity in its application to telegraphy which has benefited at Clark's hands—electrical science generally also owes him a debt of gratitude. The zinc-mercury standard cell—the electrician's practical standard of potential difference—is, no doubt, what will hand down the name of Latimer Clark to posterity in electrical science; for the use of this invention, in the laboratory as well as in practical testing, is almost unbounded. This followed on an exhaustive series of experiments with the Daniell Cell, variously constructed with a view to obtaining, if possible, a satisfactory standard of electromotive force therefrom. In these experiments Mr. Clark was greatly assisted by Mr. J. C. Laws and Mr. Frank Lambert (both since deceased)

[1] See Sir C. Bright's Specifications, Nos. 466 and 538, of 1862.

as well as by Mr. Herbert Taylor. The standard cell was first introduced to the public in 1873 by Mr. Clark in a paper entitled "A Standard Battery of Constant Electro-motive Force," read before the Royal Society.

Whilst speaking of Latimer Clark's scientific electrical researches we may mention that some of the earliest investigations on the effect of self-induction in electrical work may be ascribed to him; for Clark made a valuable contribution to telegraphic progress in his study of the errors due to the inductive action of a galvanometer needle upon its own coil when using shunts of different values in a series of comparative discharges (see his paper in the Journals of the Society of Telegraph Engineers in 1873—"On a common source of error in the measurement of currents of short duration when using galvanometers with shunts").

Clark's contribution to electrical literature was of an important and eminently useful nature. In 1868 he brought out his "Elementary Treatise on Electrical Measurements," which did much to make the laws of electric current and potential clearer to the practical electrician. Then, in 1871, followed "Electrical Tables and Formulæ"—still to be found in every cable factory and testing station—jointly compiled by Latimer Clark and the late Mr. Robert Sabine, being partly a revision of the aforesaid "Electrical Measurements."

In addition to his consulting work, Latimer Clark also figured as an electrical contractor, being senior member in the late firm of Latimer Clark, Muirhead & Co., formed in 1875 as the outcome of Warden & Co. This firm were large manufacturers of electrical apparatus and machinery generally. They were the responsible contractors for one of the earliest house-to-house electrical supply companies in London—the St. James's and Pall Mall Electric Lighting Company. This was, perhaps, the first entirely successful concern of its kind, Mr. Clark being an active director, while its formation was largely due to the late Mr. John Muirhead, his partner. But, apart from this, the latter part of Latimer Clark's life was much taken up with electric lighting developments in a technical sense.

Latimer Clark was not only a telegraph engineer and electrician, he was also a civil engineer in the broadest sense. Few are able to cope with so varied a sphere of work; few acquire so wide a field of knowledge and information. In 1874 there was commenced that association with Mr. J. Standfield (represented by the firm of Clark and Standfield) which resulted in many improvements in floating docks. His brother Edwin had designed the earlier hydraulic docks, but of greater efficiency and ability proved the single and double-walled docks now widely used. Many of these have been built since 1872 with a lifting power ranging up to 11,000 tons.

Latimer Clark was a devotee of astronomy; and in 1882 introduced his little transit instrument which has done much to popularise this most fascinating branch of science. Simple, yet effective, this instrument has enabled many who could not afford to buy costly apparatus to follow the stars. Clark wrote more than one astronomical work, amongst them a treatise that could be followed without previous

knowledge—a true popular educator. The kindred science of microscopy also found in him a student, as did also botany; and at his house at Upper Norwood he used at one time to make a special study of rare Alpine plants.

Throughout his life Clark was an ardent bibliophile; and his library, so far as electrical works are concerned, was unequalled. In this collection and in the preparation of catalogues he found congenial employment for his leisure hours.

A close student of telegraphic history, Latimer Clark was always very much impressed with the great services performed to electro-telegraphy by the late Sir William Fothergill Cooke on the one hand, and the late Mr. Jacob Brett on the other. Clark's writings were always characterised by painstaking precision and accuracy; and a notable example of this may be found in his historical description of the part played by Sir W. F. Cooke, contributed to the journal of this Institution[1] (then the Society of Telegraph Engineers) soon after his death in 1879. In his later years Jacob Brett lived almost in penury, and Clark exercised all his energies to obtain him a Civil Service pension, in which effort he was at length successful. The income so obtained was, however, altogether insufficient; and Clark, along with others, made a further effort in the direction of obtaining him an increased pension. It seemed as though this were on the point of being realised, when poor Brett died at the beginning of 1897. After Jacob Brett's demise Mr. Clark presented all the valuable papers and volumes of the Brett brothers to this Institution, as, indeed, he had done previously in the case of Sir William Cooke. He himself (Latimer Clark) lived but another year, dying quietly and suddenly, at the ripe age of seventy-six, on October 30, 1898.

The funeral took place at Kensington Parochial Cemetery, near Hanwell, when the Council of the Institution were largely represented, and a number of other engineers and electricians were present.

The name of Latimer Clark will always be prominently associated with pioneer electric telegraphy—land and submarine—and his death is to be deplored by his professional brethren.

As before stated, besides being associated with the founders in the formation of the Institution and a life trustee, Latimer Clark became President in 1875, the fourth year of its existence as the Society of Telegraph Engineers. His inaugural address on taking the chair contained an interesting and valuable account of the early history of the electric telegraph. Clark joined the Institution of Civil Engineers as an Associate in 1858, and in 1861 was created a full member. He was elected in 1889 a Fellow of the Royal Society. He was also a Fellow of the Royal Astronomical Society, of the Royal Geographical Society, and similar institutions interested in the pursuit of science. Moreover, in recognition of his services to engineering and scientific advancement, he became a chevalier of the Légion d'honneur some years before his death.

The personality of Latimer Clark was, perhaps, mainly characterised by the wonderful rapidity with which he accurately grasped the salient

[1] *Journal*, vol. viii. p. 361.

points of a problem. He seldom made a technical error in engineering or electrical work; moreover, he was, on the whole, fortunate professionally. He had the advantage of considerable foresight and a good knowledge of the world, aided by a large share of common sense.

CAPTAIN WILLIAM HENRY DAVIES was born in Swansea and died at his residence, 51, Tregunter Road, South Kensington, on the 2nd of June, 1898, aged 73. He was one of the founders of the Exchange Telegraph Company in conjunction with the late Sir James Anderson and Mr. Cyrus Field, and had been its Managing Director until his death.

Captain Davies had been in the service of the Pacific Steam Navigation Company and the Peninsular and Oriental Steam Navigation Company, and it was while acting as agent for the latter at Aden that he rendered important service to the Government of the day in coaling the ships of the Abyssinian Expedition, receiving in recognition a service of plate from the Lords of the Admiralty. He was chief officer on board the *Great Eastern* on her maiden voyage to New York. He was a director of several prosperous companies.

He was elected an Associate on the 8th of January, 1873, and was transferred to the class of Members on the 23rd of February, 1888.

F. H.

SIR JAMES NICHOLAS DOUGLASS, F.R.S., was born at Bow on the 16th of October, 1826, and died at Bonchurch, Isle of Wight, on the 19th of June, 1898. He was the son of Nicholas Douglass, Superintendent Engineer of Trinity House, and was educated in part at Newcastle-on-Tyne, and in part at Bridgend. He was regularly apprenticed to a firm of mechanical engineers, and at the age of twenty-one commenced his life's work in the capacity of assistant-engineer to his father, who, at that time, was engaged in the construction of a lighthouse on the Bishop Rock in the Scilly Isles. With the exception of two years (1852–1854) spent as manager of railway carriage works at Newcastle, his whole subsequent life was devoted to the work of a lighthouse engineer. He acted as resident engineer in the construction of several important lighthouses, and in 1862 succeeded Mr. James Walker as engineer to the Trinity House Corporation. In 1881 he received the honour of knighthood in recognition of the successful completion by him of the new Eddystone lighthouse. Sir James Douglass devoted careful attention to the illumination of lighthouses by means of the electric light, and it was on his recommendation that fluted carbons were employed for the powerful arc lamps required for this purpose. He was a Fellow of the Royal Society and a Member of the Institution of Civil Engineers.

He was elected a Member of this Institution on the 27th of May, 1886, and served on the Council from 1889 to 1892.

PETER CHRISTIAN DRESING, who was born on the 21st of October, 1852, in Jutland, Denmark, entered the service of the Great Northern Telegraph Company on the 1st of June, 1871, as a telegraph

clerk. His unusual technical abilities soon attracted the attention of the directors of the company, and after having taken part in the laying down of the company's Franco-Danish, Anglo-Danish, Skagen-Marstrand, and the East Asiatic cables, he served as the company's electrician in Aberdeen during 1875–79. In 1879 he was appointed engineer in London, where he controlled the manufacture of the company's new cables for East Asia (1,300 nautical miles), the laying down of which he superintended in 1883. From this year he was connected with the central administration at the company's head office in Copenhagen, and was appointed engineer-in-chief in 1896. In 1892 he was decorated by the King of Denmark with the order of the Knight of Dannebrog.

By the death of Mr. Dresing, which took place on the 10th of February this year, the Great Northern Telegraph Company has lost one of its most prominent officers, while this Institution, as well as the telegraph profession as a whole, is deprived of one of its most sympathetic members.

Mr. Dresing was elected an Associate of this Institution on the 27th of January, 1875, and was transferred to the class of Members on the 7th of December, 1880. In 1890 he succeeded Mr. Madsen as Local Honorary Secretary and Treasurer of the Institution for Denmark, an appointment which he held up to the time of his death.

J. L. W. V. J.

RICHARD OLIVER GARDNER DRUMMOND was born on the 6th of January, 1862, and died on the 23rd of June, 1898. He served his apprenticeship from 1879 to 1882 with Messrs. Mather and Platt, and then proceeded to Cape Colony, where he was engaged as a mechanical engineer for the Cape Government railways and gained much experience in superintending the erection of machinery. In 1885 he was employed by the French Diamond Mining Company, and put down for them installations of electric light, signals, and telephones. In 1887 he became electrical engineer to the De Beer's Mining Company, and a few months later was manager of their electrical department. Whilst serving in this capacity he acted as electrical engineer to the borough of Kimberley, and held several appointments as consulting engineer to various companies. In 1893 he gave up his appointment with the De Beer's Company and joined a firm of consulting engineers, with whom he was engaged in lighting, power, and traction work, mainly in connection with the mining industry, carrying in one instance a three-phase current at a pressure of 10,000 volts over twenty miles of wire. At the time of his death he was Vice-President of the South African Society of Electrical Engineers.

Mr. Drummond was elected an Associate on the 16th of May, 1889, and was transferred to the class of Members on the 11th of December, 1890.

RICHARD PENTNEY EIDSFORTH, who died on the 18th of March, 1899, was born at Wisbeach in 1844, and educated at Norwich. When quite young he went to Australia, where he remained for four years. From 1866–69 he was with Mr. W. T. Henley, and went on

several cable expeditions. From 1869–72 he was on the staff of Mr. Cromwell Varley, with whom he went abroad several times, and whom he assisted in his tests of the Malta-Alexandria cable.

In 1872 he became the manager of the Mildmay Park Works of Messrs. O. and F. H. Varley, where he remained until 1875. The next two years found him engaged in business on his own account, when, meeting with Mr. E. Paterson, he was induced to become his manager, which position he occupied until 1883, having during these six years done much pioneer work in connection with the practical development of telephones and electric lighting apparatus, the "R. E." main switch attracting special attention. The following nine years were spent in partnership with Mr. Mudford at the Arrow Electrical Works, Holloway, and the last seven years with the General Electric Co., Ltd., first at their Chapel Street Works, Salford, and, later, in the organisation and equipment of their new works at Silk Street, Adelphi, Salford.

His untiring zeal, energy, and geniality won for him the esteem of all with whom he was brought into contact.

He was elected a Member, May 12, 1881. F. J. M.

Sir DOUGLAS GALTON, K.C.B., F.R.S., of Himbleton Manor, Worcestershire, who died on the 10th of March, 1899, was the second son of John Howard Galton, of Hadzor House, Worcestershire. He first went to a school in Birmingham, thence he passed under private tuition in Switzerland, and after being some time at Rugby he entered the Royal Military Academy, Woolwich, where at the age of fifteen he passed highest of his term, taking first prizes in all subjects. In December, 1840, he received a commission in the Royal Engineers.

In 1842 he was employed under Sir Charles Pasley in the removal of the wreck of the *Royal George* at Spithead, when the ignition of the explosives was attempted by means of electricity. In 1846 he was appointed to the Ordnance Survey, and in 1847 he was attached to the newly formed Railway Commission as Secretary, and he conducted various experiments and made many engineering investigations for the Commissioners. Subsequently he became Inspector of Railways, and Secretary of the Railway Department of the Board of Trade. In 1860 the Government appointed a Committee, under the presidency of Captain Galton, to investigate and report on the whole question of submarine cables. In 1855 the question of army sanitation came prominently before the public, and it was in connection with warming, ventilating, and allied subjects, as well as hospital construction, that Captain Galton's work secured him a European reputation. In 1860 he was moved from the Board of Trade to the post of Assistant Inspector-General of Fortifications, and in 1862 he was appointed Assistant Secretary of State for War, which high office he held until 1870, when he became Director of Public Works and Buildings in the Office of H.M.'s Works, and he finally retired from the public service in 1875.

Since that date Captain Douglas Galton's name has been associated, professionally as an engineer or in an honorary capacity, with many great scientific and industrial works, and his name was as well known, and his advice and assistance were sought and held in high

estimation, throughout European countries as well as at home, and in the Colonies. The writer can recall General Morin (the head of the Conservatoire des Arts et Métiers) having said to him in Paris in 1867, "that Captain Douglas Galton's ventilating grate was the only original arrangement for perfect warming and ventilating with the open fireplace that the century had produced."

As a standing member of the Army Sanitary Commission he left his mark on many measures which have had to do with the health of the services all over the world.

As General Secretary of the British Association from 1870 to 1895 he gave the most earnest and persevering support to its organising and working; and there were few things which he had more at heart than the development of its popular usefulness.

He was a frequent contributor to the press, and his record as a writer is prolific.

In the last decade Sir Douglas Galton has associated himself with some of the Metropolitan electrical industries, and he has always taken much interest and held honorary office in the great engineering institutions, including our own. The charm and urbanity of his manner, whether in social life or in official or professional association, left a deep impression of his amiability, sincerity, and uncompromising disinterestedness, which was entirely confirmed by all the acts of his life and the record he has left behind.

Besides the debt which the great industries of the country owe to him, science may before long reap the harvest of one of the works which occupied the last three years of his life, in collecting facts upon which to found a claim for the support by Government of a National Physical Laboratory, and in advocating and assisting in the initiation of the scheme.

Sir Douglas Galton was elected a Member of this Institution in 1872, and was a Member of Council from 1888 to 1890. C. E. W.

DAVID W. GOTT, who died on the 7th of July, 1898, was born at Kendal in Westmoreland. He entered the Telegraph service in the year 1866, and was for some time stationed in Italy, and afterwards in Sicily. He then proceeded to Egypt for the British Indian Company, and was transferred in 1870 to the British Indian Extension Company, (now the Eastern Extension Australasia and China Telegraph Company), and served at various stations of the Company. At the time of his death he was superintendent at Penang.

He was elected a Member of the Institution on the 11th of April, 1877.

ROBERT STEWART HAMPSON was born on the 8th of August, 1860, and died on the 22nd of May, 1898.

Mr. Hampson first became associated with the electrical world under the late Mr. Moxon, then Electrical Engineer and Superintendent of the Lancashire and Yorkshire Railway, and later as assistant to the late Mr. E. C. Warburton, Mr. Moxon's successor. In the capacity of Mr. Warburton's assistant he was largely responsible for the installation of the electric light at the Fleetwood Docks. In 1887 Mr. Hampson

was appointed telegraph engineer to the Manchester, Sheffield, and Lincolnshire (now the Great Central) Railway, under the engineer of the line. Recently the appointment was created an independent one, with Mr. Hampson as its chief. The extension of the Great Central to London; its numerous ramifications; the adoption of electric lighting at the Company's Manchester, London, and other important stations on the route, afforded him a wide experience in all the modern appliances of electricity to railway working. He was an indefatigable officer, greatly respected by his colleagues, and beloved by all who knew him. In his early youth he was known as a good debater. In the discussion of political topics he was especially happy and effective, and was much sought after for platform work. Mr. Hampson's wife, daughter of the late Mr. Victor Shakery, a Liverpool merchant, and Vice-Consul for the Republic of Costa Rica, predeceased him by some eighteen months. The loss of his wife left a marked impression upon him. At Eastertide he joined his corps, the 4th Volunteer Battalion Manchester Regiment, in a three days' march. A serious illness supervened, terminating in death on the 22nd of May, 1898, at the early age of thirty-seven.

Mr. Hampson was elected a Member of this Institution on the 27th of February, 1896. W. L.

JOHN HOPKINSON, M.A., D.Sc., F.R.S., was born in Manchester in 1849, the eldest of five brothers, all of whom have been possessed of marked ability in science. He followed his early schoolmaster, Mr. Willmore, from Lindon Grove School to Queenwood College in Hampshire. He entered Owen's College in 1864, gained a scholarship in Trinity College, Cambridge in 1867, a Natural Science Exhibition and a Foundation Scholarship in Mathematics in 1868, became Doctor of Science of the University of London, both in mathematics and in mathematical physics, in 1870, was Senior Wrangler and first Smith's prizeman in 1871, and soon became a Fellow of Trinity. After some experience in his father's engineering works in Manchester he became engineer and manager of the lighthouse and optical department of the glass works of Messrs. Chance Bros., of Birmingham. His paper in the Proceedings of the Institution of Civil Engineers in 1883 will give some indication of his valuable work in connection with the design of optical apparatus and mechanism for lighthouses. He invented the very valuable and now much used "group-flash" system of working. He took an early interest in the use of the electric lighting of lighthouses, as is evidenced in a discussion at the Institution of Civil Engineers, vol. 57, 1877. Before this time (1876), and afterwards, he contributed greatly to our knowledge of residual charge in condensers (Phil. Trans., from 1876 to 1880, and indeed to 1897). His Birmingham experience gave him a thorough grasp of the true function of mathematics in engineering (see his James Forrest Lecture, Proc. Inst. C. E., vol. 108), and in 1878, when he came to London and was elected a Fellow of the Royal Society, he may be said to have possessed in perfection every quality characteristic of the good engineer.

From this time he followed the calling of an expert witness in patent cases, and he made it exceedingly lucrative. But, in spite of the engrossing nature of his professional work, he was able to do original experimental and mathematical work in pure and applied science, principally on the subject of electricity, which by itself was astonishing in amount. Thus he was awarded the Royal Society medal in 1890 for his many excellent contributions to natural knowledge, and even after he came to London he published nearly sixty valuable scientific papers. There was no theory of the dynamo till he gave us one in 1879 (Proc. Inst. Mech. Eng., 1879, 1880). He exhibited an alternate-current machine and an electric hoist (using for the first time the now common series-parallel system of working motors) at Paris in 1881. He invented the three-wire system of electric distribution in 1882. He began to improve the dynamo (see his lecture Inst. C.E., 1883) in 1883, and with his brother, Dr. E. Hopkinson, in 1886 (Phil. Trans., 1886) he published the principles which now guide all electrical engineers in designing electro-magnets. It is still in the memory of us all how the publication of this paper completely revolutionised the design of dynamo machines; and if Dr. Hopkinson had done nothing else in his life electrical engineers would have regarded him as one of their greatest benefactors. His most important papers on the running of alternators in parallel were published in the Institutions of Civil and Electrical Engineers in 1883 and 1884, but others were read later before the Royal Society. He became Honorary Professor of the Electrical Engineering Laboratory at King's College, London, in 1890, and from that time till his death carried out many investigations to verify and illustrate and extend his theories. The effect of Foucault currents on iron magnet cores; the best method of measurement of efficiency of transformers; the magnetic properties of iron and the effect of heat upon them—these are some of the subjects on which he published many valuable papers since 1890.

He acted as consulting engineer to the Corporation of Manchester from 1891, in carrying out their very important and successful system of electric lighting, introducing the then novel methods of charging customers for their supply which are now so common. The success of this work led to his being called upon to act in the same capacity in many other towns. He designed and superintended the construction of the Leeds Electric Tramway system, and was devoting much time to systems of electric traction for two years before his death. He was greatly interested in engineering teaching, and took a special interest in the Engineering School at Cambridge, to which he was an early large subscriber of money. He was a member of the Senate of the University of London, a member of Council of the Institution of Civil Engineers, of the Physical Society of London, and of the British Association. He was president of the Institution of Electrical Engineers in 1890, and again in 1896. Although ordinarily but little interested in political affairs, he was greatly moved by the events which took place towards the end of 1895, and became desirous of using his position as President of the Institution to further the cause of National defence. He conceived the idea that a technical corps of electrical

engineers might be used with advantage for the purpose of coast-defence, and at the very commencement of his second term of Presidency of the Institution he proposed that a corps of electrical volunteers should be formed with that object. He saw that all the members of such a corps would probably be experts in the electrical work that would be required of them, and that it would only be necessary that they should familiarise themselves with the special forms of apparatus used in the service. After considerable preliminary discussion the corps was organised with Dr. Hopkinson in command, and the first training took place in the autumn of 1897. During the following year, 100 electrical volunteers were enrolled, and Dr. Hopkinson was present at the commencement of the second training in the Isle of Wight at the commencement of 1898, and to his great satisfaction saw that his predictions as to the efficiency of the corps were fully verified.

At Cambridge he entered not only into the intellectual, but also into the physical life of the University. He was captain of the second Trinity boat, and won a college mile in very good time a few weeks before his Tripos. He was an enthusiastic and experienced Alpine climber. On the 27th of August, 1898, with his second son and two elder daughters, he was climbing a mountain well known to him, the Petite Dent de Veisivi, near the Rhone Valley, and all four were accidentally killed. The experience of the party made a guide unnecessary, and there was no witness of the accident. His death at forty-nine was premature, but in spite of the fact that his professional work occupied nearly the whole of his time, it is difficult to read the record of his achievements in pure and applied science without thinking that he must have devoted to them the whole of a very long life.

Dr. Hopkinson was elected a Member of the Institution on the 10th of November, 1881, and became a Member of Council in 1883, a Vice-President in 1886, and President for the first time in 1890.

J. P.

COLONEL ROBERT RAYNSFORD JACKSON was born in 1823, and at an early age commenced work at Blackburn in cotton mills, of which he ultimately became proprietor. Taking a keen interest in the subject of telephones, he was chairman of the National Telephone Company in 1881, and was vice-president of the company after its amalgamation with the Lancashire and Cheshire Telephone Exchange Company and the United Telephone Company. Until within a short time of his death, on the 28th of June, 1898, he retained a seat upon the directorate of the company.

He was elected an Associate on the 9th of March, 1882, was transferred to the class of Members on the 25th of February, 1886, and from 1890 to 1893 he was a Member of Council.

JOHN KRUESI, who died at Schenectady, on the 22nd of February, 1899, after an illness of four or five days, was born in 1843 at Speicher in Switzerland. Beginning his mechanical experience in machine shops in his own country, he proceeded to Paris at the age of twenty-four, and

to London three years later, whence, after a short stay, he continued his journey to America. He at once found employment at Elizabeth, New Jersey, with the Singer Sewing Machine Company, and in 1874 was engaged by Mr. Edison, with whom he remained until 1881. His indomitable energy, combined with his mechanical skill and readiness of resource, and with the absolute honesty of his character, led to his rapid preferment, and in the year last named he became general manager of the Electrical Tube Company, which was concerned with the installation of street electric light conduits; two years later he was appointed assistant general manager of the Edison Machine Works in New York, and both designed and erected the new works at Schenectady. In 1892, when the Edison and Thomson-Houston Companies united under the name of the General Electric Company, he became general manager, and, in 1896, chief mechanical engineer of the company.

He was elected a Foreign Member of this Institution on the 23rd of May, 1895.

WILLIAM THOMAS NEWITT was born in January, 1849. After having been in the service of the Electric and International Telegraph Company, he became connected with the Anglo-Mediterranean Telegraph Company in 1868. Finally he served in various capacities under the Eastern Extension Telegraph Company, and at the time of his death, which occurred whilst on furlough in England, on the 8th of April, 1898, he was general superintendent for that company at Madras.

Mr. Newitt was elected a Member on the 14th of February, 1877.

DR. EUGEN F. A. OBACH was born at Stuttgart in April, 1852, of Swiss parents, his father being an artist. He was educated at the Real and Polytechnic Schools of his native town, acted temporarily as lecturer on chemistry at the Veterinary College, and subsequently continued his studies in natural science at the University of Leipzig under Professors Kolbe, Wiedeman, Zöllner, Fechner and C. Neumann, where he graduated as Ph. D. in 1875. In the spring of 1875 Dr. Obach commenced his career with the firm of Siemens and Halske, at Berlin, as assistant to Dr. Werner Siemens, and towards the end of the following year he came to England and joined the staff of Messrs. Siemens Brothers, at their Woolwich works, as chief of the experimental department, which post he continued to hold up to the time of his death. Whilst temporarily staying in this country in 1875 he superintended some electrical experiments which were carried out in Ballinskelligs Bay, Ireland, during the completion of the Direct United States Telegraph Cable. He also accompanied the expedition in 1879 on board Messrs. Siemens Brothers' steamship *Faraday*, during the laying of the deep-sea portion of the second French Atlantic cable.

He published his first paper in 1875 on the action of the galvanic current on amalgams and alloys, and on the nature of amalgamation currents.

Dr. Obach also displayed considerable inventive ability. He designed a movable coil tangent galvanometer for measuring both current and potential, which is well known as an accurate laboratory instrument; also a dry cell, which has gained a wide reputation for constancy and high output, and is much used for telephone, telegraph, and other purposes. He further invented an apparatus for electrolytically decomposing water into its constituent gases, oxygen and hydrogen, on a practical scale, whereby the gases are obtained in a state of great purity. He also devised an improved form of conducting contact with carbon and other non-metallic electrodes, and a new process for the extraction of gutta-percha from the leaves and twigs of the Isonandra gutta and kindred plants.

Dr. Obach compiled complete tables, which are much used, particularly in Continental laboratories, for readily finding the ratio of the two sections of the slide wire of a Wheatstone bridge.

Other publications, which have appeared in various English and German scientific and technical journals, dealt with the following subjects:—

A battery for constant currents; a lecture apparatus for the demonstration of electric resistance in metallic wires; an electric pressure regulator; a commutator for liquid and gaseous currents; the diffusion of carbonic acid gas through caoutchouc; the action of manganese peroxide in batteries; a direct relationship between specific inductive capacity and latent heat of vaporisation; the action of phosphorescent light on selenium; the detection of free sulphur and selenium in carbon disulphide and their approximate quantitative determination, and the behaviour of carbon disulphide towards potassium permanganate, also a method for the complete purification of commercial carbon disulphide.

Dr. Obach delivered several lectures at the German Athenæum, London, the foremost being on the late Sir William Siemens as an inventor and discoverer; this lecture was subsequently published and favourably reviewed by the press. The other lectures dealt with the "Manufacture, Testing, and Laying of Telegraph Cables," and with the Electric Discharge in Rarefied Gases, and on the Röntgen Rays."

The chief work of Dr. Obach was undoubtedly the study of gutta-percha, which extended over many years, and the results of his great experience and varied knowledge of this substance were embodied in a course of Cantor Lectures, which he delivered before the Society of Arts towards the close of 1897. These lectures, which were fully illustrated by numerous experiments and by a unique collection of rare and valuable specimens, have since appeared in the columns of several technical journals, and have been published in separate form, and translated into German and Dutch. The subject was dealt with in a very exhaustive and comprehensive manner, and the information given is generally considered to be the most important yet published on gutta-percha.

Some time before the preparation of these lectures was commenced, Dr. Obach's health showed signs of failing, and it was mainly due to his indomitable energy and will that he was able to

bring them to a successful issue. By the advice of his physician he subsequently went abroad and sought relief in a cold-water cure at Brixen, in the Austrian Tyrol, but the course of his disease could not be checked, and he was finally removed to the house of his brother, Emil Obach, at Graz, Styria, where he died on December 27, 1898, at the comparatively early age of forty-six years.

He was elected a Member of this Institution on the 11th of April, 1877. A. S.

OLIVER BLACKBURN SHALLENBERGER was born at Rochester, Pa., U.S.A., on the 7th of May, 1860, his father, Dr. A. T. Shallenberger, being one of the leading physicians of Western Pennsylvania.

He was educated at the Public Schools at Rochester, and at Beaver College, in the neighbourhood of Beaver, Pa., U.S.A.

In 1877 he went up for an examination for a cadetship in the Naval Academy at Anapolis, and came out at the head of the list of 126 candidates. Throughout the first year he was the head of his class; and during the second and third year, in spite of a dislocated wrist and a broken arm, and his eyesight giving way (compelling him to abandon night work), he passed out third of his class.

Shallenberger found the department of physics at Anapolis peculiarly suited to his taste, and he acquired here a systematic training and a thorough knowledge of the fundamentary principles of physics, which stood him in good stead later in his life.

After completing a three years' course at the Naval Academy he was attached to the U.S. Flagship, *Lancaster*, where he spent the greater portion of his two years' cruise in the Mediterranean, and was present at the bombardment of Alexandria.

It is noticeable that among the contemporaries of Shallenberger at the Naval Academy were Mr. Frank J. Sprague, Dr. Lewis Duncan, Mr. F. W. C. Hasson, and Mr. Gilbert Wilkes, and several others whose names are prominent among the electrical engineers of the present day.

Shallenberger returned to the United States in 1883, and resigned his commission in 1884, devoting his entire attention to the science of electricity.

Mr. George Westinghouse was at that time organising an Electric Light Department for one of his companies, the Union Switch and Signal Company, of Pittsburgh, and Shallenberger became associated with him then. His genius and executive ability were at once recognised, and he very soon took a prominent part in the electrical work which was being done by that company.

On the Westinghouse Electric Company being formed, Mr. Shallenberger was appointed Chief Electrician, and has left his mark on the apparatus which is manufactured by that company and its successor, the Westinghouse Electric and Manufacturing Company, of Pittsburgh. In 1891 his failing health compelled him to resign his position as chief electrician, but the company, unwilling to dispense with his services, retained him as consulting electrician. His health gradually gave way,

which compelled him to spend the winters in Colorado, and during the evening of January 23, 1898, he passed away from amongst us.

During the fourteen years in which he was working at electricity he has made his mark on electrical industry, more particularly in connection with the development of the alternating current system.

Among his numerous contributions to electrical science may be mentioned the street lighting system in which lamps are placed in series on a high tension alternating current circuit, and each lamp is shunted by a reaction coil having its winding so proportioned that on any one of the lamps being extinguished a normal current flows through its coil, by reason of the magnetic saturation of its core, so that the remaining lamps are not affected.

Shallenberger also designed a very large number of instruments and accessories connected with alternating current, too numerous to mention. His diligence and grasp of a subject were most marked, and the comprehensive way in which he attacked a problem was a source of much wonder to those associated with him.

The invention which, perhaps, brought his name most prominently forward was the development of the alternating meter that bears his name. This meter suggested itself to him through a small accident which by most men would have been passed unnoticed. While testing an arc lamp which was burning with alternating currents, a small spiral spring happened to fall on the brass head of the coil, which had a core of iron wire. This spring began to revolve. Immediately he grasped the idea, and the applicability to a meter suggested itself to his trained mind. He realised the new phenomenon, and turned all his energies to the problem. Within a month he had completed his experiments, and the meter as we know it to-day was the result.

Mr. Shallenberger was elected a Foreign Member of the Institution on the 31st of May, 1888. R. B.

CHARLES H. SUMMERS, who died suddenly in San Francisco on the 2nd of November, 1898, at the age of 61, was born in Kentucky. He began his telegraph career as an operator on the Pittsburg, Cincinatti, and Louisville line, and five years later became associated with railways in Indianapolis, where he remained until 1867. From that year until 1869 he was telegraph superintendent of the Indianapolis, Cincinatti, and La Fayette Railway, and in the latter year was appointed electrician of the Central Division of the Western Union Telegraph Company, a position which he held up to the time of his death.

Mr. Summers was one of the oldest Foreign Members of the Institution, having been elected to that class on the 11th of December, 1872.

LIEUTENANT-COLONEL HENRY LAKE WELLS, R.E., C.I.E., who died at Karachi, after an illness of only six days' duration, on the 31st of August, 1898, was a son of the late Reverend T. B. Wells. He was born on the 8th of March, 1850, and received his commission in the Royal Engineers in 1871. During the next four years he served in England, qualifying as instructor in army signalling, and then, in 1875, proceeded to India. Here, after gaining valuable experience as an

executive engineer under the Public Works Department, he raised a corps of Ghilzais for the construction of the road across the Khojak, and saw active service in the Afghan campaign of 1878–1880. He was wounded in an engagement in the Khojak district whilst commanding a detachment of Punjab Cavalry and the Sind Horse, and was five times mentioned in despatches for services rendered during the campaign.

After having been employed on special duty with the Indian Telegraph Department in surveying and reporting upon routes for a telegraph line connecting Srinagar (Kashmir) and Gilgit, he was in 1880, with the rank of Captain, appointed an assistant director in the Government Indio-European Telegraph Department, and was transferred to Persia, where he remained until within a short time of his death. In 1885 he received the local rank of major and was appointed to officiate as director of the Persian section of the Telegraph Department. During his period of service in Persia he was granted a sword of honour by the Shah, and repeatedly received the special thanks of various Government departments for signal assistance rendered to them. The directors of the Indo-European Company passed a special vote of thanks to him "for admirable spirit and example set during the cholera epidemic of 1893," and in 1897 he was created a Companion of the Most Eminent Order of the Indian Empire.

Colonel Wells contributed several papers to the Royal Geographical Society, of which he became a member in 1880.

He was elected a Member of this Institution—at that time the Society of Telegraph Engineers—on the 10th of January, 1884.

GEORGE KIFT WINTER was born on the 7th of March, 1842, and died in Madras on the 17th of January, 1898. He was the second son of the late Mr. William Winter, a surgeon, residing at Henbury, near Bristol, and was educated at the Godolphin School in Hammersmith. The greater portion of his life was spent in India, where he served from 1864 to the time of his death, first as assistant, and later as Chief Telegraph Engineer of the Madras Railway. Prior to this he was in the employment of the Electric and International Telegraph Company, and during the later period of his service with them was associated with Mr. W. T. Ansell in the construction of their lines in Ireland.

Mr. Winter's professional ability and technical knowledge were combined in an unusual degree with the power of organisation and the faculty of inventiveness, as well as with great amiability of character, so that his department rapidly attained to a high degree of efficiency, and his assistants and subordinates were warmly attached to his person. At the same time his interests extended beyond the immediate sphere of his professional duties, as evidenced by his connection with the University of Madras and his association with the Government Astronomer of Madras in observations made during the solar eclipses of 1868 and 1871.

Among his patented inventions two (1873 and 1878) related to duplex or quadruplex telegraphy, one (1874) was the method, recently referred to by Mr. Langdon,[1] for enabling intercommunication to be effected

[1] *Journal Institution of Electrical Engineers*, 1898, vol. xxvii. p. 794.

in railway trains between passenger and guard, or between guard and driver, whilst one (1886) dealt with the regulation of the E.M.F. of a dynamo for lighting trains, and four (1878, 1880, 1885, and 1895) with electrical railway signalling apparatus.

Mr. Winter was a Fellow of the University of Madras, a Member of the Institution of Civil Engineers, and a Fellow of the Royal Astronomical Society, and communicated many scientific and technical papers to the publications of the learned societies. Among those communicated to this Institution may be recorded his paper[1] read on the 12th of March, 1873, and entitled *On Earth Currents, and on their Bearing upon the Measurement of the Resistance of Telegraph Wires in which they exist;* and the following, published in the form of original communications, *On Testing the Metal-Resistance of Telegraph Wires on Cables influenced by Earth-Currents.*[2] *On the use of Electro-Magnetic Induction in Cable Signalling;*[3] and (in collaboration with Mr. G. B. Winter) *On a Graphic Method of Studying the Behaviour of the Exposed Ends of Broken Telegraph Cables, and of Means of Eliminating the Effect of Earth-Currents, and of Polarisation at the Fault.*[4]

He was elected a Member of the Institution on the 11th of December, 1872.

[1] *Journal Society of Telegraph Engineers*, 1873, vol. ii. p. 89.
[2] *Ibid.*, 1872, vol. i. p. 260.
[3] *Ibid.*, 1874, vol. iii. p. 103.
[4] *Journal Institution of Electrical Engineers*, 1893, vol. xxii. p. 348.

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